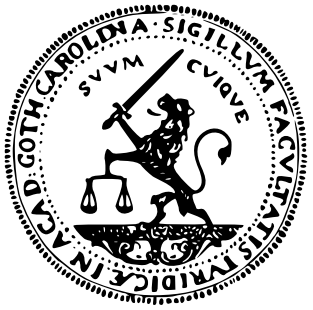

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The Effect of Regime Change on Human Rights Treaty Effectiveness

A Quantitative Study

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Abstract

International law is well known to lack enforcement mechanisms similar in effectiveness to those that exist on the domestic level. Many scholars have tried to explain how international law can still constrain state behavior. I look at one such theory, and attempt to formulate hypotheses for how states should be expected to behave around and after regime changes with respect to their human rights obligations. These are then tested quantitatively using publicly available datasets. I find that there is no statistically significant difference between the human rights trend in these circumstances for ratifiers and nonratifiers of the International Covenant on Civil and Political Rights and its Optional Protocol.

Sammanfattning

Folkrätten saknar, som allmänt känt, den typen av starka mekanismer för att se till att lagen efterlevs som existerar i den inhemska rätten. Många rättsvetare och andra forskare har försökt förklara hur det är möjligt för folkrätten att ändå påverka staters beteende. Jag undersöker en sådan teori och försöker formulera hypoteser för hur stater kan förväntas agera vid och efter regimskiften avseende deras skyldigheter på mänskliga rättigheter-området. Hypoteserna testas sedan kvantitativt med hjälp av allmänt tillgängliga databaser. Jag finner ingen statistiskt signifikant skillnad i trenden för mänskliga rättigheter under de omständigheterna för de stater som ratifierat och de som inte ratifierat FN:s konvention om medborgerliga och politiska rättigheter och dess första tilläggsprotokoll.

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1. Introduction

1.1. Motivation

In international law, unlike domestic law, there is no outside enforcer that can maintain respect for agreements. Still, international law is often complied with. Why states comply with their obligations more often than chance is an interesting topic in itself and theories have been constructed to explain it, but in order to be useful for saying something about the future behavior of states they also have to explain under what circumstances states will choose to comply and under which they will not. Constructing such a theory is a daunting task because there are so many factors that can affect the decisions of a state. However, one has to start somewhere and a theory that is right more often than chance is better than no theory at all. The desire to further knowledge about the factors affecting state behavior is what motivates this thesis.

1.2. Aim

The aim of this thesis is to study the effect that regime changes have on how states approach their international obligations. I build on the work of Professor Andrew Guzman of the USC Gould School of law and try to see what implications his theory has in the context of regime changes. The goal is to produce testable hypotheses in line with the theory and test them quantitatively. I also draw inspiration from other quantitative studies in the human rights area and see how their methodology can be applied in my own work.

1.3. Delimitations

I will look at compliance with two treaties, namely the International Covenant on Civil and Political Rights and its Optional Protocol. The theory developed in this thesis is general for all of human rights law and perhaps even international law as a whole but a delimitation has to be made to fit the scope of a master's thesis. Moreover, while regime changes can take a state in a more or less democratic direction, I will only look at those bringing about the largest change towards democracy or towards autocracy. This is because the theory that will be developed relies on there being a large break with the previous regime.

1.4. Disposition

In the next section, I will give an overview of the field of compliance theory with its different schools of thoughts and what problem it is trying to solve. I will go into detail about one theory of compliance developed by Andrew Guzman which serves as a basis for the hypotheses that I develop later on. Earlier quantitative studies like this one are also presented in a short literature review at the end of the section.

After the background section, I develop my own hypotheses. It is suggested that states will have something of a "fresh start" when a new regime comes to power after a large

regime change. This will result in states that have ratified treaties being able to improve their reputation more cheaply and thus seeing a rise in their compliance at first, followed by a gradual decrease as reputational gains become more expensive for reasons that will be explained in the chapter.

The method section contains descriptions of my data sources, how my program processes the sources and how the statistics for checking the validity of the results works. There is also a discussion about how well my data sources measure what I want to measure. The source code of my program can be found in the appendix and is also hosted online.

In the results section, I present the output of the program on the form of bar plots, histograms and tables together with descriptions of what they mean. Following that, I interpret the results in greater detail in the analysis section. The results are not statistically significant but I look at what they may indiciate and potential problems with the samples.

Lastly, I attempt to put the work in a wider context and discuss how it relates to previous studies in the discussion section.

2. Background and theory

2.1. Compliance theory

An initial example: Imagine that you are a rational, self-interested human living in an inflationless state with a well-developed justice system and you are considering robbing a bank. You are also well-informed about the likely consequences of any action you take. There is a 50% likelihood that you can successfully rob the bank but an 80% risk of getting put to justice and receiving a 10 year prison sentence if you do. On top of that you also lose the money from the heist if you get caught. If you successfully rob the bank, you will get $1,000,000. While in freedom, you are able to earn $30,000 per year with a 100% likelihood of keeping the money. Should you rob the bank?

The expected outcome of robbing the bank is:

$$\frac{0.50 \times 0.20 \times \$1000000}{10 \text{ years}} = \$10000/\text{year} \tag{1}$$

That is, $10,000 per year – three times less than the expected outcome if you do honest work instead. You should not rob the bank. What if there is no risk of any justice system prosecuting you and enforcing a punishment? Let us calculate:

$$\frac{0.50 \times 1.0 \times \$1000000}{10 \text{ years}} = \$50000/\text{year} \tag{2}$$

Now you can gain more from robbing the bank than by doing honest work. If you only want to maximize the amount of money you make, you should rob the bank. That is the issue faced in international law. There is no World Police that can enforce decisions by international courts and advance consent by the defendant is necessary for a court to even deem an application against it admissible. Yet, as Louis Henkin put it in his 1968 book *How Nations Behave*: "it is probably the case that almost all nations observe almost all principles of international law and almost all of their obligations almost all of the time".[1]

The inquiry into why states comply is generally known as compliance theory. I will outline a few modern examples of compliance theories below, before moving onto the one that is the basis for this thesis. Harald Hongju Koh, Oona Hathaway and Andrew Guzman make similar categorizations of these theories so as I rely largely on their papers I will use the similar classification.

2.1.1. Normative theory

Two of the more famous scholars dealing with the issue of compliance are Abram and Antonia Handler Chayes. Harold Hongju Koh reviews their book *The New Sovereignty: Compliance with International Regulatory Agreements* together with Thomas M. Franck's *Fairness in International Law and Institutions* in his 1997 review essay *Why Do Nations Obey International Law?* which also contains a history of the field of compliance theory.

[1] Henkin 1968, p. 42.

The "managerial model" developed by the Chayeses says that the reason for states complying with treaty regimes is that they are persuaded by the dynamics within treaty regimes to do so. This includes the "iterative process of discourse" among different relevant actors.[2] By repeatedly interacting and providing justification for for why the rules should be interpreted in one way, the regime members end up complying with this common interpretation.[3] Andrew Guzman, whose own work will be introduced more thoroughly later on, describes how noncompliance is explained by the Chayeses as the result of confusion around what the treaty actually prescribes and plain inability to comply. According to Guzman, the model relies on the assumptions that if states only knew how to and were able to comply, they would do so – they have a general propensity to comply.[4]

Oona Hathaway, who is also among the authors who have greatly inspired this thesis, gives an example of how the Chayeses do *not* think that compliance works. They do not believe that fear of economic or military sanctions is a viable explanation for compliance because instituting and putting in place sanctions is too politically and economically costly for the state or states that want to punish the offender and in any case are "often ineffective at changing behavior".[5] The reason that the process of persuading states to comply argued for by the Chayeses is more effective is that states are aware of a "threat of alienation from the 'complex web of international arrangements' that have become central to most nations' security and economic well-being".[6]

Thomas Franck, in his theory, puts the focus on the fairness of international rules rather than on the process of management as the mechanism for generating compliance.[7] In his view, as described by Hathaway, states comply with an obligation only when they see it as fair and legitimate. He has four factors that he thinks determine the legitimacy of an obligation: "determinancy", "symbolic validation," "coherence" and adherence to secondary rules of process. Determinancy is about transparency – ability to see that the rule is fair. Symbolic validation is when the rule has attributes signalling that it serves a purpose as a part in a system of social order. Coherence means that the rule is fair in the sense of treating like cases alike but also that it relates to the rest of the rules in the system.[8]

Koh himself is a proponent of a theory of Transnational Legal Process. He feels that both Franck and the Chayeses have something to offer even though they do not give the complete picture and so his model combines them and adds consideration of the said "transnational legal process".[9] In Koh's theory, there are three stages to the creation of a legitimate rule of international law. First, a transnational actor initializes the process by provoking "interaction". In the interactions, "interpretation" becomes necessary and

[2]See Koh 1997, p. 2601.
[3]See ibid., p. 2637.
[4]See Guzman 2008, pp. 15–16.
[5]See Hathaway 2002, p. 1956.
[6]See ibid., p. 1956.
[7]See Koh 1997, p. 2602. See also Franck's own book *Fairness in international law and institutions* (1997)
[8]See Hathaway 2002, pp. 1958–1960.
[9]See Koh 1997, p. 2603.

results in a general norm being found. Lastly, the interpretation is "internalized", so that coercion is no longer necessary for obedience. An internalized norm is obeyed out of the free will of the actor and then the issue of why states obey is solved.[10] Internalization continues after the treaty has been concluded through regular follow-up meetings where the members once again interact and interpret what has been agreed on. Gradually, noncompliers are persuaded of the norms' validity. Involving more actors, such as nongovernmental organizations and private individuals, will make internalization more likely.[11] Some ways in which internalization can happen, according to Hathaway's interpretation of Koh, is "incorporation into the legal system through judicial interpretation" and "acceptance by political elites". She also notes that Koh's theory is able to explain why a state may, contrary to its self-interest, obey a human rights norm. The problem she sees with the theory (and which is perhaps true for the other two theories also) is that it is lacking in predictive power. It is, according to her, difficult to use the theory to predict in advance what norms will be internalized and thus obeyed.[12] This also relates to Guzman's comment on the theories of all of the three authors mentioned until now not being able to predict when international law is more or less likely to actually work.[13]

2.1.2. Rational actor models

Hathaway classifies compliance theories into two schools of thought in her 2002 article *Do Human Rights Treaties Make a Difference?*: normative theory and rational actor models. The three examples in the previous section are all examples of normative theory. What they have in common, according to her, is that they think there is something beyond pure self-interest and geopolitical strategizing that affects states' behavior. There is something compelling about a norm that has been created the right way and that has a sense of justice to it, they posit. So much so that states will set aside their self-interest to stand up for a norm that they have been persuaded is right.[14] Rational actor models make no such assumptions. One might say that they lie closer to the field of international relations in that they take into account other factors than just law.

Hathaway describes rational actor models in general in the following way:

> The theories I term "rational actor models" have at their heart a shared belief that states and the individuals that guide them are rational self-interested actors that calculate the costs and benefits of alternative courses of action in the international realm and act accordingly. In this view, international law does not hold a privileged position. It is one of a series of tools available to the relevant actors in their ongoing battle to achieve their self-interested ends. Compliance does not occur unless it furthers the self-interest of the parties by, for example, improving their reputation, enhancing their geopolitical power,

[10] See Koh 1997, p. 2646.
[11] See Neumayer 2005, p. 929.
[12] See Hathaway 2002, p. 1962.
[13] See Guzman 2008, p. 8.
[14] See Hathaway 2002, p. 1955.

> furthering their ideological ends, avoiding conflict, or avoiding sanction by a more powerful state.[15]

Compare this with the example in section 2.1. In that case the only concern of the robber was how he would be able obtain as much money as possible. We see here that more complex and realistic ends are possible. States may be interested in reputation, geopolitical power or conflict avoidance.

In its simplest form, where states' objectives are limited to wealth and power, rational actor models have difficulties explaining why international law is in general observed and why – as in the case of human rights law – states go through the effort of negotiating a human rights regime, and allow the regime to monitor the state. The answer, according to some scholars, is that they do not. In reality, it costs a state very little to set up the regime, and after that, the treaty is practically irrelevant for state behavior. Both Guzman and Hathaway points out that there is a contradiction between the claims that law is just cheap talk that states engage in so as not to be seen as uncooperative and that law is not effective. If international law is not effective, there would be no need to put on an appearance of being cooperative when dealing with it.[16]

One thing that is made clear in rational actor models is that international law is just one factor that influences the behavior of states. Guzman says that "the relevant legal rules can at most put a finger on the scale in favor of compliance",[17] although he grants that this is not exclusive to rational actor models. For example, the Chayeses' managerial model implies that the same rule will be more likely to be obeyed depending on the capabilities of the state.[18]

Realism was perhaps the first rational actor model. It became common among international relations scholars after the second World War and later also among scholars of international law.[19] According to Hathaway, its view on international law is that it "exists and is complied with only when it is in the interests of a hegemon or a few powerful states, which coerce less powerful states into accepting the regime and complying with it."[20]

Within rational actor models, realism can be contrasted against institutionalism. Realism sees security as the core concern for states and thinks that states compare gains relative to each other rather than on an absolute basis. Institutionalism does not think that states use this relative way of deciding actions.[21] Relative evaluation of gains means that states would forego gains if some other state stood to gain even more from it because that would mean it would in actuality suffer a loss, relatively speaking. An example would be if two states start out with equal-sized armies and are not able to increase their sizes for some reason. If one of the states could choose between remaining in this position or letting both states develop their armies but the state making the choice can only

[15] Hathaway 2002, p. 1944.
[16] Cf. ibid., p. 1946; Guzman 2008, pp. 12–13.
[17] See ibid., p. 15.
[18] See ibid., pp. 15–16.
[19] See Hathaway 2002, p. 1944.
[20] Ibid., p. 1945.
[21] See Guzman 2008, p. 18.

double its own army whereas the other can build up its army to triple the original size, the state should choose to remain in the original position according to classical realism. If gains are instead evaluated on an *absolute* basis, states will take an action as long as the net outcome for themselves is positive. This makes sense e.g. in free trade. If one state can increase its GDP by 0.5% by joining a free trade agreement, but doing so will allow the trading partner to increase its GDP by 0.6%, this is still a deal worth making according to institutionalism. When considering the circumstances during the cold war, where there might have been a race between the United States and the Soviet Union to be the most developed nation in the world, what mattered was the relative position. This helps to understand the stance of classical realism. One can also look at this situation from an institutionalist point of view and bring up the worry that if one of the states "won", the ideology of the other would cease, and perhaps the whole of the other state, in some sense, with it. Historically speaking this is what happened, because the war ended with the dissolution of the Soviet Union. Taking that into account, a choice to enter into a treaty may still be a net loss, even from an institutionalist point of view.

