1. **Statement of Purpose**
   1. The General Goal

I began my studies at Northwestern in 2019, motivated by adesire to pursue an ambitious research agenda: evaluating the efficacy of international human rights. During the Trump era, whilst the clouds of right-wing populism gathered over much of the Western world, it seemed natural for students of human rights such as myself to interrogate the wisdom and tenability of the movement. Had international human rights institutions—many of which were established following the Second World War, the last high-water mark of radical conservatism—stoked the very politics of grievance that they were designed to neutralize? And even if so, could the movement be bettered and buttressed against the salvos of the reactionaries—many of whom regarded it with contempt, an affront to national sovereignty and to rule of, by, and for the privileged *volk*?

Though not without flaws, the project to realize the lofty ideals propounded by human rights documents and institutions has, in my estimation and on balance, enhanced the human experience. At the very least, it has afforded the world’s oppressed peoples with a *lingua franca* with which to mobilize resistance and to encourage us all to treat others as we would ourselves, lest the horrors of yesteryear recrudesce. So it was that I expressed a desire to tackle such questions, hoping that my research would produce timely defenses against human rights’ detractors or urgent calls for their exponents to improve them—or both.

Five years on, the human rights *problématique* is very much unsettled. Indeed, many of the same forces that gave rise to human rights’ crisis of confidence in the Trump era persist largely unabated. Russia has launched a barbarous war of aggression in Ukraine, arrantly flouting human rights norms and laws with each passing day; Germany’s main nationalist party, *Alternative für Deutschland* (AfD), has enjoyed a stretch of unprecedented support levels, threatening to disrupt the country’s postbellum consensus for multilateralism and morals-based decision-making in international affairs; and Donald Trump himself not only remains the standard-bearer of the Republican Party, but also is its presidential candidate, his sundry and relentless attacks on both democracy and the rule of law notwithstanding. *Inter alia*, these examples underscore how the human rights movement remains under threat and why its present efficacy continues to be a worthwhile subject of inquiry. With my concerns over the future of human rights being unassuaged, for my dissertation, I aim follow the research agenda that I set forth at the outset of my tenure at Northwestern.

What has changed in the interim, however, are my career goals and methodological predilections. The first point is straightforward and merits little discussion herein. Put simply: I no longer desire a career in academia, at least in the near-term. Therefore, what I propose is a project tailored to the “non-academic” job market, a salient consideration insofar as it informs the work I wish to perform and the epistemological postures I ultimately adopt. Relatedly, I intend to avail myself of this opportunity to strengthen and showcase my quantitative skills, namely in data analytics and machine learning. Though I entered this PhD program with a background in the humanities, my methodological interests have drifted in a decidedly “hard” direction, such that I am now pursing Northwestern’s Ad-Hoc Master’s in Applied Statistics—with a sequence in Data Science to boot—and seeking jobs with a strong data-analytics component. As such, my preference for quantitative methods is to be regarded as an instrumental decision, one that will ideally signal my newfound competencies and hence bolster my professional prospects.[[1]](#footnote-1)

* 1. The General Plan

The dissertation I propose belongs to the “three-article” genre of such works. Each chapter aims to evaluate the efficacy of a particular facet of international human rights, with a thematic interest in the nexus of human rights and globalization. This also arises from instrumentalist thinking on my part in the sense that I would like the content of my dissertation to be germane to international trade and policy, two fields that I’m considering entering. Yet I’ve held a keen research interest in the topic for quite some time, beginning with the research proposal I submitted for my International Political Economy (IPE) course and culminating, in some ways, in a recent internship with the U.S. Trade and Development Agency.

The aforementioned “facets” of international human rights I endeavor to explore may be formulated as the following **puzzles**:

1. *How effective are human rights provisions in preferential trade agreements (PTAs)?*
2. *What are the human rights outcomes of bilateral investment treaties (BITs)?*
3. *How robust is human rights discourse to “foreign political investments” (FPIs)?*[[2]](#footnote-2)

As we shall see, the first two topics are similar. *Prima facie*, they both deal with laws regulating types of cross-border economic exchange and their human rights outcomes; yet they are perhaps even more linked by the literature they engage with and the methods they deploy. The third is perhaps the “unlikeliest” of the three, particularly inasmuch as it seeks to appraise neither laws nor particular pieces of scholarship. As I analyze each topic, however, I intend to utilize an array of methods so as to produce the best scholarship possible whilst demonstrating the breadth of my skills.

As to my plan for completing the dissertation, I propose that I follow a simple schedule of completing one chapter per quarter, so that my dissertation is finished and defended by the end of my sixth year. In this way, I may be positioned to enter the workforce in the summer or fall of 2025. I believe that this timeframe is wholly reasonable given my exceeding progress at this stage (particularly on chapters 1 & 2, as will become apparent) and that few impediments exist from the standpoint of data collection.

Having outlined the impetuses animating my agenda and my general plan for completing the project, I proceed to discuss each chapter in detail, adhering to the following structure:

1. A brief description of the puzzle under exploration.
2. An analysis of the literature at issue (where appropriate), which includes both (brief) summary and criticism.
3. A discussion of work already completed towards the chapter.
4. An enumeration of work I aim to complete and any outstanding questions I hope to address, whether independently or through committee-member feedback.

Moreover, in the interest of pithiness, I shall avoid collating and expanding on all my work completed heretofore. Rather, I aim to summarize what I’ve produced and to offer new insights, whether into my extant work or into areas where no such work yet exists. When the reader may be benefitted from a more complete explanation on my end, I will try to direct them to the relevant documents and/or passages.

1. **Chapter 1: PTAs**
   1. Puzzle: “*How effective are human rights provisions in PTAs?*”

Preferential Trade Agreements (PTAs) are bi- or multilateral treaties which aim to promote trade between signatory states, at a bare minimum through reduced tariffs.[[3]](#footnote-3) Many of these treaties, however, contain provisions constructed to bolster and protect human rights respect. Such provisions are classified as either “hard” or “soft,” where “[h]ard PTA [provisions] establish enforceable conditions for [economic] integration, while soft [provisions] appeal to voluntary principles of cooperation that do not require behavioral change to receive market access benefits” (Hafner-Burton, 2005, p. 594). In virtue of these provisions’ existence, interrogating the causal relationship between PTAs and human rights outcomes has promised to furnish scholars with at least one piece in the puzzle of international human rights law’s overall efficacy.

To date, two works have attempted to evaluate the efficacy of human rights provisions in PTAs using the methods of causal inference: Hafner-Burton (2005) and Spilker & Böhmelt (2013). Each deserves credit for attempting to put theory and sophisticated quantitative techniques to work to clarify the efficacy of human rights provisions in PTAs, as well as for producing a roadmap of sorts for future scholarship engaging these very questions. Nevertheless, both of these works are sufficiently flawed, from a methodological standpoint, so as to merit a rethinking of how the relationship between human rights-infused PTAs and human rights outcomes might be appraised.

* 1. Literature Review
     1. Summary
        + Hafner-Burton (2005)

The pith of her thesis is that hard PTAs (i.e., those containing hard provisions) *are* effective at improving respect for physical integrity rights among signatory states, whereas soft PTAs (i.e., those containing exclusively soft provisions) and cognate arrangements called HRAs[[4]](#footnote-4) are not; and that this is so on account of the sheer advantage of coercion—hard provisions’ mechanism of influence—over persuasion—soft provisions’ mechanism of influence.[[5]](#footnote-5)

From a theoretical and anecdotal standpoint alone, she avers, we can expect this hypothesis to be true.[[6]](#footnote-6) But more important for our purposes is her attempted substantiation through the leveraging of quantitative methods. Hafner-Burton constructs her model by first generating a novel variable, REPRESSION*it*. Derived from an amalgamation and recoding of Poe and Tate (1994) and Gibney’s (2005) respective datasets on governmental repression, REPRESSION*it* is a five-point, ordinal variable describing levels of repression in “176 states over twenty-six years, 1976 to 2001” (Hafner-Burton, 2005, p. 615), and it ultimately serves as her primary dependent variable of interest.[[7]](#footnote-7) She then incorporates a total of ten control variables.[[8]](#footnote-8) Finally, Hafner-Burton creates and includes her three main independent variables of interest: PTA*hardit*-1, PTA*softit*-1, and HRAS*it*-1. PTA*hardit*-1 and PTA*softit*-1 are dichotomous, indicating whether a given state was party to at least one hard or soft PTA, respectively, in the year prior.[[9]](#footnote-9) HRAS*it*-1, on the other hand, is ordinal on a 0 to 2 scale and signifies whether a given state was party in the year prior to neither, either, or both of two physical integrity rights-related HRAs: the International Covenant on Civil and Political Rights (ICCPR) and the Convention Against Torture (CAT).[[10]](#footnote-10)

Since her dependent variable, REPRESSION*it*, is ordinal, Hafner-Burton runs her model using ordered logit.[[11]](#footnote-11) What she finds comports with her theory: the coefficient on PTA*hardit*-1 is negative and statistically significant at the α = 0.05 level, while the coefficients on PTA*softit*-1 and HRAS*it*-1 are statistically insignificant at said level. Put in simpler terms, hard PTAs lead to decreased levels of physical repression, whereas soft PTAs and other HRAs have no effect on physical integrity rights respect. These results hold even under the stress of several robustness checks—namely, running the regression with two different dependent variables, IMPUTE*it* and CIVILLIBERTY*it*,[[12]](#footnote-12) and controlling for trade relations with the European Union (EU), time fixed effects, and the economic leverage of PTA partners, respectively.[[13]](#footnote-13) As such, and as Hafner-Burton would have it, PTAs with human rights provisions do appear to improve physical integrity rights respect and other human rights outcomes, but *only if* they possess mechanisms enforcing against human rights violations.