Institutionalism is based on a number of assumptions. First, there are the fundamental rational choice assumptions of rationality, self-interestedness and ability to identity and pursue their interests.[22] Second, there is the assumption that states are unitary actors. Guzman's theory, which is presented in the next section, is institutionalist so he also models states as unitary actors. This means, according to him, that he does not look into their internal workings such as interaction between different branches of government, nor does he take into account supranational unions. Even if there are a number of supranational unions in existence, depending on one's definition, it is still the case that "governments retain vertical lines of accountability", he writes. Furthermore, supranational unions rarely affect legal outcomes, except for in the case of the EU. He also does not take into account international organizations even though he recognizes the value in e.g. the Constructivist perspective, where such organizations are seen as being part of the international legal arena and influencing legal norms.[23]

Another of institutionalism's assumptions is that state preferences are given and fixed.[24] In other words, the preferences are stable over time. Changes in other states' preferences or behavior do not affect them.

The name institutionalism comes from the importance that is put on "system-wide institutions", or "regimes" (which is different from the regimes that are the main topic of this thesis – the governing bodies of states). Hathaway quotes one author as defining regimes as "principles, norms, rules, and decision-making procedures around which actors' expectations converge in a given area" and two other defining it as "'sets of governing arrangements' that include 'networks of rules, norms and procedures that regularize behavior and control its effects'". Institutionalists say that the regimes exist because states can use them to maximize their gains.[25]

[22] See Guzman 2008, pp. 16–17.
[23] See ibid., pp. 19–21.
[24] See ibid., p. 21.
[25] See Hathaway 2002, pp. 1947–1948.

2.2. Andrew Guzman's theory

The hypotheses of this thesis build on a rational actor model developed by Andrew Guzman. In his 2008 book *How International Law Works*, he aims to "advance a coherent and general theory of how international law influences state behavior". He recognizes that there are multiple other factors affecting state behavior but maintains that his theory covers the part that legal considerations play for states when deciding how to act. The theory is general in the sense that it covers all types of international law from hard law to mere customary international law and even nonlegal norms.[26] It posits that all law, from hard law and soft law to customary international law and treaties, can be seen as lying on a spectrum. They all work by the same mechanism and the difference between them is in degree of reputational capital pledged and seriousness of commitment.[27] Guzman adopts institutionalist assumptions for his book.[28] What this entails is discussed in section 2.1.2. He also describes his method as originating from the social sciences and sees his book as part of the movement of international law towards that approach.[29]

The book starts by presenting the different game theoretical situations that states can find themselves in when interacting with other states. In general, it is possible to divide theses into situations where cooperation is easy and where it is difficult. It is easy when all states involved stand to gain more individually from cooperating than from defecting. The most trivial example is when it is clear to all states involved that an action would result in a negative outcome for themselves. In such cases, law is not needed because the states would behave the same way in either case. Guzman gives the example of a potential treaty between the United States and Canada reaffirming their obligation not to attack each other. Neither state has anything to win on net from a decision to invade the other and this is obvious to both of them so the treaty is superfluous. Then there are also *coordination games* where both states want to cooperate because it is clearly to their benefit but an agreement is still useful to have in order to coordinate state behavior. Guzman gives the example of the Warsaw Convention which regulates air travel, making international air travel easier for everyone by ensuring that standards are the same regardless of where in the world one flies. It does not matter much to any individual state what exactly the standards for air travel regulations are but unless something is agreed upon, they will differ between states and cause headaches for air travel companies.[30]

Why international law is complied with in the aforementioned type of situations can be understood without much trouble. Where it gets interesting is when there are net gains to be had by cooperating but for each party involved, noncooperation can lead to larger individual gains than they would have obtained if they cooperated. Thus, *prima facie*, game theory predicts that cooperation will not happen, resulting in a situation that is worse for both parties than if they had settled for a slightly lower-than-maximum

[26] See Guzman 2008, pp. 23–24.
[27] See ibid., pp. 212–213.
[28] See ibid., p. 20.
[29] See ibid., p. 13.
[30] See ibid., pp. 25–27.

payoff by cooperating but one which is still better than what they ended up getting. For a one-time interaction between two states that act according to the rational choice assumptions, there is no way to achieve cooperation.

2.2.1. Reputation

Guzman uses the ABM treaty between the United States and the Soviet Union as an example of a prisoner's dilemma. He also notes that the example will generalize to situations where several states are involved.[31] ABM stands for "Anti-ballistic missile" and the treaty was an agreement not to construct such devices. If they were constructed, they could be used to shoot down nuclear missiles coming from the other party and thus providing a benefit for the builder. There is also a negative side, which is that anti-ballistic missile systems are expensive to build. If only one of the parties built it, they could bomb the other party without fear of retaliation because they could just shoot down the missiles coming back at them. Thus, from the perspective of an individual state, if the other state does not build the system, you should build it because it will give you huge military gains. However, you should also build it when the other state does build it, because the alternative is to be dominated. Consequently, both parties construct the system, which ends up costing them a lot of money instead of both abstaining from building it, which would have saved them that cost. The payoffs suggested by Guzman for the situation are show in table 1 where the number within parentheses represent (payoff for the United States, payoff for the Soviet Union).[32]

Table 1: ABM Treaty

		United States: Comply	United States: Violate
Soviet Union	Comply	$(100, 100)$	$(200, -50)$
	Violate	$(-50, 200)$	$(80, 80)$

Before moving on to explain how compliance with the ABM treaty can be generated, I will introduce the concept "discount rate". The discount rate of a state is a representation of how much the state values future gains. For example a discount rate of 10% would mean that a gain of 80 in the next round is equivalent to a gain of 72 this round for that state. The discount rate compounds like interest rates, so a gain of 80 two rounds into the future will be worth $80 \times 0.9 \times 0.9 = 64.8$ for a state with a discount rate of 10%.

Cooperation is possible when the interaction continues for several rounds, such as in the case of the real ABM treaty. Guzman shows how, when potential future gains are taken into account, a rational, self-interested state will in fact choose to cooperate. In short, he adds together all future payoffs, taking into account discount rates, to find that at a given discount rate and given that the payoffs are as he suggests, a rational

[31] See Guzman 2008, p. 36.
[32] See ibid., pp. 30–31.

state should comply with the ABM treaty. For the solution that he provides, it is also necessary that the other party will never cooperate again after one breach from the first. The grounds for not cooperating again is not vengefulness but a new estimate of the discount rate of the other state that is high enough that it would be rational for it to defect, given the available payoffs. Guzman calls the estimate by other states of one state's discount rate that state's *reputation*.[33] There are of course many things which can be called reputation but hereinafter, unless otherwise specified, I mean reputation for compliance with international law.

This example is good for explaining how reputation works. If we call the parties State A and State B, what is it that happens if State B defects? Since both of them are self-interested, State B will do what it gains the most from. If it chooses to defect, State A learns that that is what is most beneficial for State B, which says something about how much State B values future gains or in other words, how shortsighted it is. With this new knowledge, and under the assumption that the payoffs remain unchanged in the next year, there is no point in trying to negotiate the treaty again with State B since State A now knows that State B will breach it.

It might further facilitate understanding to include a passage from the book where Guzman explains reputation for compliance:[34]

> If an observing state knew everything about the acting state, including the extent to which it preferred gains today over gains tomorrow (i.e., its discount rate) and the value for it of all possible interactions, the observing state would be able to calculate the acting state's payoffs and accurately predict its actions. Because these things are not observable, however, observing states form a judgment about an acting state's "reputation," which represents a measure of its willingness to comply with its international legal obligations.

One important thing to keep in mind is that states do not want a good reputation for its own sake. They want it because it will make it easier for them to enter into future contracts, from which they will be able to get what they really value.[35] Regardless of the actual discount rate of the state, a good reputation will make its promises more credible and so make future cooperation and the finding of states to cooperate with easier, while also letting it extract more from a bargain.[36]

One consequence of the fact that reputation is valuable is that states will have an incentive to comply with some existing obligations even when it would be in their interest not to, if it was not for the factor of reputational gains. Compliance in such cases can be seen as a costly signal.[37] Non-compliance would generate a cost for the state. Every time a state enters into a treaty, it asserts that it has a discount rate such that it will comply with the obligations stemming from it. If the state then breaches one of those

[33] See Guzman 2008, pp. 38–40.
[34] Ibid., p. 34.
[35] Cf. ibid., pp. 35–36.
[36] See ibid., p. 35.
[37] See ibid., p. 36.

obligations, it will be harder for it to be believed when making such a claim in the future, and that is what is meant by a "loss of reputation."[38]

There are two other means by which compliance can be generated in Guzman's theory: reciprocity and retaliation. Reciprocity means responding in kind to the actions of the other party, in other words compliance in response to compliance and noncompliance in response to noncompliance. In this regard, reciprocity is similar in its effects to the *reputational* mechanism for compliance. However, there are differences. For example, if a state temporarily sees a change in its payoffs which makes it more beneficial for it to breach an agreement that year but clearly not the next, reputational concerns by themselves would not be able to generate compliance. The reason is that the noncompliance of the first year does not indicate that the state has a too high discount rate to be willing to comply going forward. The next year, the benefit from noncompliance is lower, and so may not be worth the breach for the state.

Reciprocity, on the other hand, is a strategic way to signal that one does not tolerate noncompliance and will withdraw from the agreement even if it is still valuable. At first glance, this goes against the rational choice assumptions. However, if the reciprocity sets an example and makes the other state less likely to breach other agreements, it may still be the best way to maximize overall gains.[39]

Another example of reciprocity would be when the breach by one party somehow changes the available payoffs for future rounds and make future cooperation worthless. This could be the case of a treaty banning dumping nuclear waste near a specific coral reef. If one party breaches the treaty, there is no point in renewing it even if the breach will not be repeated since the coral reef is already lost.

The third mean of generating compliance is called retaliation and includes all types of sanctions – actions taken to punish a violator at an immediate cost to oneself.[40] One may wonder why a rational state would accept a cost for itself in order to punish another. A rational state is not interested in revenge *per se*. Instead, what Guzman thinks is at work when a state sanctions another is an attempt at building a reputation as a state that will punish violators. If the state can make credible threats of sanctions when entering agreements, it is more likely that the other parties will comply because they will take into account the cost of being sanctioned when calculating the gain from defecting from the treaty.[41] It should be noted that a reputation for punishing those who violate agreements is different from the reputation for compliance that is generally discussed in this thesis.

2.2.2. The spectrum of international law

Guzman suggests that international law lies on a spectrum rather than being separated into categories. He argues that treaties, soft law, customary international law and mere norms are not fundamentally different from each other and should not be thought of as separate areas. They all work by the same mechanisms and differ only in how much

[38] See Guzman 2008, p. 38.
[39] Cf. ibid., p. 44.
[40] See ibid., p. 34.
[41] See ibid., p. 46.

commitment they represent. There are other ways to commit more strongly to a promise. One can for example provide for dispute resolution mechanism in the agreement to show more commitment, include escape clauses to commit less strongly, or allow for monitoring of compliance to increase commitment.[42]

Soft law may not be biding, but that does not mean that it is not possible to have a reputation for not complying with it. If states wanted to, they could also include dispute resolution mechanism in soft law agreements or even provide for sanctions for noncompliance. This is true whether or not states actually do this.[43] Consequently, the same three mechanisms that generates compliance with hard law – reputation, reciprocity and retaliation – are available for soft law agreements.