* + - * Spilker & Böhmelt (2013)

Gabriele Spilker and Tobias Böhmelt, on the other hand, begin by noting their skepticism of Hafner-Burton’s narrative. Indeed, in “The Impact of Preferential Trade Agreements on Governmental Repression Revisited” (2013), they expressly presuppose that hard PTAs are *unlikely* to have a positive effect on physical integrity rights respect. In their view, states—“being aware of the ‘shadow of the future’”—anticipate “what may happen at the succeeding enforcement stage,” and thus are exceedingly unlikely to accede to hard PTAs if they do not “have a general propensity to abide by human rights in the first place” (p. 357). This means that states electing to join hard PTAs will intrinsically and nonrandomly possess higher levels of physical integrity rights respect, a confounding factor virtually guaranteeing a significant difference in the dependent variable between states party to hard PTAs and those who are not. Put differently, the process whereby states are selected into the hard PTA treatment category—one which states themselves command—is the source of Hafner-Burton’s proposed causal effect, *not* the hard PTA treatment per se, rendering her inference invalid.

To test this hypothesis, Spilker and Böhmelt first reproduce the variables from Hafner-Burton’s 2005 article, availing themselves of the selfsame or updated versions of the datasets from which she drew.[[14]](#footnote-14) They then replicate her results for the sake of comparison, yielding findings that are effectively identical to hers.[[15]](#footnote-15) Most important, though, is how they proceed: by estimating the effect of hard PTA membership on physical integrity rights respect through genetic matching.[[16]](#footnote-16) Matching on TRADE, DEMOCRACY, and HUMAN RIGHTS RATIFICATION, specifically,[[17]](#footnote-17) Spilker and Böhmelt distill a well-balanced[[18]](#footnote-18) sample of their dataset (i.e., their collection of reproduced variables) with which they perform ordered logistic regression.[[19]](#footnote-19) Backed by a raft of robustness checks,[[20]](#footnote-20) their results ultimately lend credence to their theory: the significance of the coefficient on PTA HARD LAW disappears, suggesting that hard PTA membership does *not* affect physical integrity rights respect.[[21]](#footnote-21)

* + 1. Analysis
       - Hafner-Burton (2005)

In the last analysis, selection bias (i.e., confoundedness) undermines the empirical underpinnings of Hafner-Burton’s proposed effect. All regression-based causal inference rests on the assumption of unconfounded treatment assignment.[[22]](#footnote-22) Unconditional unconfoundedness, which stipulates that treatment assignment is inherently independent of the error term, is virtually impossible to prevail in a non-experimental context.[[23]](#footnote-23) Conditional unconfoundedness, on the other hand, which denotes when the inclusion of covariates establishes statistical independence between the treatment and the error term, is feasible when observational data such as Hafner-Burton’s are the objects of analysis.[[24]](#footnote-24) One means of meeting this assumption is by incorporating an instrument which, as per the researcher’s reasoning, randomly assigns the treatment in question.[[25]](#footnote-25) Hafner-Burton attempts to do just this by proffering, in her 2009 book, the number of intergovernmental organizations (IGOs) to which a given country belonged in the year prior (IGOs*it*-1) as an instrument for hard PTA membership.[[26]](#footnote-26) Yet this proposal is unlikely to withstand scrutiny: all instruments must be “correlated with the causal variable of interest […] but uncorrelated with any other determinants of the dependent variable,”[[27]](#footnote-27) though one may reasonably suspect that IGOs*it*-1 is indeed correlated with many of the covariates, including TRADE*it*-1, DEMOCRACY*it*-1, and *pc*GDPit-1.[[28]](#footnote-28)

Even so, owing to not only the panel nature of her data, but also the near certainty of differences in baseline levels of physical integrity rights respect across countries,[[29]](#footnote-29) Hafner-Burton could have made a viable case for conditional unconfoundedness by controlling for unit fixed effects in addition to time fixed effects. Indeed, doing so would have rendered the treatment variable independent from the presumably sundry, and occasionally difficult to operationalize, country-specific factors which help determine treatment assignment but are otherwise subsumed under the error term (e.g., differences in “the shadow of the future”). In both of her works, however, she fails to attend to unit fixed effects entirely. As such, her models fail to fulfill the conditional unconfoundedness assumption, and selection bias continues to afflict them.

* + - * Spilker & Böhmelt (2013)

Like Hafner-Burton, Spilker and Böhmelt fail to adequately tackle selection bias themselves. With respect to probing panel data, fixed effects are almost invariably necessary on account of the high likelihood that there exist variables conditioning treatment assignment which depend on differences across units or time. Indeed, the only scenario wherein “pooling”[[30]](#footnote-30) is admissible is when baselines (i.e., intercepts) in the dependent variable are effectively the same across units and time. Since countries’ baseline levels of physical integrity rights respect are almost certain to vary, pooling is axiomatically not an appropriate method of analysis in Spilker and Böhmelt’s case. The authors, however, pay no heed to this insight, as demonstrated by their matching cases and conducting ordered logit *irrespective of fixed differences*.[[31]](#footnote-31) Consequently, their model does little to extricate the selection bias which motivated their study in the first place.

* 1. Completed Work
  2. Remaining Questions

1. **Chapter 2: BITs**
   1. Puzzle: “*What are the human rights outcomes of BITs?*”

Bilateral investment treaties (BITs), which aim to promote foreign direct investment (FDI) by expressly conferring rights—including national and most-favored-nation treatment—to international investors,[[32]](#footnote-32) are another class of international economic agreements that have been evaluated for their impact on human rights. Unlike PTAs, BITs do not typically endeavor to uphold human rights standards directly, with “very few, if any […] mention[ing] […] human rights” (Bodea & Ye, 2018, p. 955). Yet many observers have followed the proliferation of BITs with apprehension, viewing the treaties as centering the interests of distant and powerful investors at the expense of domestic stakeholders in developing states—a recipe for human rights abuses on the part of leaders seeking to enforce them. Perhaps the most significant work to date to assess the human rights outcomes of BITs is Bodea and Ye’s “Investor Rights versus Human Rights: Do Bilateral Investment Treaties Tilt the Scale?” (2018), the results of which seem to confirm their hypothesis: BITs worsen human rights respect in developing states,[[33]](#footnote-33) particularly in non-democracies.

* 1. Literature Review
     1. Summary

Bodea and Ye’s hypothesis is predicated on a pair of assumptions as regards BITs’ proximate negative externalities. First, they argue, BITs “[lock] in [the] investor-friendly policies” that developing countries adopt pre-ratification in their regulatory “race to the bottom” to attract FDI, including “low taxes and lax labor standards, or reduc[ed] welfare spending” (2018, pp. 960-61). Second, “BITs constrain government’s choices for sustainable development and welfare improvement” by inhibiting or outright precluding investor-unfriendly policies that states might entertain to achieve these ends, such as increased spending on social benefits and infrastructure, or even expropriation (2018, p. 961). Being more hamstrung in their ability to deliver material gains to their people, these states increasingly face the prospect of mass protests—and become increasingly likely to resort to repression as a solution thereto, given that BITs raise the costs to “address the root causes of popular grievance” (2018, p 963). So it is that BITs are thought to give way to human rights abuses in developing states, especially in non-democracies, where leaders enjoy a low probability of facing accountability for their transgressions.

Bodea and Ye assess their hypothesis with models explicitly inspired by Hafner-Burton (2005) and Spilker & Böhmelt (2013).[[34]](#footnote-34) Their dependent variable is CIRI’s nine-point ordinal “measure of government respect for physical integrity rights” (Bodea & Ye, 2018, p. 964). Furthermore, motivated by a belief that “[t]he more BITs a host state ratifies, the greater the potential for popular grievance and repressive government tactics,” they use as their principal treatment variable a count of the number of BITS to which a country-year was party (2018, p. 965). To appraise the effect of regime type on the relationship between BITs and human rights respect, they also include two additional treatments: (1) POLITY2 (from Polity IV) and (2) an interaction term between POLITY2 and BITS (BITS \* POLITY2).

As to their controls, Bodea and Ye opt for a familiar assortment. Indeed, it mirrors Spilker and Böhmelt’s (2013), which itself derives from Hafner-Burton’s (2005)—featuring HARD PTA, SOFT PTA, and HUMAN RIGHTS RATIFICATION, *inter alia*—whilst including dummy measures of the presence of INTERSTATE WAR and CIVIL WAR, as well as POLITICAL DISSENT, a “[count] of antigovernment protest, riots and general strikes” (Bodea & Ye, 2018, p. 966). Among the controls are also (net) FDI INFLOW, measured as a percentage of GDP, and TRADE OPENNESS, “the [logged] sum of a state’s total exports and imports as a share of [GDP]” (Bodea & Ye, 2018, p. 965).[[35]](#footnote-35) They execute this model using ordinary least squares (OLS) regression, though they also run variations of said model with two instruments that attempt to capture competitive pressures potentially prompting BIT accession: (1) “[an] average of the total ratified BITs in neighboring [country-years],” and (2) “[a] three-year lagged total of new BITs ratified by other countries” (2018, p. 968). In all instances, as alluded to previously, their findings comport with their priors: the coefficients on BITS are negative and statistically significant, suggesting that belonging to more BITs does decrease human rights respect, while the coefficients on the interaction term BITS \* POLITY2 is positive and statistically significant, implying that the negative relationship between BIT membership and human rights respect is, at the very least, likely nonexistent in democracies.

* + 1. Analysis

Bodea and Ye’s choice of a spatially-determined instrument (see instrument 1, above) is intriguing, one that suggests new avenues for acquiring and demonstrating quantitative skills—a core goal of my dissertation. I will return to this point later; but for now, I shall discuss the missteps that I see as undermining the integrity of Bodea and Ye’s work. Of course, we have already seen how HARD PTA and SOFT PTA are likely crude measures of the concepts they seek to operationalize. What is more concerning in the context of this piece, however, is that they—along with HUMAN RIGHTS RATIFICATION—are used as controls at all. As we know, it is generally understood that, if a researcher is to manually include a covariate in a regression model, then they should have reason to suspect that it is a confounder—that is, a variable possibly affecting realized values in the treatment and outcome, simultaneously yet independently.[[36]](#footnote-36) Though the potential relationship between PTA membership and BIT membership seems intuitive—states keen to increase trade with international partners through PTAs surely hold similar attitudes with respect to encouraging FDI via BITs—it is not immediately clear what the connection between *human rights commitments* and BITs might be. This is especially so given that they at no point defend their inclusion of HARD PTA, SOFT PTA, and HUMAN RIGHTS RATIFICATION.