The reason that treaties are the most serious form of commitment is not that it is more costly to enter into one than to enter into a soft law agreement but that it is understood by everyone to be the maximal pledge of reputation. States may sometimes want to leave some leeway for themselves by making a less serious commitment and one way to do this is to choose soft law over hard law. Pledging more reputation lets a state extract more from the other parties but the flip side is that it suffers a greater reputational loss if it breaches the agreement. States always have to take into account both how strongly they want to bind the other parties and how strongly they want to bind themselves when choosing the form of an agreement.[44] Including monitoring clauses in a treaty makes the commitment more serious, so it places it on another point in the commitment spectrum than a treaty without such a clause. The reason that it shows more serious commitment is that defection is less likely when the risk of being exposed is higher.[45]

2.2.3. Compartmentalization

One question when it comes to reputation is whether states have one or several of them. Different scholars have, according to Guzman, different opinions on this but he himself suggests that reputation is indeed to some extent compartmentalized. He thinks that there could be different reputations for the same state by issue area and by regime. He also discusses the possibility of there being a separate reputation between each pair of states (dyads). Reputation by area may entail separate reputations for human rights law and trade law. When there is a regime change in a state, it can reasonably be suspected that the likelihood of the state complying with its obligation changes. Guzman suggests that even smaller "regime changes" can lead to differences in reputation, such as difference in expectation of compliance with free trade agreements depending on what party the president of the United States is from.[46] There have been both studies indicating more impact of international law on republicans than on democrats, and more impact on moderates and liberals than on conservatives.[47] A more complete change of power in the

[42] See Guzman 2008, p. 9.
[43] See ibid., p. 160.
[44] See ibid., p. 59.
[45] See ibid., p. 135.
[46] See ibid., pp. 100–109.
[47] See Stein 2017, p. 19.

state will probably lead to a more "separate" reputation for the new regime. On the other hand, perhaps it should not be seen as completely unrelated to the previous one. There are several reasons to believe that the previous and current regime of a state are similar in some ways with regards to compliance with international law; perhaps the previous and current leader come from the same culture or maybe parts of the bureaucracy of the state are inherited by the new regime. The larger the change, the more separate the reputations of the regimes should be expected to be, generally. Of course, two very different regimes might value the future equally much just by chance. A change from a democracy to an autocracy or vice versa seems like it should indicate that the regimes are more different, so the reputation should be seen as more compartmentalized, than a change from e.g. one autocratic regime to another. The same is true if the change is from a communist government to a liberal democratic government, such as in Eastern Europe after the cold war.

Reputations for different issue areas should be expected to be similar in many cases. Firstly, it is not always clear where one issue area starts and another begins, or how finely issue areas should be divided. Should, for example, environmental law be considered one issue area or should protection of the ocean be distinguished from agreements for the purpose of controlling carbon dioxide emissions? Secondly, all states have their own general willingness to accept present costs in order to get future gains which influences how they behave in all issue areas.[48]

Should not the underlying discount ensure that reputations are the same regardless of what issue area the agreement covers? Guzman is not completely clear on this; maybe it is seen as self-evident, but it seems that it has to do with different areas in themselves being of different value to a state. Cooperation in one area might just not be of that much benefit for one area so a state might not be willing to invest in creating and maintaining a reputation.[49] It must also be noted that reputation is not an objective property of a state but the estimate by other states about how likely it is to comply with its obligations in various circumstances. Thus, it is each observing state that determine how much influence a compliance decision in one issue area has on other issue areas.[50]

Reputation gets more and more entrenched with time. Guzman provides a real world example of how this happens for one regime. Gorbachev attempted to improve the reputation of the Soviet Union in its final days but since the Soviet Union already had a well-established reputation, this was more difficult than if he had started from a clean slate. Other states' prior beliefs were already strong and so individual compliance decisions had a smaller effect. On the other hand, in the early 90's after the fall, Russia announced that they would honor the debts of the previous regime, which helped them quickly create a new, better reputation.[51] This shows that at least in one case, a new regime meant a fresh start for the state's reputation.

[48] See Guzman 2008, p. 102.
[49] See ibid., pp. 103–104.
[50] See ibid., p. 104.
[51] See ibid., pp. 89–90.

2.3. The instrumental and expressive roles of treaties

Treaties can have at least two purposes – they may be used to try to change state behavior and they may be used as a way of expressing where one stands on an issue. Hathaway calls these the instrumental and the expressive roles of treaties.[52]

The instrumental purpose of treaties is fairly straightforward and what one at first glance probably expects to be the sole purpose of them. The institutionalist framework of Guzman and others says that states use treaties or the "regimes" around them to achieve mutual benefit. This seems to explain the instrumental role. The expressive role is discussed by Hathaway when interpreting the results of her study, which will be detailed in the next section. Since I do not bring up her discussion at that point, I will summarize what she has to say about the expressive role of treaties here instead, since it may be useful to interpret my own results later on.

Membership in a treaty can express at least two things: what the members believe is acceptable behavior and what kind of states the members are. In other words, what they want the law to be and how they want others to expect them to act. For the former, the treaty by itself, as a document and a regime in the world, gives a list of prescribed behavior that states should comply with. States that have joined it signal to others that this is how they want others to act. For the latter aspect, the treaty shows the world that each individual member is the type of state that acts in that way, so it is a form of position taking.[53]

Why then would other states believe that ratifiers really are trying to change their behavior by ratifying the treaty and are not just using its instrumental role? Hathaway's explanation is that, at least in the case of treaties with little monitoring, it is hard to say what states are really complying and which are not. Some states probably really improve their behavior after joining but it may be hard to separate these from those who do not. Thus, all members receive the expressive benefit of the treaty.[54]

The expressive and instrumental roles of treaties may not always move the members in the same direction. If a state knows that by expressing support for the treaty there will be less pressure on it to actually comply, it may end up just ratifying it in lieu of actualizing real change.[55]

In a comment on the institutionalist theories, Hathaway says that they need to take into account the fact that states may have reputation to gain from joining the treaty but face little risk of loss of reputation when monitoring is weak.[56] However, this dynamic should be less prominent in treaties that have stronger monitoring mechanisms.[57] One example of such a treaty would be the CCPR when ratified *together with* its Optional Protocol. The Optional Protocol provides for an individual complaints mechanism, so the Human Rights Committee could be contacted by any victims from states that ratified it.

[52] See Hathaway 2002, p. 2002.
[53] Cf. ibid., pp. 2005–2006.
[54] See ibid., p. 2011.
[55] See ibid., p. 2013.
[56] See ibid., p. 2011.
[57] See ibid., p. 2006.

2.4. Previous studies

In this section I want to give a brief overview of previous studies that have used similar methods and which have inspired this thesis. Four papers are reviewed – by Oona Hathaway from 2002, a similar one by Eric Neumayer from 2005, one with results contradicting both of them by Christopher Fariss from 2017 and finally one by David Cingranelli and Mikhail Filippov from 2018, responding to Fariss.

2.4.1. Oona Hathaway 2002

The earliest of the four studies, and perhaps the most comprehensive, is Oona Hathaway and her team's 2002 study. Titled *Do human rights treaties make a difference?*, it looks at the five areas genocide, torture, civil liberty, fair and public trials, and political representation of women.[58]

The analysis looks at two things – first, whether states comply with the treaties studied and second, whether the treaties change their practices.[59] Compliance just requires that the behavior of the states conforms to what the treaty prescribes. However, they may have had the same behavior even if they did not ratify the treaty. Thus, the second part of the analysis focuses on whether ratification changes their behavior. This part is important, because, as Hathaway says, "law that has no effect on behavior cannot really be said to be law at all."[60] She looks at ratification of various treaties and measures state practice with data from four sources, the Center for International Development and Conflict Management at the University of Maryland, College Park, the United States Department of State Country Reports on Human Rights, Freedom House's Annual Survey of Political Rights and Civil Liberties, and the Inter-Parliamentary Union.[61]

Hathaway draws several conclusions from her results. States that have ratified human rights treaties generally have higher human rights ratings than those who do not. This varies though, as many of the states with the worst ratings have ratified more treaties than some other groups. Furthermore, she finds that ratification is sometimes associated with worse and sometimes with better ratings than what would have been expected if they were not ratifiers. Ratifiers of the Optional Protocol to the CCPR and article 21 of the Torture Convention generally have better human rights ratings than nonratifiers but she does not think that the explanation is causal.[62] For the universal (rather than regional) human rights treaties that were analyzed, ratification shows either no statistically significant relation to human rights ranking, or it is associated with worse ratings than what would have been expected if they had not ratified.[63]

[58] See Hathaway 2002, p. 1965.
[59] See ibid., p. 1965.
[60] See ibid., p. 1989.
[61] See ibid., p. 1967.
[62] See ibid., p. 1999.
[63] See ibid., p. 1994.

2.4.2. Eric Neumayer 2005

Eric Neumayer presents a study done by him and his team similar to the one by Hathaway in his 2005 paper *Do international human rights treaties improve respect for human rights?*. He gives a clear description of his model and compares his method to Hathaway's. Differences include his using a dummy variable that gives each year its own intercept, in contrast to Hathaway who used a linear time trend for the same purpose of accounting for changing levels of respect for human rights over time. He also has a different way of dealing with the fact that the explanatory variable – the country rankings – are ordinal and not cardinal.[64] They control for a number of variables, among them democracy, external and internal armed conflict, economic development, population size and civil society strength.[65] They do find positive results for the relation between ratification and improvements in human rights but it is dependent on democracy and the strength of civil society.[66] However, they note that due to the various problems they encountered with the statistics, even if they had not found any positive effect of ratification, it would not have meant that there was none. The effect might just have been so slow that it would not be visible in short time periods, for example.[67]

2.4.3. Christopher Fariss 2017

In 2017, Christopher J. Fariss published *The Changing Standard of Accountability and the Positive Relationship between Human Rights Treaty Ratification and Compliance* in which he tests the effectiveness of human rights treaties, similarly to Hathaway and Neumayer before him. One of the major differences in his study is that he develops a latent variable model to account for changing standards of accountability within human rights reporting.[68] Fariss thinks that the standards of accountability have risen due to three reasons. Firstly, there is more information available for monitoring agencies which lets them come up with a better assessment of the actual state of affairs in countries. Secondly, the agencies have better and better access to countries over time (government documents, witnesses, victims, prison sites and other areas). Lastly, more and more actions are classified as human rights breaches.[69]

Fariss furthermore controls for states' embeddedness within the global human rights regime by developing a "dynamic binary item-response theory" model. "Embeddedness" is a variable he constructs himself based on, among other things, what treaties a state has signed, the number of treaties available for signing in a given year, and reservations to treaties.[70]

In the end, he contends to have found, using human rights data accounting for changing standards of accountability, evidence that "ratification of human rights treaties is

[64] See Neumayer 2005, p. 936.
[65] See ibid., p. 939.
[66] See ibid., p. 950.
[67] See ibid., p. 951.
[68] See Fariss 2017, p. 247.
[69] See ibid., pp. 242–243.
[70] See ibid., pp. 247–250.

empirically associated with higher levels of respect for human rights over time and across countries." He also finds that levels of respect for human rights coincide with "the increasing embeddedness of countries withing the international human rights regime."[71]

2.4.4. David Cingranelli and Mikhail Filippov 2018

Fariss' results go against conventional wisdom in the field of international human rights treaty compliance.[72] Naturally, this is a reason for evaluating his work sceptically. Two other researchers, David Cingranelli and Mikhail Filippov, do just that in their 2018 article *Are human rights practices improving?*.[73]

Cingranelli and Filippov start by recognizing that there is an issue with uncertain data when it comes to human rights rankings. Depending on what type of human rights breaches one looks at, one can come to different conclusions about what the trend in human rights is. Even if one takes a more nuanced view and looks at breaches in all fields, one must determine how much weight to give to each group; even the choice of giving all sources of evidence equal weight is a choice.[74]

Fariss tries to account for the inconsistency he thinks exists in the human rights data by using a dynamic binary item response theory model, as I mentioned in the previous section. Cingranelli and Filippov note that this is an innovation from Fariss and analyzes the method critically. They note that in general when using a method of adjusting data with other data, the result should be somewhere in between the two sources. However, they find that Fariss' results are entirely determined by the indicators for mass killings. They use Fariss' own code but modify it to only base the adjustments on the indicators for mass killings and get an upward trend in human rights. On the other hand, when they repeat the analysis with only other indicators, they find no upward trend. The most important sign that there might be something wrong with his model is that even when they replace the index to be adjusted with random values, the model "corrects" them to show an upward trend. According to them, the version they created using only the indicators for mass killings explain 88% of the variation is Fariss' scores.[75]

The explanation for mass killings having such a large impact on the final scores seems to be that when Fariss combines the results from the two categories, he uses a fixed intercept for mass killings but a variable one for lesser human rights breaches. The latent variable he produced thus has to fit with the fixed intercept of mass killings but is allowed more freedom when it comes to the other indicators.[76]

Fariss assumes that the instances of mass killings, political executions and genocides can be used as a baseline for the other indicators. According to Cingranelli and Filippov, this assumes that there has been no change in standards of accountability for the reporting of these incidents. They are sceptical of that assumption but also of the assumption

[71] See Fariss 2017, p. 266.
[72] See ibid., p. 266.
[73] See Cingranelli and Filippov 2018.
[74] See ibid., pp. 1084–1085.
[75] See ibid., p. 1086.
[76] See ibid., p. 1086.

that incidents of mass killings say something about the hidden incidents of lesser human rights breaches. It could be the case that regimes nowadays have learned to use more of the lesser human rights breaches and less of the mass killings as a way of selectively achieving their goals. They also doubt that incidents of mass killings in some states can be used to say something about lesser human rights breaches in all the states of the world.[77]

Finally, they note some surprising results for the period 1949–1975, before which there were any human rights indexes but for which Fariss anyway uses the latent variable to produce them. Fariss' scores are equal for North Korea and The United States in 1953. Those for the United States for all of the 1950s are worse than for South Africa which was building its Apartheid regime at the time.[78]

It should be noted that their critique is made possible by Fariss' whole model, with programming code, being available[79] for them to work with. Fariss' transparency puts him at risk for critique by others but makes possible to learn from his model and improve on it.

[77] See Cingranelli and Filippov 2018, pp. 1087–1088.
[78] See ibid., p. 1088.
[79] See ibid., p. 1086.