Bodea and Ye do, however, explicitly agree with Hafner-Burton in that, with respect to international agreements, only “hard” provisions can reliably condition state behavior.[[37]](#footnote-37) Perhaps they included these variables, then, to test for the possibility that human rights commitments—particularly enforceable ones, such as those found in hard PTAs—constrain states’ ability to utilize repression when BIT-related popular dissatisfaction materializes, thereby reducing some states’ likelihood of acceding to BITs in the first place. Nevertheless, the use of these covariates appears flawed, largely for reasons we have already seen. From Spilker and Böhmelt (2013), we have reason to believe that states ratify hard PTAs generally when they expect to comply with their provisions—to wit, when states already tend to respect human rights. Thus, HARD PTA—and, to a lesser extent, SOFT PTA and HUMAN RIGHTS RATIFICATION—might well possess a reverse-causal relationship with the dependent variable, CIRI. What’s more, as I have shown, HARD PTA and SOFT PTA are in all likelihood overly crude and—perforce—inaccurate measures of the concepts they purport to capture. Finally, HARD PTA and SOFT PTA are problematic in view of Bodea and Ye’s instruments. Indeed, as we have already discussed, instruments are valid when they correlate *exclusively* with the treatment; however, it seems probable that the competitive pressures influencing states to join BITs likewise motivate PTA membership, a concept at least partially captured by HARD PTA and SOFT PTA. *In toto*, these considerations cast doubt on the wisdom of including human-rights-commitment variables in the manner that Bodea and Ye adopt.

A significant consequence of my final observation above—that there may exist correlations between at least some of the controls and the instruments—intimates that the instruments themselves lack steady footing. In fact, one may reasonably suspect that each of the instruments are correlated with FDI INFLOW and TRADE OPENNESS, specifically: a country’s openness to international investment and trade may motivate competitor states (whether neighbors or not) to match or surpass their openness, perhaps by acceding to increasing numbers of BITs, and vice versa. If so, the extent of correlation between the controls and instruments would seem far-reaching, rendering the use of instruments especially unsuitable for their model.

Another key flaw in Bodea and Ye’s model that is perhaps unsurprising by now is their dependent variable, for CIRI is undercut by the same instrumentation bias as that found in the dependent variables of Hafner-Burton (2005) and Spilker & Böhmelt (2013).[[38]](#footnote-38) A problem requiring more in the way of elaboration, however, is that of Bodea and Ye’s main treatment, BITS. As aforementioned, BITS is a count of country-year BIT membership, and its use derives from the hypothesis that belonging to more BITs increases the likelihood of protests and hence repressive acts on the part of the state. Yet it is not apparent, in my estimation, why we should expect this claim to be true. By way of example, consider the following. Developing country *X* belongs to ten BITs. *X*’s ten co-signatories, however, are generally small or poor; it is thus likely that their cumulative investments in *X* may only marginally undermine the interests of *X*’s domestic stakeholders. On the other hand, developing country *Y* belongs exclusively to a BIT with the United States, which by contrast is an exceedingly large and wealthy country. The magnitude and depth of investments being made by American entities in *Y* is therefore far more likely to unsettle domestic stakeholders. In such a scenario, we can reasonably expect to see a greater probability of repression in *Y* than in *X*, even though the former belongs to far fewer BITs than the latter. As such, there is sufficient reason to believe that BITS misses variation *between* BITs with respect to the investment flows they facilitate—a concept which, if well-operationalized, might be conducive towards more robust findings.

* 1. Completed Work

Bodea & Ye’s use of instruments does not appear justifiable, as aforementioned; yet this is not to suggest the same about spatial modeling more generally. Recall that the first of these instruments is the mean number of BITs ratified by each country-year’s neighbors, with a “neighbor” being defined as a country “shar[ing] a land border or […] separated by 12 miles of water or less” (Bodea & Ye, 2018, p. 968). Accepting the presupposition that states are in competition with one another over a scarcity of economic gains—and that this may be especially so among neighbors, who may share similar resources, political or business cultures, and more—we may consider such a spatial variable to be a useful control, at the very least. Additionally, emphasizing spatial data analysis in this chapter may advance my overarching agenda of demonstrating my methodological virtuosity. To this end, we may also consider the use of spatial techniques for variables besides the cumulative BITs counts (i.e., the treatments), including the dependent variable and the controls.

In my final project for Stat-302 (Data Visualization), I began laying the groundwork for the use of spatial data analysis in this chapter. In particular, I set about computing spatially-weighted scores for a number of *ad rem* variables, including Bodea & Ye’s treatments, and determining the intensity of spatial clustering therein, namely by computing the statistic “Global Moran’s *I*” over time. In brief, my work evinced spatial clustering in the cumulative BITs counts (treatments) and HR Scores (my intended outcome), but not in the POLITICAL DISSENT variable. These results held irrespective of whether one adopted a strict or lax rule for neighborhood membership (12 miles or 200 kilometers, respectively). Therefore, it would seem appropriate to at least strongly consider these weighted scores’ inclusion as controls. Given my observation of clustering in the treatments and outcomes, the following models may at least be considered as candidates for my work:

1. Spatial Autoregressive Model (SAR): includes as a control a spatially-lagged version of the outcome,
2. Spatially-lagged X Model (SLX): includes as a control a spatially-lagged version of the right-hand side variables,
3. Spatial Error Model (SEM): includes as a control a spatially-lagged version of the error terms,
4. Spatial Durbin Model (SDM): combines the features of SAR and SLX,
5. Spatial Durbin Model (SDEM): combines the features of SLX and SEM.