3. Development of hypotheses

3.1. The influence of regimes on reputations of states

The hypothesis of this thesis builds on Guzman's theory of international law. As we saw in section 2.2.3 on compartmentalization there is reason to believe that reputation is compartmentalized by regime. In the United States, this would for example mean that the state has one reputation when the Democrats are in power and another when the Republicans are in power. When a party comes back to power, the reputation it had last time it was in power is taken into account by other states when determining its present-day reputation. There is a "jump" in the "reputation level" of the state in the eyes of other states. Conversely, when the same party remains in power, there is no such jump. When there are larger changes in regime, for example after a coup, it might make more sense to speak of "replacement" or "resetting" of the reputation of the state instead, given that the old regime never comes back to power.

Of course, many factors play a role when the reputation of a state is determined. However, the "regime reputation" is the only factor that should make a sudden large change when there is a regime change. I also assume that which regime is in power plays a significant part in determining the general reputation of the state. Perhaps, even though the regime matters a lot, the reputation does not change much during power transitions in stable democracies but that would be because the regimes are very similar, not because the regime does not matter. To illustrate, the regime changes in Eastern Europe during the early 1990s likely mattered a great deal for states considering making agreements with those states and trying to figure out how likely they were to comply with their international legal obligations.

3.2. The relation between general reputation and human rights reputation

As explained in section 2.2, According to Guzman's theory, there are three things that can generate compliance. In most areas of law, all factors are at work. This makes it difficult to say if a state complied to protect or enhance its reputation, or out of fear of reciprocity or retaliation. Human rights law is different because states will not respond to a violation by themselves committing a human rights violation, and sanctions on states for human rights violations are, albeit existent, rare.[80] What remains as a reason for compliance is reputational concern.

One might ask why a state would want a good reputation for compliance with human rights law. On the one hand, reputation is compartmentalized not only by regime but also by issue area. Just because a state shows one degree of interest in future payoffs in trade does not mean it has to show exactly the same concern for protection of foreign nationals. On the other hand, the compartmentalization is not absolute. Actions in one issue area say something about how the state will act in another, according to Guzman. Each state has only one discount rate and behavior in any issue area says something

[80] See section 2.2 and also Hathaway 2002, p. 2007.

about it.[81] Therefore, a state should want to improve its reputation in the human rights area both because there are long term gains from complying but also because it marginally improves its reputation in other areas where there are larger gains to be had. The reputation for compliance with human rights obligations is taken into account by other states when forming beliefs about how likely the state is to comply in other areas.

In order to build a reputation in the human rights area, it must be true that compliance with one's treaty obligations in general provides long-term gains that outweigh the present cost. If states are rational, they only enter into agreements where cooperation is to their benefit, so no assumption other than rationality should be necessary to say that compliance is more beneficial in total compared to non-compliance, seen over longer time periods. Further support for human rights treaties providing long-term gains is given by Daniel Farber. According to him, a state that binds itself to abide by human rights signal to investors – even if the investors do not care about human rights in themselves – that the state is willing to sacrifice short-term gains for benefits in the long-term.[82]

3.3. Expected behavior for regimes

The reputation of concern in this thesis is reputation for compliance with international law. If a state does not have obligations related to human rights, they cannot improve this reputation by complying because there is nothing to comply with. They might benefit from it in other ways, such as by gaining goodwill, but they will not be known for following up on what they have promised if they did not actually promise it by e.g. ratifying a treaty. Therefore, there is no reputational gain at all for them in complying, even when the regime is new. This is the case for nonratifiers. Now let us look at ratifiers.

If a state has international obligations concerning human rights, they should benefit reputationally from complying. There may sometimes be benefits from committing breaches. It may not be obvious what those would be, but suffice to say, when thinking of all the ways a state may act in the pursuit of their goals, it would be exceptional if the fastest or cheapest way to achieve them never entailed a breach.

At the very start of a new regime, there are no historical incidents for judging how likely it is to comply with its obligations. It has not yet had any opportunity to make compliance decisions. The first time it makes a compliance decision, this one compliance decision is all that other states can judge it by. Thus, it is very important. It is the only direct basis other states have to determine what to expect from it regarding compliance in this area. In other words, the reputational gain from complying at this point in time is very large. Compared to future compliance decisions, the state should be much more likely to comply now. Once its reputation has been established, each compliance decision has much less impact on its reputation and so it takes much less short-term benefit from a breach for the state to commit the breach. The less well-established the reputation of the state, the more likely it should be to comply in a specific instance.

To put the situation in very theoretical, and somewhat simplified terms, if the first compliance decision of the state does not result in a breach, it will be known to have

[81] See section 2.2.3.
[82] See Farber 2002, p. 98.

complied in one out of one cases, or 100% of the time. If the next case is a breach, it will be known to have complied half of the time. One breach this early in its existence has a huge effect on its reputation. On the other hand, if the state has taken nine compliance decisions and none of them resulted in breaches, it has complied in nine out of nine cases, and like before is known to have complied 100% of the time. If the next one results in a breach, the state will now be known to have complied nine out of ten times. The hit to its reputation at this point is much smaller. It is still known to have complied 90% of the time. There is thus less reputational cost to non-compliance. Of course, the recency of the act also matters so the actual reputational cost may be larger than 10% in this case but the history of compliance is also one factor. The more time passes, the less the reputational cost for a single instance of non-compliance. Sooner or later, the state should end up at the level where it complies at its natural rate, taking into account that there really still is a small reputational gain from complying. Here it is important to note that not only the cost of non-compliance but also the benefit from compliance decreases. This should result in a stabilization of the reputation somewhere close to the state's real discount rate.

I will illustrate with an example in an area where the benefits from non-compliance are more clear. A state might have used its newly established good reputation to enter into trade agreements but then acted in opposition to the agreement when it discovers that it can make its population happy and increase its tax income by protecting native industries. It might start out only doing it in cases where the gain from breaching is huge, but each such occasion decreases the state's reputation until it finally lands at the level that actually corresponds to the state's discount rate.

One caveat is that in order for the regime-related reputation of the state to be important in determining its more general reputation, there must be an expectation from other states that the new regime is different from the previous one. This should be the case after large regime changes, for example when there is a large change towards or away from democracy.

The preceding discussion means that ratifiers of a treaty should see an initial peak in its reputation that then steadily declines and levels out, at least when comparing it to nonratifiers. If ratifiers see initial improvements followed by slow declines, this should be noticeable as the level of respect for human rights in general being lower before a regime change, because the reputation for that regime has leveled out, compared to after, when the new regime is investing heavily in human rights law compliance. This is the first hypothesis that will be tested. The second is that during the first few years of a regime, the level of respect for human rights should decrease.

One issue with the model is that even if a state does not comply, it may be hard for other states to take notice if there is no good monitoring mechanism in place. When there is, each compliance decision should become more important since the risk of it actually affecting the reputation is higher. If a treaty provides for stronger monitoring mechanisms, ratifiers of it should then be expected to see the two effects of the previous hypotheses more strongly. This is the third hypothesis.

3.4. The case of the CCPR and its Optional Protocol

There are several large human rights treaties in the world and therefore many sources of international obligations to respect human rights (this is discussed further in section 3.6.1). Controlling for ratification of all of these would make my results much more solid but would entail much more work than there is time for. For one thing, different treaties protect different rights so different measures of compliance would need to be used and for many of them there may not even be a good measure that covers enough states and enough years.

The International Covenant on Civil and Political Rights protects mainly, as the name implies, civil and political rights. Freedom House's Freedom in the World survey provides civil liberties and political rights rankings for most states for a larger number of years. The relationship between the two will be explored more thoroughly in section 4.3.

The Optional Protocol to the CCPR has the sole purpose of providing an individual complaints mechanism for the rights protected in the CCPR. Thus, ratification data for it can be used to test the prediction concerning the effect of stronger monitoring mechanisms.

3.5. Predictions

The three hypotheses of the thesis has already been mentioned but will be reiterated here for clarity. What I expect to find is that:

1. There will be a larger change for the better with regards to human rights after a regime change in states that have ratified the International Covenant on Civil and Political Rights compared to those that have not.

2. There will be a negative trend in human rights development the first years of a regime's existence in states that have ratified the International Covenant on Civil and Political Rights compared to states that have not.

3. The effects in the two other predictions will be stronger when states have also ratified the Optional Protocol.

3.6. Additional concerns

3.6.1. Other sources of obligations

There is a danger that if one only looks at the CCPR and its Optional Protocol, one fails to account for states that have international obligations to respect civil and political rights that come from other treaties or from customary international law.

Out of the three studies in section 2.4, Hathaway looked at the following universal treaties: Convention on the Political Rights of Women, Convention against Torture with its articles 21 and 22, the CCPR with its Optional Protocol and the Genocide Convention. She also looked at the regional treaties of the American Convention on Human Rights, the American Torture Convention, the European Convention on Human Rights, the

European Torture Convention and the African Charter on Human Rights.[83] Neumayer looked at the exact same regional treaties, and with the exception of not including the Convention on the Political Rights of Women, also the same universal treaties.[84]

Fariss' selection was slightly different. The treaties he studied included the Convention Against Torture, the Covenant on Civil and Political Rights, the Covenant on Economic, Social and Cultural Rights, the Convention on the Elimination of All Forms of Racial Discrimination, the Convention on the Elimination of all Forms of Discrimination against Women, and the Convention on the Rights of the Child.[85] The first two were studied by Hathaway and Neumayer also, but the rest were not.

Apart from the listed treaties, customary international law also includes some human rights. The International Court of Justice pointed out in *Reservations to the Convention on the Prevention and Punishment of the Crime of Genocide (Advisory Opinion)* that "reservations contrary to provisions of the CCPR which reflect either *ius cogens* or even just customary international law are inadmissible."[86] This implies that parts of the CCPR is customary international law or *ius cogens*. When that is the case, all states have the obligation reflected by those parts, not just the ratifiers of the CCPR.

The issue if there are many treaties or other sources of obligations that overlap with the CCPR is that the mechanisms described above will be at work for nonratifiers of the CCPR who are ratifiers of other treaties that provide similar obligations. For example, the Convention Against Torture protects political prisoners and nonpolitical prisoners alike. A state will thus be able to gain reputationally by doing something that improves political rights, and the mechanisms described above will then be at work even for this state. The prohibition on torture is also generally considered to be part of customary international law.[87]

My predicted effects should, however, be seen more strongly for states that have all the obligations corresponding to what Freedom House assesses and nothing else. For my discussion about the correspondence between what Freedom House measures and what the CCPR prescribes, see section 4.3. Some of the treaties that previous studies have used seem to have very little overlap, and those which have more still seem to cover a wider number of rights. There may of course still be some noise because of this. Noise from rights protected under customary international law may be weaker for reasons discussed in section 2.2.2. This is especially important in the case of the UDHR which is considered by some[88] to be customary international law and by others[89] to be soft law. Its relation to the CCPR is strong, which is also described in section 4.3.

Both Hathaway[90] and Neumayer[91] use Freedom House to measure compliance with the CCPR, so at least my method does not deviate from the one commonly used.

[83] See Hathaway 2002, p. 2026.
[84] See Neumayer 2005, pp. 937–939.
[85] See Fariss 2017, p. 251.
[86] Tomuschat 2019.
[87] See Guzman 2008, p. 186.
[88] See Charlesworth 2008, par. 16.
[89] See Guzman 2008, p. 216.
[90] See Hathaway 2002, p. 1975.
[91] See Neumayer 2005, p. 935.

3.6.2. Reservations

Another problem is that some states make reservations to the CCPR. The human rights rankings which I use do not take into account any reservations by the governments. Freedom House themselves claim that they assess "real-world rights and freedoms enjoyed by individuals, rather than governments or government performance per se."[92] The problem for this study then is that a state which has made a reservation will have its level of compliance evaluated in the same way as all other states.

However, neither the convention, nor the Optional Protocol have any provisions concerning the admissibility of reservations. The International Court of Justice said in its advisory opinion *Reservations to the Convention on the Prevention and Punishment of the Crime of Genocide* that reservations may not conflict with the object and purpose of the treaty. Specifically in the case of the CCPR, it noted that reservations to it cannot run contrary to those rights that reflect *ius cogens* or customary international law.[93]

Despite the fact that the admissibility of reservations is limited, many states have handed them in. Bahamas, in the reservation, say that while they recognize and accept the right of wrongfully punished people to be compensated, they reserve the right not to do so due to problems of implementations. Gambia made a reservation to article 14 (3) (d) of the covenant, concerning free legal assistance, with the justification that it was not financially viable for them to provide it to all those accused of crimes. The United States made a reservation to, among others, article 20 concerning propaganda for war and incitement to discrimination, hostility or violence, insofar as it would conflict with their constitutional right to free speech. Apart from the reservations by these three states, there are numerous others.[94]

It is true that reservations may pose problems for the study, but in general it should be expected that most states that ratify will accept most parts of the treaty. On the other hand, it is probably the case that if a state makes a reservation, it is likely that they will occasionally act in a way that would otherwise have been contrary to the treaty, which will affect their scores when Freedom House evaluates them. There is no easy way around this problem, so it will be assumed for the sake of this thesis that either the reservations are so limited in scope that they do no affect the score of the state or they are so contrary to the treaty that they are invalid (cf. the advisory opinion above). The suitability of using the Freedom House data is discussed in detail in section 4.3.

[92] See Freedom House 2019c.
[93] See Tomuschat 2019.
[94] See United Nations Treaty Section 2020a.

4. Method

4.1. Introduction

Testing the predictions of this thesis requires one to know three things: what regimes have existed, what the level of respect for human rights is in a given state and year, and the ratification status of every state. I am using three different datasets to acquire this information and they will be detailed in the next section.

The information mentioned in the previous paragraph needs to be processed, combined and interpreted. I have elected to carry out these processes in the programming language R[95] and have done so for two reasons. First, it allows anyone to reproduce the results and audit the process of obtaining them. Second, it was created for "statistical computing and graphics" and specifically for time series analysis[96], which is what I need it for.

The full programming code used to obtain the results of this thesis can be found in appendix A and all the input needed for the program is available online[97]. Anybody who wants to can reproduce the results by running the code with the datasets as input.

Finally, the statistical significance of the results must be checked to make sure that they are unlikely to be just due to chance.

4.2. Data sources

The Center for Systemic Peace, through the Integrated Network for Societal Conflict Research, maintains and annually updates and revises databases of "regime authority characteristics and transitions." The data covers all independent countries with populations greater than 500,000 individuals.[98] They provide several databases but for this thesis I am using the *Polity IV Annual Time-Series, 1800-2018*[99] to determine when regimes begin and end in each country. I will hereinafter refer to it as the Polity dataset.

The Polity dataset is in a country-year format.[100] That means that each *row* in the dataset contains information for one country, one year. For example, there is one line for France in 1935 where it has a **DEMOC** value of 10, **AUTOC** value of 0, and **DURABLE** value of 58, which indicates that it has a democracy score of 10 (the highest), an autocracy score of 0 (the lowest) and that the regime in power that year had been in power for 58 years.

The dataset contains a variable called **D4**. This is a flag variable that is set to 1 for years where there has been a regime change or years that are "the final year of a multi-year regime transition."[101] All years that have a **D4** of 1 have a *Polity transition record*. It includes information about when the regime that came to power after the regime change began and when the one that was in power before the regime change ended.

[95] R Core Team 2019.
[96] See Hornik 2020.
[97] The code is available at `https://github.com/nat13sbe/reputation_and_regime_changes`.
[98] See Center for Systemic Peace 2019a.
[99] Center for Systemic Peace 2019b.
[100] See Marshall, Gurr, and Jaggers 2019, p. 1.
[101] See ibid., p. 35.

The Polity transition record contains a column called **REGTRANS**. This provides information about the type of regime transition. A scale of -2 to 3 is used, where -2 denotes an "adverse regime transition" and 3 denotes a "major democratic transition." A **REGTRANS** value of 2 means that a "minor democratic transition" has taken place, and -1 and 1 means negative and positive regime change, respectively. Finally, 0 denotes "little or no change in Polity score." There are also "special auxiliary codes" for state demise (98), state creation (99), state disintegration (96), state transformation (97) and interruption (66).[102]

Some examples of entries with **REGTRANS** values of 0 are France in 1986, Poland in 2002, the United States in 2016 and the United Kingdom in 2016.[103] All such cases will be filtered out in the program in order to count these as the same regime before and after that year. There is of course a danger that some large regime changes have resulted in very small changes in democracy level. These will then still be counted as being the same regime before and after that year. This is a trade-off that has to be made. The main benefit from doing it before adding ages is that regimes will appear longer, making more of them pass through the age requirement filtering.

The second dataset comes from Freedom House, who annually publishes its report *Freedom in the World* where they assess the state of civil liberties and political rights in different countries and territories. In all, 209 countries and territories are assessed. Data is available for all years since 1972.[104] For this thesis, I am using their dataset of historical data called *Country and Territory Ratings and Statuses, 1973-2019*.[105]

Freedom House evaluates countries on several levels: scores, ratings and statuses. At the highest level, all countries are categorized as having one of the statuses Free, Partly Free or Not Free. At the most detailed level, they receive scores on 25 indicators. 10 are in the political rights domain and 15 are in the civil liberties domain. These are then combined into a civil liberties rating and a political rights rating. The ratings go from 7 to 1 respectively, with 7 representing the worst human rights standards.[106]

Since I will be working with means I want to mention something about the nature of the ranking. It is clear that a ranking of, say, 6, is worse than a ranking of 4. There is, however, no indication in the Freedom House methodology page that a state with a ranking of 3 is as much better than a state with a ranking of 5 as a state with a ranking of 4 is better than a state with a ranking of 6. The question is whether the scale they use is ordinal or cardinal – whether the distance between ranks is consistent. In order to conduct a study like this, however, it has to be assumed that the rankings are comparable in this way to some extent.

The third and fourth dataset are spreadsheets provided by the United Nations Office of the High Commissioner[107], which I will from now on refer to as the "UN datasets." They contain data about when various states ratified the CCPR and the Optional Protocol,

[102]See Marshall, Gurr, and Jaggers 2019, pp. 35–36.
[103]Center for Systemic Peace 2019b.
[104]See Freedom House 2019a.
[105]Freedom House 2019b.
[106]See Freedom House 2019c.
[107]Office of the United Nations High Commissioner for Human Rights 2020.

respectively. They seems to only contain information for currently existing states, but since many of the regimes that are relevant for this study were in power in states that no longer exist, the datasets need to be complemented.[108] The columns in the UN datasets that I use are the ones with the name of the states in question and the ones named "Date of Ratification/Accession."

4.3. The relation between Freedom House and the CCPR

This thesis is concerned with compliance with the CCPR. There is no easy way to measure compliance. Court cases are rare and the self-reports of states may be biased. Non-governmental organizations study the respect for human rights in the world and may be more objective. I have elected to use the Freedom House dataset as a measure of compliance in this thesis. It has data for most of the states of the world and it goes back many years. However, the data must also deal with the same human rights as those that are protected by the CCPR in order to be useful. Let us therefore look at the connection between the data and the treaty.

According to Freedom House, it bases its methodology on the Universal Declaration of Human Rights.[109] The Universal Declaration was written by the UN Commission on Human Rights, whose initial work after the Second World War was focused on creating an international bill of rights. The first document to come of their work was the Universal Declaration, which was adopted by the United Nations General Assembly in 1948. Following that, they were instructed by the General Assembly to work together with the Economic and Social Council of the United Nations to produce a draft *covenant* of human rights, which finally resulted in the International Covenant on Economic, Social and Cultural Rights (CESCR) and the International Covenant on Civil and Political Rights (CCPR), which were both adopted by the General Assembly on December 16, 1966. For the latter treaty, an optional protocol was added to provide for a complaints procedure for individuals.[110]

Articles 2 through 21 of the Universal Declaration contain civil and political rights and most of them were transformed into the CCPR. The following 6, articles 22 through 27 dealt with economic, social and cultural rights, and were incorporated into the CESCR.[111]

One thing we learn from this brief history is that the connection between the CCPR and the Universal Declaration is strong and the CCPR (together with the CESCR) seem to have been developed as the treaty format of the Universal Declaration. Together, they form the International Bill of Human Rights.[112]

On the one hand, this strong connection between the CCPR and the UDHR indicates that Freedom House indeed attempts to study the same rights as those in the CCPR but on the other, some of those rights seem to be contained in the CESCR. However, when looking closer at what kind of indicators Freedom House uses, it becomes clear that it

[108]The details about the complementation are in section 4.4.4.
[109]See Freedom House 2019c.
[110]See Tomuschat 2019, pars. 3–6.
[111]See Charlesworth 2008, par. 11.
[112]See Riedel 2011, par. 1.

is mainly the rights within the CCPR which are measured. The rest of the section is devoted to this matter.

I mentioned earlier that Freedom House keeps scores within the two domains *political rights* and *civil liberties*. One might assume that since the names of the domains correspond perfectly to the name of the International Covenant on Civil and Political Rights, they will also correspond in what rights they are concerned with. Even if it seems likely, it is better to try to verify it based on their contents.

Freedom House goes into great detail about the questions it uses to determine the scores of states in the 25 indicators.[113] There is not enough space to make a full comparison of its methodology to the articles of the CCPR here but I will show that there is reason to believe that they overlap to a significant extent.

First I look briefly at the indicators for political rights. They are grouped into four categories, Electoral Process, Political Pluralism and Participation, Functioning of Government and Additional Discretionary Political Rights Question.[114]

One question from the Political Pluralism and Participation group reads:

> Are the people's political choices free from domination by the military, foreign powers, religious hierarchies, economic oligarchies, or any other powerful group that is not democratically accountable?[115]

This clearly corresponds to article 19 (1) of the CCPR (freedom to hold opinions without interference) and more generally to article 19 as a whole, article 21 (right of peaceful assembly), article 22 (freedom of assembly) and article 18 (2) ("No one shall be subject to coercion which would impair his freedom to have or to adopt a religion or belief of his choice"). Another question from the same group is:

> Do various segments of the population (including ethnic, religious, gender, LGBT, and other relevant groups) have full political rights and electoral opportunities?[116]

which corresponds to some extent to articles 25 concerning equal opportunity for political participation and article 26 (protection from discrimination "on any ground such as race, colour, sex, language, religion, political or other opinion, national or social origin, property, birth or other status").

The questions in the "electoral process" group are concerned with such things as the right to vote and the right to run as a candidate, both for the position of head of government and for positions as legislative representatives. The third indicator in the group is about the fairness of electoral laws and frameworks.[117] These questions correspond directly to article 25.

[113]See Freedom House 2019c.
[114]See ibid.
[115]See ibid.
[116]See ibid.
[117]See ibid.

Finally, the indicators in the "functioning of government" group deal with transparency of government, corruption, and the power of elected officials to govern without interference from unelected state actors, criminal gangs, foreign governments, etc.[118] These can potentially be seen as necessary preconditions to fulfill the provisions of article 25 (the right to take part in public affairs, vote and be elected).

It seems that the political rights indicators of the Freedom House survey correspond well to the political aspects of the CCPR. Freedom House mentions that they assume that "freedom for all people is best achieved in liberal democratic societies"[119] but it does not seem to me that this confessed bias is in conflict with the provisions of the CCPR. It might, however, make the Political Rights rating very similar to the polity index from the Polity dataset, which would have effects on how these human rights, as measured by Freedom House, change during regime transitions. More on this later.

When it comes to to civil liberties ratings, there are 15 scores, divided into the groups *Freedom of Expression and Belief*, *Associational and Organizational Rights*, *Rule of Law* and *Personal Autonomy and Individual Rights*.[120]

Freedom of belief is protected in articles 18-19 and freedom of expression is specifically protected in article 19, which covers the first group. The second group is covered by articles 21-22. Freedom of association and organization is directly protected in article 22 and the freedom of assembly is protected in article 21. The third category – rule of law – is extensively protected in articles 6, 7, 9, 14, 15, 16, 17 and 26. Personal autonomy in its most direct form is protected in article 17 ("unlawful interference with his privacy, family, home or correspondence"). Further questions that Freedom House looks at within this category are the freedom to move within the states and to exit the state (protected by article 12) and the equal enjoyment of those rights between women and men (which is required by the non-discrimination article 26). One indicator in this group is more concerned with economic rights and so might be better at measuring compliance with the CESCR ("Are individuals able to exercise the right to own property and establish private businesses").[121] Although there is also protection for some basic economic rights in the CCPR, such as the right to own property in article 1, although this article is directly concerned with peoples rather than individuals. Choice of marriage partner, on the other hand, is protected in article 23. Protection from economic exploitation can be found for children in article 24 and generally in article 8.

In 2016-2017, Freedom House revised its methodology and made some changes to it that were implemented from the 2018 survey onwards. The changes made after the review were small and it assures us that "basic structure and most methodology questions remained the same." However, the largest change was the addition of gender-related "guidance questions" for all the indicators, which is an important part of the rights of the CCPR.[122] Since I am using historical data, which, as far as I have understood, is not revised retroactively, the overlap with gender equality will be limited for this study.

[118] See Freedom House 2019c.
[119] See ibid.
[120] See ibid.
[121] See ibid.
[122] See ibid.

All in all, it seems that the correspondence between what Freedom House evaluates and the rights the CCPR protects is good. There are certain areas that exist in one but not the other but in general the overlap is large. Hathaway, in her study, also looked at compliance with the CCPR and used data from Freedom House although the dataset she used is called *Comparative Survey of Freedom* and is only evaluated with a "civil liberties checklist."[123] Without appealing to authority, the fact that others have found almost the same way to measure compliance with the same treaty before also indicates that it is the best choice.

One further reason to use the Freedom House dataset is that it provides a long data series (with rankings starting from 1972[124]) that include all states in a way that is easy to interpret for computer programs.

One more aspect to consider when assessing the sensibility of using the Freedom House dataset is the temporal aspect. I want to have data that shows objectively the level of respect for the CCPR rights for any given year *that year* – not something inferred from the general level in the surrounding years. Freedom House says:

> A score is typically changed only if there has been a real-world development during the year that warrants a decline or improvement (e.g., a crackdown on the media, the country's first free and fair elections), though gradual changes in conditions—in the absence of a signal event—are occasionally registered in the scores.[125]

I will proceed under the assumption that there is good reason for being careful about changing scores and that human rights respect generally changes little unless there are major crackdowns, reformations or the like and that the rankings therefore indeed reflect the current level for any year. Hathaway also suspects that such "bureaucratic inertia" exists. It is due to individuals and institutions getting used to that kind of means of control and the more they are used, the more accepted they become, even after the original reason for using them no longer exists.[126]

4.4. Description of the code

4.4.1. Overall structure

The first two hypotheses from section 3.5 are that ratifiers will on average see more of an improvement in human rights after a regime change than nonratifiers and that this high level will not be maintained, resulting in a relative decline over the first few years for ratifiers as compared to nonratifiers. The methods by which the hypotheses will be tested are similar in some ways. Both are comparing the index or mean index of two time periods, and regime changes are involved. Both have to be divided into populations of ratifiers and non-ratifiers and need to take into account the ages of individual regimes.