In the process of completing this project, I also analyzed Bodea & Ye’s replication files, running them in Stata, with which the code for the models was exclusively compatible. I was surprised to find a yawning gap between the number of observations in the dataset and that which remained after running the first, main model: 7261 and 2679, respectively. Bodea & Ye do not discuss means of imputing missing data in their article, and it would appear, in my estimation, that no attempt at doing so was ever performed. This raises further doubts about the robustness of their findings, especially if nonrandom processes may explain how the missing observations failed to participate in the final regression.

* 1. Remaining Work and Questions

Although significant progress has been made towards this chapter—namely, the preliminary analysis of spatial clustering and review of the replication data—a good deal of work obviously remains. The primary next steps I seek to accomplish include:

1. Gathering data for newer country-year observations, since Bodea & Ye’s dataset ends in 2010,
2. Finishing to determine which variables might justifiably be spatially lagged,
3. Selecting which spatial models to use,
4. Selecting which treatments and what covariates to use,
5. Selecting which regression model(s) to use (e.g., OLS, lasso, etc.),
6. Selecting an imputation method.

The first task is the easiest, since Bodea & Ye provide their sources for their variables. In fact, I’ve already commenced this work by downloading and analyzing the most recent observations for the POLITICAL DISSENT variable.[[39]](#footnote-39) The second also seems generally simple insofar as I may, at a minimum, continue my work computing Global Moran’s *I* for the remaining covariates. However, I do have questions involving *Local* Moran’s *I*, namely whether it may also be leveraged as a means of locating spatial clustering for the purposes of my project. This, along with identifying the most appropriate spatial model(s), is something which consultations with my committee members may especially benefit.

In discussing the shortcomings of Bodea & Ye’s model, above, I expressed how a better *treatment*—one that accounts for the relative importance of individual BITs, rather than a simple cumulative count—may be helpful. Formulating such a treatment (at the very least to supplement the originals) should be simple; indeed, one approach may be to sum, for every country-year, the (logged) GDP of every country belonging to said country-year’s portfolio of BITs. In my Stat-302 final project, I also discuss the problem of instrumentation bias in the POLITICAL DISSENT variable. In finalizing my right-hand side variables, then, it might be worth seeking out a more reliable indicator of anti-government unrest, perhaps with the V-Dem dataset as a starting point. The V-Dem dataset may be of further use as a source of additional covariates, particularly should I opt for a model (whether as a baseline or as a robustness check) that lends itself to a more “kitchen-sink” approach to variable inclusion, such as a lasso regression. The final hurdle I will need to clear—imputing missing values—is one wherein I’d appreciate input. , given computational constraints.

1. **Chapter 3: FPIs**
   1. Puzzle: “*How robust is human rights discourse to FPIs?*”

Perhaps the most remarkable—if divisive—development in the recent history of sports has been the emergence of the Middle East as a locus of influence. In 2022, Qatar, a small nation lacking a record of sporting success or existing requisite infrastructure, hosted the FIFA Men’s World Cup, an honor conferred not only under a cloud of bribery accusations,[[40]](#footnote-40) but also notwithstanding the country’s well-documented mistreatment of migrant workers, women, the LGBTQ+ community, and political dissidents.[[41]](#footnote-41) A year prior, and in the wake of the brutal assassination of U.S.-based journalist Jamal Khashoggi, Saudi Arabia’s Public Investment Fund (PIF) completed its takeover of Premier League outfit Newcastle United—a move met with opprobrium on the part of rival clubs and human rights monitors yet near-universal support by Newcastle’s fans.[[42]](#footnote-42) More recently, the Saudi Pro League, under the aegis of the PIF,[[43]](#footnote-43) has waged an unprecedented campaign to attract the world’s best talent. In 2023 alone, Saudi teams signed Ballon d’Or winners Cristiano Ronaldo and Karim Benzema (and other superstars such as Neymar and Sadio Mané) to contracts befitting Croesus.[[44]](#footnote-44) Most alarming to officials on the western shores of the Atlantic, however has been Saudi Arabia’s foray into the world of golf. In 2021, the PIF formed LIV Golf as a competitor to the Florida-based PGA Tour, luring away such marquee athletes as Phil Mickelson and Dustin Johnson. Though their early relationship was unflaggingly litigious and acrimonious—PGA commissioner Jay Monahan even invoked Saudi Arabia’s putative involvement in the events of 11 September to cast aspersions on his organization’s upstart adversary—the two leagues abruptly agreed in June 2023 to a merger. The surprising truce prompted immediate and ongoing scrutiny from lawmakers, who have not only convinced the Justice Department to launch a probe into the matter on antitrust grounds, but also vociferously condemned its optics: a storied American institution willingly entwining its destiny with an autocratic regime infamous for its insouciance towards human rights norms.[[45]](#footnote-45)

In these and other instances,[[46]](#footnote-46) detractors have leveled against Middle Eastern states the charge of “sportswashing”—“the practice,” according to Cambridge Dictionary, “of an organization, a government, a country, etc. supporting sports or organizing sports events as a way to improve its reputation.”[[47]](#footnote-47) Spokespeople for the criticized have downplayed the sportswashing accusation, ascribing their governments’ unprecedented investments in athletics to the more anodyne motives of tourism-promotion or peace-building.[[48]](#footnote-48) Nevertheless, there is widespread agreement among academics,[[49]](#footnote-49) human rights monitors,[[50]](#footnote-50) and the press[[51]](#footnote-51) that countries such as Qatar and Saudi Arabia are indeed gaining purchase in the international sporting landscape for political advantage. Extant scholarship has described the sportswashing phenomenon[[52]](#footnote-52) and evaluated the human rights impacts of sports “megaevents” held in states pursuing sportswashing,[[53]](#footnote-53) but there has yet to be a concerted attempt to assess whether sportswashing succeeds in burnishing states’ reputations. Likewise, though there exists scholarship , including Bodea & Ye (2018), . And, in any case, these works almost universally analyze d—not the other way around.

1. That is, my preference is not to be regarded as a product of a belief in the superiority of quantitative methods, per se. [↑](#footnote-ref-1)
2. Henceforth, I will refer to “preferential trade agreements,” “bilateral investment treaties,” and “foreign political investments” by their acronyms. [↑](#footnote-ref-2)
3. See Congressional Budget Office (2016, pp. 1-2). [↑](#footnote-ref-3)
4. HRAs, or “human rights agreements,” are a broad category of treaties designed to “[encourage] repressors” to better their human rights respect without the use of mechanisms enforcing against defection (Hafner-Burton, 2005, pp. 593-4). [↑](#footnote-ref-4)
5. For more on the logic underpinning each mechanism, see Footnote 4 and Hafner-Burton (2005, pp. 599-600). [↑](#footnote-ref-5)
6. For her theoretical reasons, of which there are eight, see Hafner-Burton (2005, pp. 600-2). For her anecdotes, see Hafner-Burton (2005, pp. 609-14). [↑](#footnote-ref-6)
7. The specific acts of repression accounted for in the novel variable include “murder, torture, or other cruel, inhuman, or degrading treatment or punishment; prolonged detention without charges; disappearance or clandestine detention; and other flagrant violations of the right to life, liberty, and the security of the person” (Hafner-Burton, 2005, p. 615). For more on the recoding process, which effectively involved converting the raw data into indicators, see Hafner-Burton (2005, p. 615). [↑](#footnote-ref-7)
8. The ten variables are REPRESSION(1)*it*-1, REPRESSION(2)*it*-1, REPRESSION(3)*it*-1, REPRESSION(4)*it*-1, DEMOCRACY*it*-1, DURABILITY*it*-1, DENSITY*it*-1, TRADE*it*-1, INVESTMENT*it*-1, and *pc*GDP*it*-1. For more on these variables, see Hafner-Burton (2005, pp. 615-7). [↑](#footnote-ref-8)
9. See Hafner-Burton (2005, p. 618). [↑](#footnote-ref-9)
10. See Hafner-Burton (2005, pp. 617-8). [↑](#footnote-ref-10)
11. This is a form of logistic regression tailored to ordinal dependent variables, specifically. For more on ordered logit, see Woolridge (2016, p. 612). [↑](#footnote-ref-11)
12. The former is a version of REPRESSION*it* which imputes missingness in its underlying data, and the latter captures “repression of civil rights collected annually by Freedom House” (Hafner-Burton, 2005, p. 621). Interestingly, Hafner-Burton neglects to note which method of imputation she ultimately deploys to generate IMPUTE*it*. [↑](#footnote-ref-12)
13. For more on the logic behind these three controls, see Hafner-Burton (2005, pp. 621-3). [↑](#footnote-ref-13)
14. Indeed, many of these datasets were added to in the seven-year interval separating the two articles’ publications. For more on the variable reproduction process, see Spilker and Böhmelt (2013, pp. 350-3). [↑](#footnote-ref-14)
15. See Spilker and Böhmelt (2013, pp. 353-4). [↑](#footnote-ref-15)
16. “Matching” is a family of methods commonly used by social scientists to overcome nonrandom treatment assignment (i.e., selection bias) suspected to be at work in observational data. More specifically, all forms of matching endeavor to rein in selection effects and enable *ceteris paribus* comparison through a conceptually similar process: by pairing cases from a dataset as per their covariate similarity, the only significant difference being their treatment assignment status. Genetic matching is a particular member of this family, and it offers a significant advancement over traditional (and now-discredited) propensity score matching in that it “uses a search algorithm to iteratively check and improve covariate balance,” obviating the need for manual propensity score checks and adjustments (Diamond and Sekhon, 2013, p. 932). For more on the general logic and goals of matching, see Diamond and Sekhon (2013, p. 932) and Morgan and Winship (2014, pp. 140-3). For more on the shortcomings of propensity score matching, see King and Nielsen (2019). For more on genetic matching’s pairing process, whose central metric is “a generalized version of Mahalanobis distance,” see Diamond and Sekhon (2013, p. 934). [↑](#footnote-ref-16)