[123] See Hathaway 2002, p. 1975.
[124] See Freedom House 2019b.
[125] See Freedom House 2019c.
[126] See Hathaway 2002, p. 2003.

Thus, the two tests benefit from being done in the same program, with access to the same specific functions to accomplish the aforementioned tasks.

At the center of the workings of the program are two variables: `conreg` and `sinreg` (a variable, in programming, is data associated with a label). `conreg` stands for "consecutive regimes". When testing hypothesis 1, I need to look at two consecutive regimes because I want to know the index at the end of the first one and at the beginning of the second one, so `conreg` should be though of as associated with hypothesis 1. `sinreg` stands for "single regimes". It is associated with the second hypothesis, where I only need to look at one regime at a time since I am only interested in different periods of time within that regime. I may also refer to the tests that use `conreg` as "test 1" and the ones that use `sinreg` as "test 2".

The two variables can be though of as spreadsheets stored within the program. In R, this data type is called a "data frame." `sinreg` contains one regime per row and `conreg` contains two consecutive regimes per row. Each row will also be associated with columns saying whether the regime or regimes were a member for the full period studied or if they were *not* a member for the full period studied. It will also contain the average Freedom House index for each of the periods studied. The columns are added later on once the information is combined.

Freedom House has data from 1972 onwards[127] so regimes that ended before that year cannot be included in the study. I therefore filter out regimes that began before the year 1946. Technically, it would have been more correct to use the end year of regimes for the filtering and exclude those regimes that ended before 1972 but due to the structure of the dataset this method removes the need to deal with a lot of special cases and either way, decolonization started around the end of the Second World War so most states with major regime changes should be expected to not even have existed before that year. Right after loading the data from the external files, a copy of the Polity dataset is created with the name `regime_changes` with regimes before 1946 filtered out and only regimes with Polity Transition Records kept.

4.4.2. Special cases

The first thing that happens in the program, after additional packages have been loaded, is that the raw datasets are imported. Then work starts with the Polity dataset. The goal of this part of the program is just to identify which regimes fit the criteria to be evaluated. These are: a minimum ages of 10 years for `sinreg` and two regimes each having a minimum age of 3 years for `conreg`. Consequently, the ages of all regimes need to be calculated so that the ones that fulfill the criteria can be selected. The raw data contains some issues with the begin or end date of certain regimes that will produce errors for the function calculating ages unless they are dealt with so before adding the ages, these special cases are handled. This is done in a copy of the raw data before it is divided into the two variables. The special cases are listed here, with explanations of why they were excluded or modified:

[127] Freedom House 2019b.

- Polity uses the name "Sudan-North" for Sudan from 2011, probably to make clear that a new state was created in the north too after the South split off that year.[128] For the purpose of this thesis, I am concerned with the regime in power and so I changed the name to Sudan to treat it as the same state before and after 2011. I also removed the row denoting the regime change of that year so that before and after that year is treated as the same regime.

- Kosovo is completely excluded from the study because there is no Freedom House ranking for it for the first years of its existence.[129]

- There seems to be an error in one of the rows for Myanmar, with the start year shown as 6 and no month specified.[130] In accordance with how the rest of the data is structured, I changed the begin-year to 2016. Since a regime that began in 2016 is still not old enough to be included in the study, the month it began does not matter and so I set it to January.

- Syria in 1961 is flagged as having a regime change but there is no begin date for the regime so it is not possible to calculate its age. This entry was removed since due to the next regime taking over after just three years it would have been filtered out regardless.[131] The discrepancy is probably due to how the Center for Systemic Peace dealt with the United Arab Republic, Syria's union with Egypt around this time period.

4.4.3. Regime filtering

The program has a function for adding ages to regimes. Due to there being a number of exceptions from the general pattern of having the date a regime began on the row for that year and the date it ended on the same row as the begin date for the next regime, there are some conditionals and exceptions in the function. It is not necessary to go into detail about all of them. I will just mention one discretionary choice I had to make in determining an end date for regimes that are still in power. I set their end date as December 31, 2018. 2018 is the latest year that is covered by the Freedom House dataset and there is no reason to put the end date earlier. There are no adverse consequences to choosing a false end date for the purposes of this study.

A note also has to be made about date accuracy. Polity provides the exact date regimes begin and end and the UN dataset has exact dates of ratification. Freedom House, on the other hand, generally survey whole years and provides a rating for the year as a whole. However, when determining the ages of regimes, I use the detailed information in the Polity Transition Record, because there is no reason to lose accuracy when it is possible to keep it.

[128] Freedom House 2019b.
[129] Ibid.
[130] Ibid.
[131] Center for Systemic Peace 2019b.

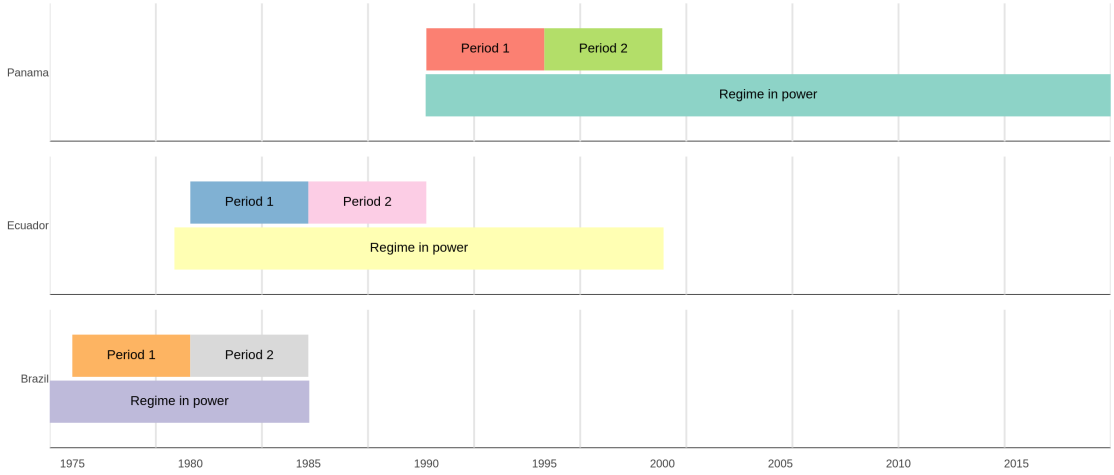


Figure 1: Visualization of periods constructed for three entries in the `sinreg` variable.

Before adding the age, I remove regime changes with a **REGTRANS** value of 0, for reasons discussed above in section 4.2.

After the age of each regime has been added, the filtering is simple. I create the `sinreg` by selecting all rows where the age of the regime is greater than or equal to 10 years and where the begin year of the regime is greater than or equal to the first year with Freedom House data, 1972. I also keep only those columns containing data relevant to the rest of the study. Additionally, columns indicating when the periods under study start and begin are added. For the first years of the regime, I use an interval of 5 years, starting from the year the regime began. For the period to compare with, I use the year 5 years from the start year to year 10 of the regime. Table 2 shows the column names of `sinreg` with the first row of data at this point in the program. The constructed periods to analyze for three entries in the `sinreg` variable are visualized in figure 1. The periods always start on the first day of the year and end on the last because the data that will be associated with the period is only available on a per-year basis.

country	year	bdate	edate	regtrans	age	p1.bdate	p1.edate	p2.bdate	p2.edate
Haiti	2006	2006-05-14	2018-12-31	3	12.63	2007-01-01	2011-12-31	2012-01-01	2016-12-31
Dominican Republic	1978	1978-08-17	1996-08-16	3	18.00	1979-01-01	1983-12-31	1984-01-01	1988-12-31
Mexico	1997	1997-07-06	2018-12-31	3	21.49	1998-01-01	2002-12-31	2003-01-01	2007-12-31

Table 2: Example of `sinreg` appearance before adding ratification and index data, with the first three regimes displayed.

The other variable, `conreg`, is a little more complex to create. The first step is to test all regimes that are consecutive to another regime and see if *both* of them have an age larger than or equal to 3 years. If they have, they are added to a new data frame variable

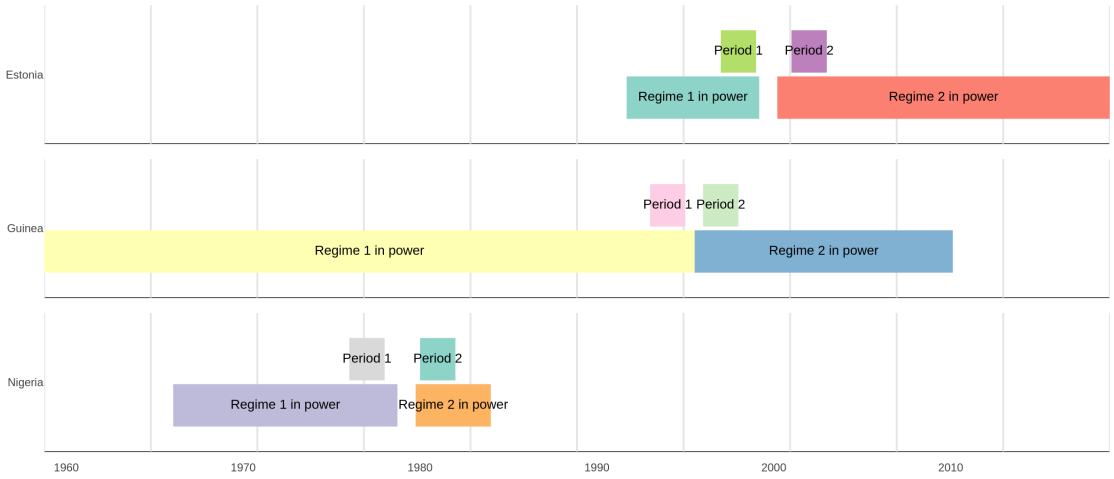


Figure 2: Visualization of periods constructed for three entries in the `conreg` variable.

with information about both regimes on the same line. Once that is accomplished, those regimes where the time passing between them is greater than 3 years are removed because there is a risk that other factors in the world that also affect the level of respect for human rights within the state have changed too much if too much time has gone by. A number larger than 3 would probably have been acceptable but only 8 regimes are filtered away in this step so not much is lost by doing this. Columns for determining what periods of time to study the index for are also selected here. The first period is from 2 years before the year of the regime change to one year before it (since, as discussed in section 4.2, the ranking for the year of the regime change tends to be determined by the new regime). The second period under study is an interval of 2 years, starting the year of the regime change. At this point in the program, `conreg`, with its first row, will look like table 3. The columns for the ages are, however, omitted due to lack of space on the page. The periods of three of the other entries of the variable are visualized in figure 2.

country	regime1. bdate	regime1. edate	regime2. bdate	regime2. edate	regtrans	p1. bdate	p1. edate	p2. bdate	p2. edate
Haiti	1991-09-30	1994-10-15	1994-10-16	1999-01-10	3	1992-01-01	1993-12-31	1995-01-01	1996-12-31
Haiti	1994-10-16	1999-01-10	2000-11-26	2004-03-08	-2	1997-01-01	1998-12-31	2001-01-01	2002-12-31
Haiti	2000-11-26	2004-03-08	2006-05-14	2018-12-31	3	2002-01-01	2003-12-31	2007-01-01	2008-12-31

Table 3: Example of `conreg` with the first three regime change in the list before ratification and index data is added. The columns **regime1.age** and **regime2.age** are not shown due to lack of space.

4.4.4. Adding and processing the Freedom House data

In this next step, the Freedom House dataset and UN datasets are used to add information about mean indices over the studied time periods and whether the state was a ratifier, not a ratifier or joined the treaty during the beginning of the first period and end of the second.

The data processing for test 1 and test 2 is the same so they are processed in close connection with each other. For both tests, four columns are added to the associated data frame (`conreg` and `sinreg`). These are called **p1.mean**, **p2.mean**, **mean.index.change** and **trend** and they store the mean Freedom House index of the first and the second periods studied, the difference between them and a column saying whether the index change was an improvement, a decline or no change in the human rights level.

In this stage of the process, issues with dates have to be dealt with again. Freedom House in general assigns one ranking per state per year. Regardless of whether there has been a regime change in the middle of the year, it provides a ranking for the year as a whole. The Freedom House methodology does not say whether the ranking is determined by conditions under the new or the previous regime. There are also other possible ways in which they could deal with it, for example using the average of the rankings the two regimes would have had or even a weighted average determined by the fraction of the year they were in power. Since it is left out, I choose the safer way to deal with the problem and only analyze the years around the regime change in test 1 and begin the first period of analysis of test 2 the year after the regime change.

One further complication is that the time period covered by Freedom House is not always January 1 to December 31. Between 1983 and 1989, the time period was November one year to November the next.[132] I chose to treat this as the rating for the second of those years, from January to December. There is also one special case that has to be dealt with separately. The 1982 survey edition and the 1983-1984 survey edition covers January 1981 to August 1982 and August 1982 to November 1983, respectively.[133] That is approximately 3 years. Treating it as the ratings for individual years would leave a gap with one year of no ratings at all. My solution is to treat the earlier ranking as being for 1981, the later as being for 1983 and creating a column in the dataset for 1982 that is the average of the two surveys.

The state known as Yugoslavia in the Freedom House dataset up until 2002 changed its name to Serbia and Montenegro in 2003 but there was no regime changes. The regime found by the program is the one that started on October 27, 2000 and ended on June 2, 2006. I deal with this by copying the rankings for Serbia and Montenegro from 2003 to the row for Yugoslavia. In the list of regimes, the state is also called Yugoslavia so when the function for getting the index for the state is called, it looks on the row for Yugoslavia in the Freedom House dataset.

Once the Freedom House data has been prepared, which also entailed changing the names of some states to correspond to what those states are called in the Polity dataset

[132] Freedom House 2019b.
[133] Ibid.

so that they can be matched properly, the rest is easy. I have written one function called `get_average_index()` which takes a start year, an end year, a state name and the data frame of indices and returns the average Freedom House index for those years and that state. These produce the **p1.mean** and **p2.mean** columns. Subtracting the former from the later produces the **mean.index.change** column and then I have written a function called `get_trend_column()` which takes the **mean.index.change** column as argument and returns a column with one of the values "IMPROVEMENT," "NO CHANGE" or "DECLINE" depending on the trend that can be seen from the average index change.

4.4.5. Adding the ratifier column

The next step is to produce the ratifier column for `conreg` and `sinreg`. Just as with the Freedom House dataset, the UN dataset uses different names for some states so these need to be replaced. Another problem is that even if the United Nations seems to track ratification by all its member states, the Polity dataset contains states other than that. Article 48 (1) of the CCPR says that:

> The present Covenant is open for signature by any State Member of the United Nations or member of any of its specialized agencies, by any State Party to the Statute of the International Court of Justice, and by any other State which has been invited by the General Assembly of the United Nations to become a Party to the present Covenant.

so it would be fair to assume that if any non-member of the United Nations fulfilling the criteria had ratified, it would also show up in the UN dataset. Additionally, since regime changes are by their nature historic, I might also need to know the ratification dates of no longer existing states. Some specific examples are North Vietnam, South Vietnam, Czechoslovakia and West Germany. For all cases not in the UN dataset, I look at the historic data from the United Nations Treaty Collection.[134] If the states do not show up in the Treaty Collection, I conclude that it is not a member. For all states that are not members, be it because they lack a ratification date in the UN dataset or that they are completely missing as in the cases just explained, I set the ratification date to December 31, 2020. This ensures that any regime from these states will be deemed not a ratifier since its ratification date is in the future.

The states that fulfill the requirements for regime changes in either of the two tests but do not appear in the UN dataset nor the historic data from the UN Treaty Section are Taiwan, Kosovo, North Vietnam, South Vietnam, North Yemen.[135] Since they are not members of the CCPR, they also cannot be members of the optional protocol[136] so these are treated as non-members in both cases. Additionally, South Yemen was a member of the CCPR but not of the Optional Protocol.[137]

[134] It can be found by going to `treaties.un.org` and going to "Registration & Publication" and then "UN Treaty Series" and searching for the treaty there. Clicking on it should produce a list of all depositions concerning membership of the treaty.
[135] United Nations Treaty Section 2020b.
[136] See article 8 (2) of the Optional Protocol.
[137] United Nations Treaty Section 2020c.

Some no longer existing states did ratify during their existence. For the CCPR, these states are West Germany (1973-12-17), Yugoslavia (1976-06-02), Czechoslovakia (1975-12-23) and South Yemen (1987-02-09).[138] For the Optional Protocol, the only case was Yugoslavia (1990-03-14).[139]

According to article 49 (1) of the CCPR, the date of effect of ratifications is 3 months after the ratification or accession has been deposited. Some states joined the covenant before its original entry into force on March 23, 1976. I of course treat these as having the obligations under the treaty from the entry into force of the treaty itself, unless they ratified less than three months before.

In the code, I add 3 months to the ratification date and put the results in a new column called **date.of.effect**. Then I change all dates in the column that are still earlier than the entry into force of the treaty to the date of entry into force of the treaty. According to article 9 of the Optional Protocol, the date of effect is also three months after deposition and the entry into force is according to the ingress the same as that for the CCPR. So the same function can be used for it.

Finally, the *ratifier* column is produced by a function called `get_ratifier_column()` which takes a data frame of ratification dates and one of the data frames `conreg` or `sinreg` and produces a vector where each element can take the value "YES," "JOINED" or "NO" depending on the ratification status during the analyzed time periods. JOINED is for those states that joined the treaty between the beginning of the first period and the end of the second period. For `sinreg` that means between January 1 the year after the regime began and December 31 the year 10 after that year (cf. table 2) and for `conreg` it means between January 1, 2 years before the regime change and December 31 the same number of years after it (cf. table 3). If the ratification date is before that, the element in the vector corresponding to that row of the data frame takes the value YES and if it is later than that time interval, the element takes the value NO.

4.4.6. Running the tests

At this point in the code, the preparation is done and we are ready to get into the actual statistics that will reveal whether the hypotheses hold up or not. Selecting the samples for analysis is done in different functions depending on whether I run the initial test, in which equal number of democratizations and autocratizations are needed, or the main test, in which the ratio of democratizations to autocratizations needs to be the same for both samples while making sure that the samples are as large as possible.

For the initial test, the selection is done by taking all regimes or regime changes from the smallest of the groups of democratizations and autocratizations and then randomly sampling the same number of regimes or regime changes from the other populations.

The process for selecting samples for the main test is slightly more complex. The samples of ratifiers and non-ratifiers should be as large as possible, while at the same time keeping the same ratio of democratizations to autocratizations. For one of the samples, all democratizations and autocratizations will be used, and for the other, with

[138] United Nations Treaty Section 2020b.
[139] United Nations Treaty Section 2020c.

a larger ratio, only some of its democratizations or autocratizations will be used. The ratio for both groups is therefore calculated, and the smaller of the two selected. Then random regimes or regime changes are selected from `conreg` or `sinreg` (depending on which of my two first predictions are being tested) to produce a new dataset with the right ratios.

The plots for each test and numerical information about the samples are produced by calling functions written for that purpose. For the initial test, samples are selected, the numeric information printed to the command line and a bar plot showing the fraction of improvements, declines and no change in human rights level produced. For the main test, the procedure is the same but a density plot may be requested instead of the default bar plot.

4.5. Differences from other studies

Hathaway, in her 2002 paper, also does a time series analysis of the effect of ratification on compliance. Hers is a much larger study, as detailed in section 2.4.1. Her study is more advanced in at least a couple of ways. Firstly, she uses a *time-trend variable* to control for potential trends of better human rights in the world over time.[140] I do not do this, mainly because I am studying so short periods of time that the global improvement over those years would be minimal. Additionally, I only look at the difference in average index over nearby time periods so it should not matter whether I am looking at a regime from 1980 or from 2010 as long as the rate of change in global human rights level is fairly constant. Secondly, she accounts for the possibility that the effects of treaties are "cumulative and long-term" and does that by measuring the ratification variable as a sum of the number of years since ratification.[141] In other words, she sees the "degree of ratification" as increasing over time. One of the assumptions that I inherit from Guzman is that states are unitary actors. A unitary actor would be able to change its behavior in a short time-span which I take to mean that they can quickly improve their level of human rights. Then, there should be no "degrees of ratification" but only a yes or no ratification status. This is further supported by e.g. the fact that after the major autocratic transition in Gambia in 1994,[142] the civil liberties ranking jumped from the one-point-from-perfect score of 2 to a one-point-from-worst 6.[143]

4.6. Statistics

My hypotheses predict that there is a difference in human rights trend between ratifiers and nonratifiers for certain periods in their existence. This is common for both the test with consecutive regimes and the one with single regimes. However, there are multiple angles to test this from. Two will be used: one that looks at the exact change between

[140] See Hathaway 2002, p. 1990.
[141] See ibid., p. 1990.
[142] See Center for Systemic Peace 2019b.
[143] See Freedom House 2019b.

the periods, with magnitude, and one that uses different categories that the changes can be divided into (improvements, declines and "no changes").

Before moving on to the two types of tests, some terms need to be introduced. A population is defined as "the complete set of individuals, objects, or scores that the investigator is interested in studying"[144] which in my case would be all regimes that have existed for the single regimes tests and all regime changes that have occurred for the consecutive regimes tests. A sample is "a subset of the population".[145] I will use samples with equal ratios of democratization to autocratizations in order to ensure that the results are not skewed due to one population containing a larger fraction of democratizations than the other.

A *variable* in statistics, as opposed to in programming, is "any property or characteristic of some event, object or person that may have different values at different times depending on the conditions."[146] The variables that I am using are the ratification status of states, whether the regime change was a democratization or autocratization (or in the case of single regimes, whether the regime came to power in a regime change classified as democratization or autocratization), the rankings from Freedom House, and finally the difference between the mean index change of the two periods compared or alternatively, for the other test, what the trend in human rights between the periods was. These two latter variables are dependent variables, meaning that they may depend on the former if the hypotheses hold up. The other variables are independent variables, meaning that they do not change when other variables change.

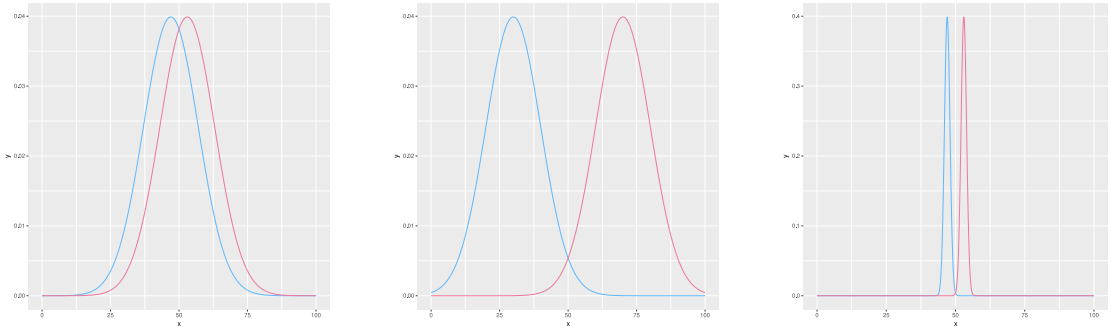
4.6.1. The t-test

For the first type of test – the one using the real change in mean index – the hypotheses predict that ratifiers and nonratifiers have different *means* of the mean index change for all regimes or regime changes belonging to either group. However, even if I find that there is a difference between the groups, it could be due to chance. When sampling regimes or regime changes randomly from a population, it will sometimes occur that I get more that have a large mean index change and sometimes that I get more with a small mean index change as compared to the mean of the underlying population. Then, even if the "real" mean for the underlying population (the ratifiers) is the same as the one I am comparing with (the nonratifiers), it will look as if they are not. That is why I need to test the statistical significance of the result.

To illustrate why just knowing the means is not enough, I have drawn two normal distribution curves in figure 3a. The height of the curve at any point represents the likelihood of getting the value in that region of the horizontal axis (or rather, the likelihood of getting a value within any interval on the horizontal axis is equal to the area under the curve in that interval). The curves have different means (47 and 53) but probably not different enough that it can be ruled out that they are actually taken from identical populations when the large variance is considered. In figure 3b, the variance is the same but

[144]Pagano 2004, p. 6.
[145]Ibid., p. 6.
[146]Ibid., p. 6.

the difference between the means is larger so it is less likely that the difference between the populations is just due to chance. In figure 3c the difference between the means is the same as in figure 3a but the variance is much smaller, so here too the difference is unlikely to be due to chance. There are formal tests for determining exactly how likely it is that the difference is due to chance, and the one that will be used here is the t-test. The test produces a p-value which represents the likelihood that the results are due to chance. For example, a p-value of 0.05 would mean that the likelihood of the results being due to chance is 5%.



(a) Two normal distributions with means of 47 and 53 and both with variance of 10

(b) Two normal distributions with means of 30 and 70 and both with variance of 10

(c) Two normal distributions with means of 47 and 53 and variances of 1.

Figure 3: Illustrations of how population difference can be statistically significant.

The t-test is different depending on whether the compared groups are independent or not. Here I use the t-test for independent groups. This compares a *test group* – in my case the ratifiers – with a *control group* which would be the nonratifiers.[147]

There are two assumptions when using the t-test for independent groups. First of all, the populations that the samples are taken from should be normally distributed, and second, the variance of the populations should be equal. However, the t-test is very robust, meaning that it will still work even when the assumptions are not fully met. If there are moderate violations of the homogeneity of the variance and the normality of the populations, the test may still be used as long as the size of each sample is at least 30.[148] As will be seen in the results section, the samples in my case tend to be more or less normally distributed and their variance is not very different which I take to indicate that the variance of the underlying populations are also similar.

A t-test can test for just seeing any effect at all, either positive or negative, or it can test for an effect that makes the mean index go in a specific direction. In my case, I have predicted that the change will be either towards a lesser or a greater mean index, depending on the test. If it turns out that even my predicted direction is wrong it may

[147]See Pagano 2004, p. 327.
[148]See ibid., pp. 338–339.

still be interesting to know whether there is any difference at all between the populations. Then a two-sided test should be used.

4.6.2. The chi-square test

All tests are additionally performed using categories created according to the sign of the mean index change. The categories are IMPROVEMENTS (negative change), NO CHANGE (no change in mean index) and DECLINE (positive change). Figure 4 shows visually how all the regime changes in `conreg` that were members of the CCPR since at least the start of the their first period would be divided according to this rule.