17. These are the equivalents of TRADE*it*-1, DEMOCRACY*it*-1, and HRAS*it*-1, respectively. The reasons why they match on these variables, exclusively, are twofold: doing so not only effectuates “the highest balance between treated and control observations [vis-à-vis] any other combination of variables,” but also is judicious theoretically since “the pool of potential control units is not much larger than the pool of treated units” (Spilker and Böhmelt, 2013, p. 355). [↑](#footnote-ref-17)
18. Perfect covariate balance “mean[s] that the treatment and control groups have the same joint distribution of observed covariates” (Diamond and Sekhon, 2013, p. 932). The better is the covariate balance, the more robust is the causal inference. [↑](#footnote-ref-18)
19. For their balance statistics, see Spilker and Böhmelt (2013, p. 356). For their interpretation thereof, see Spilker and Böhmelt (2013, p. 355). [↑](#footnote-ref-19)
20. To review the robustness checks, download the online appendix as per the instructions in Spilker and Böhmelt (2013, p. 356). [↑](#footnote-ref-20)
21. PTA HARD LAW is the equivalent of PTA*hardit*-1. The dependent variable is unnamed; but as alluded to previously, it is constructed so as to resemble REPRESSION*it*. [↑](#footnote-ref-21)
22. See Imbens & Rubin (2015, p. 257). [↑](#footnote-ref-22)
23. Put differently, all units in a population have the same probability of being assigned the treatment in question, since both the sampling process and the assignment mechanism have been randomized, and since the sample is sufficiently large so as to enable *de facto* covariate balance between the treatment and control groups. Controlled experiments are effectively the only means whereby perfect randomization can be achieved. See in particular Imbens & Rubin (2015, pp. 257-8). [↑](#footnote-ref-23)
24. In other words, the treatment is deemed “as if” random by controlling for the covariates which condition treatment assignment. See in particular Imbens & Rubin (2015, pp. 265-6). This is also referred to as the “conditional independence assumption” (Ibmens & Rubin, 2015, p. 43). Of course, conditional unconfoundedness is never fully possible, for the litany of covariates conditioning treatment assignment in an uncontrolled setting is virtually infinite. [↑](#footnote-ref-24)
25. See Imbens & Rubin (2015, p. 513). [↑](#footnote-ref-25)
26. For more on this instrumental variable, see my Appendix. [↑](#footnote-ref-26)
27. Angrist & Pischke (2009, p. 116). [↑](#footnote-ref-27)
28. To elaborate, it is likely that countries which trade more belong to more organizations promoting and protecting their trade, that countries which are more democratic belong to more power-sharing organizations, and that countries which are richer belong to more trade-regulating organizations. [↑](#footnote-ref-28)
29. That different countries have different such baselines is effectively a given (consider a comparison of Sweden and North Korea, for instance). [↑](#footnote-ref-29)
30. That is, treating all observations in a panel dataset as independent, thus ignoring the potential for fixed differences. [↑](#footnote-ref-30)
31. Their robustness checks also lack fixed effects. See their online appendix. [↑](#footnote-ref-31)
32. <https://www.law.cornell.edu/wex/bilateral_investment_treaty> [↑](#footnote-ref-32)
33. Their universe of cases is indeed limited to “developing countries.” See Bodea & Ye (2018, p 964). [↑](#footnote-ref-33)
34. Indeed, see Bodea & Ye (2018, p. 965). [↑](#footnote-ref-34)
35. For more, see Bodea & Ye (2018, pp. 965-67). [↑](#footnote-ref-35)
36. CITE [↑](#footnote-ref-36)
37. See Bodea & Ye (2018, p. 959). [↑](#footnote-ref-37)
38. Indeed, Fariss (2013) gives the CIRI index as a paradigmatic example. See Fariss (2013, p. ). [↑](#footnote-ref-38)
39. See my final project, section… [↑](#footnote-ref-39)
40. CITE [↑](#footnote-ref-40)
41. CITE [↑](#footnote-ref-41)
42. 97% support. <https://www.bbc.com/sport/football/53662771>. Exceptions include… [↑](#footnote-ref-42)
43. The 4 best teams 75% owned by the PIF; CITE [↑](#footnote-ref-43)
44. CITE CONTRACTS [↑](#footnote-ref-44)
45. See <https://www.cnbc.com/2023/06/15/pga-tour-liv-golf-merger-justice-department-to-investigate.html> <https://www.blumenthal.senate.gov/newsroom/press/release/blumenthal-opens-probe-into-pga-tour-and-liv-golf-agreement> [↑](#footnote-ref-45)
46. Cases I didn’t discuss include Manchester City and Paris St.-Germain [↑](#footnote-ref-46)
47. <https://dictionary.cambridge.org/us/dictionary/english/sportswashing> [↑](#footnote-ref-47)
48. See, for instance, Saudi Prince Abdulaziz’s defense in *60 Minutes*. <https://www.cbsnews.com/news/saudi-arabia-sportswashing-accusations-60-minutes-transcript-2023-04-09/> [↑](#footnote-ref-48)
49. CITE [↑](#footnote-ref-49)
50. CITE [↑](#footnote-ref-50)
51. CITE [↑](#footnote-ref-51)
52. See, for instance, Skey (2022). [↑](#footnote-ref-52)
53. See, for instance, Scharpf et al. (2023). [↑](#footnote-ref-53)