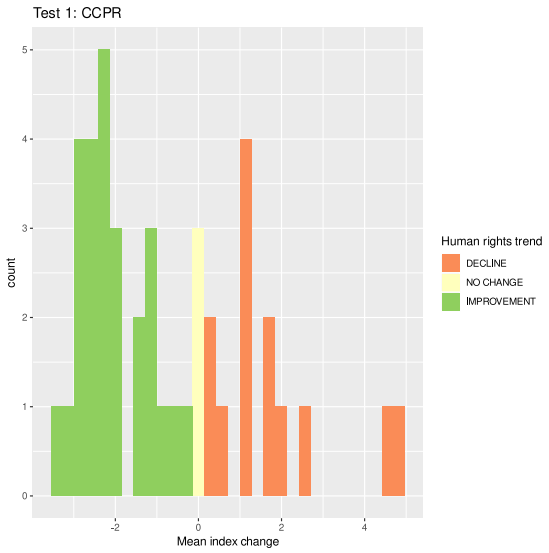


Figure 4: Visualization of how trend categories are created based on `conreg`.

There is always a risk that the proportions between the categories for the sample of ratifiers is different than for the sample of nonratifiers even when in reality, the underlying populations have the same proportions. To make sure that this risk is small, a significance test must be used. The t-test is not applicable here because the data is in categories and not in a numerical format. However, the χ^2 test can be used. It is a type of inference test that works well for data in the format of frequency numbers for mutually exclusive categories.[149] This is what I have. Any entry will be classified as only one out of IMPROVEMENT, NO CHANGE or DECLINE. It can also be only one of "ratifier" or "nonratifier".

The χ^2 test requires that I have both observed frequencies and expected frequencies. The expected frequencies are the ones one would expect to get when taking a random sample from the null-hypothesis population[150] – in my case the nonratifiers because if

[149] See Pagano 2004, p. 427.
[150] See ibid., pp. 427–428.

ratification has no effect, I would expect that the ratifiers would behave the same way as nonratifiers. The observed frequencies then are those of the ratifiers.

There are two assumptions underlying the χ^2 test. First, the groups have to be independent meaning that each subject can only be used once.[151] This is largely true for my test but sometimes there are several regime changes in the same state that and both fulfill my criteria for being included in the study. One way to get around this would be to exclude all but one regime change from each state. However, it might not be necessary since different regimes are always involved. The second assumption underlying the test is that the sample size is large enough to have a minimum frequency of 5 for the expected frequency of each category when the contingency table is larger than 2x2.[152] Mine is 3x2 since there are three categories for human rights trends and two for ratification status so I have to make sure that the expected frequencies are large enough.

4.6.3. Sample selection

When running the tests, I select samples from `conreg` or `sinreg` according to specific criteria. For the pure regime change test, the criterion is that there should be an equal number of democratizations and autocratizations. Accordingly, I take all the objects from the smallest of the groups and an equally sized, randomly selected sample from the other group.

For the main test, the regimes or regime changes need to be divided into ratifiers and nonratifiers so that they can be compared. This means that the samples will be smaller than in the pure regime change test. One way to make sure that there are enough regimes to analyze is to drop the requirement for equal number of democratizations to autocratizations. In order to make the samples comparable, the *ratio* of democratizations to autocratizations still needs to be the same. This might change the fraction of the regimes that experience improvements or declines in human rights, but what we want to know is just if the samples are *different* from each other so that is not a problem.

[151] See Pagano 2004, p. 440.
[152] See ibid., p. 440.

5. Results

5.1. Results for consecutive regimes tests

Hypothesis 1 predicts a positive effect of ratification on the level of respect for human rights immediately after a regime change. As mentioned before, this hypothesis is to be tested with periods of time in consecutive regimes, and these consecutive regimes are in the variable `conreg` in my program. I may therefore refer to the tests as the tests with `conreg` or the tests with consecutive regimes. In the plots below, the title "Test 1" is also used for all tests with this variable. The group of tests which these names all refer to, will be presented in the following three sections.

First, I look at a pure regime change test to see if regime changes in themselves tend to move the human rights trend in a specific direction. That is, whether we see more improvements or declines than would have been expected based on there being equal numbers of democratizations and autocratizations.

The section after that compares ratifiers of the CCPR with nonratifiers of it. This is done using the numerical value of the change in mean index from before the regime change to after it but also based on what category of trend the change belongs to (see section 4.6.2). The first method presents the results in histograms, while the second presents them in bar plots. Details about the plots are also presented in associated tables together with the p-values of the tests. For the histograms, the detailed information is the means of the samples. For the bar plots, it is the exact frequency and fraction size of each category for each group.

The third, and last section presents the results that were obtained when dividing the population by ratification status for the Optional Protocol. Apart from that, the information presented is the same as in the section before it. However, the nonratifiers of the Optional Protocol will include ratifiers of the CCPR unless I exclude them. I perform the test for both those scenarios. All tests are run three times in order to show that they are consistent across new sample taking.

5.1.1. Pure regime change test for consecutive regimes

The pure regime change test for consecutive regimes looks at whether the change in human rights tends in a certain direction when there is a regime change. It is reasonable to believe that autocratizations are associated with declines in the standards of human rights for states and that democratization are associated with improvements. Then, if we include equally sized samples of democratizations and autocratizations, we can know if regime changes in themselves have an effect by seeing if they have an equal number of improvements and declines. This is almost certainly true for political rights, and consequently I do not use these, as explained in earlier sections. When using only the civil liberties rankings, the results for this pure regime change test are as displayed in figure 5.

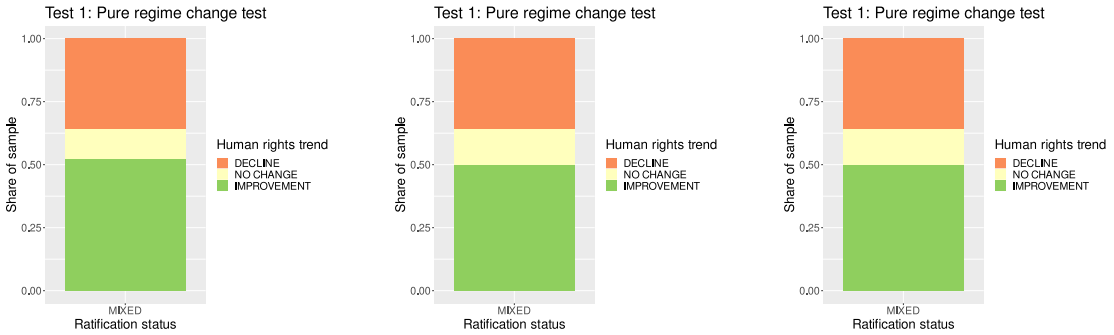


Figure 5: Bar plots showing the human rights trends when there is a regime change for a sample including equal number of democratizations and autocratizations.

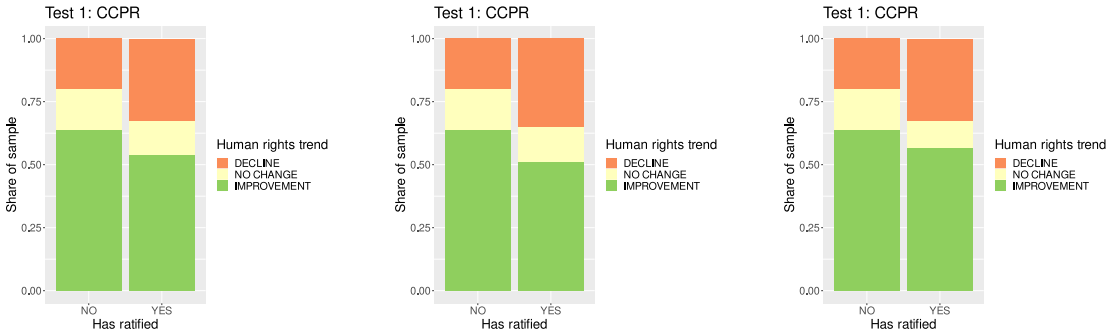
	Run 1	Run 2	Run 3
Improvements	22 (52.4%)	21 (50%)	21 (50%)
No change	5 (11.9%)	6 (14.3%)	6 (14.3%)
Declines	15 (35.7%)	15 (35.7%)	15 (35.7%)

Table 4: Numerical information for the tests in figure 5.

The three plots of figure 5 are produced with different samples among all the democratizations and autocratizations in `conreg` (although for the smaller of the groups, all of it is sampled). In total, each bar plot is based on 42 regime changes. The fraction of the plot made up by each trend category varies a little since regime changes are sampled at random, but overall they look similar enough. The exact makeup of each plot's 42 regime changes can be seen in table 4. We see that improvements are slightly more frequent than declines in all runs, with the former group making up around 50% of the total and the latter around 35%. There are also a number of cases that saw no change in their level of respect for human rights, which is somewhat surprising given that the system of government changed a great deal in all cases.

5.1.2. Consecutive regimes and the CCPR

In section 3.5, I predicted that there would be larger improvements in human rights in states that had ratified the CCPR compared to nonratifiers when there is a new regime and that human rights would receive less focus later on. Sometimes other circumstances will result in a net decline for ratifiers or a net improvement for nonratifiers but in general, it should be more common with improvements in human rights during regime changes for ratifiers than for nonratifiers. Using the trend categories, the results for this test are as seen in figure 6.



(a) Run 1 (b) Run 2 (c) Run 3

Figure 6: Bar plots showing the human rights trend for ratifiers and nonratifiers of the CCPR when there is a regime change.

	Ratifiers			Non-ratifiers
	Run 1	Run 2	Run 3	All runs
Improvements	20 (54.1%)	19 (51.4%)	21 (56.8%)	16 (64%)
No change	5 (13.5%)	5 (13.5%)	4 (10.8%)	4 (16%)
Declines	12 (32.4%)	13 (35.1%)	12 (32.4%)	5 (20%)
p-value (χ^2 test)	0.5603	0.4353	0.5264	

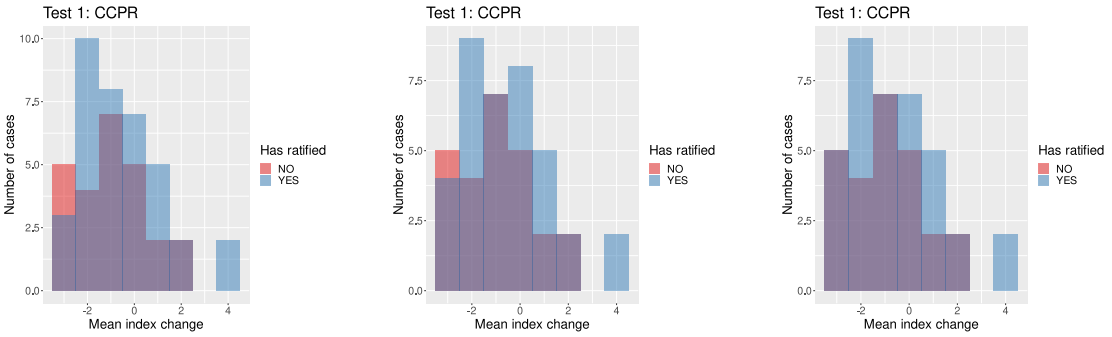
Table 5: Numerical information for the tests in figure 6 and associated p-values.

The first thing that can be said is that the results are more or less consistent over different runs of the test. This is obviously the case for the nonratifiers because the sample used is simply the whole population of nonratifiers. In order to match the ratio of democratizations to autocratizations, however, sampling is used for the ratifiers. The exact frequency of any group can be seen in table 5. Here, the consistency between runs is also visible as the difference between runs for any trend category never being larger than 1.

Looking at the plots, it seems as if the ratifiers have relatively few improvements and many declines. This is the opposite of what was predicted. However, the results are never significant at the 0.05 level, as can be see table 5. The p-values are the results of χ^2 tests performed on the samples for each run and the nonratifier sample is always the same. The ratifier sample varies and so the p-values for each run is placed in the ratifier columns.

All in all, improvements seem to be much more common than declines in both groups but this is an artifact of there being more democratizations than autocratizations. As we saw in the pure regime change test, the human rights trend tends to, in general, follow the direction of change in the system of government.

I have also produced histograms using the **mean.index.change** column of `conreg`. These are in figure 7.



(a) Run 1 (b) Run 2 (c) Run 3

Figure 7: Histograms showing the mean index changes of ratifiers and nonratifiers of the CCPR when there is a regime change.

	Run 1	Run 2	Run 3
Nonratifier mean	-0.8200	-0.8200	-0.8200
Ratifier mean	-0.4189	-0.3919	-0.4865
p-value (two-sided t-test)	0.3454	0.3176	0.446

Table 6: Sample means and p-values for the tests in figure 7.

As with the bar plot, the sample that varies between the runs of the test is ratifiers. It is more or less consistent across runs. The main thing to note is that the overlap between the groups is large, which indicates that they are not very different. There are more ratifiers than nonratifiers but the *shapes* of their bars are similar to those of the nonratifiers. The difference between the means of the groups is between 0.335 and 0.4281 points, depending on the run, with the mean of the mean index change for the ratifiers being the larger one, as can be seen in table 6. This indicates that ratifiers generally have more of a decline between regime changes, relatively speaking, but there is no statistically significant difference between the groups in any of the runs. The p-values varies between 0.3176 and 0.446, giving us at best a 31.76% chance that the difference is not due to chance. This is the likelihood that there is any difference at all, not that ratifiers would have a higher mean of the mean index change than nonratifiers (which would be even less likely considering that the means of the samples go in the opposite direction).

5.1.3. Consecutive regimes and the Optional Protocol

We will now look at the human rights trend for ratifiers and nonratifiers of the Optional Protocol to the CCPR. I first present the results for when the nonratifier group includes ratifiers of the CCPR who did not ratify the protocol and then the results for when these are excluded.

Figure 8 shows the results from three runs of the test with the less strict nonratifier sampling.

(a) Run 1 (b) Run 2 (c) Run 3

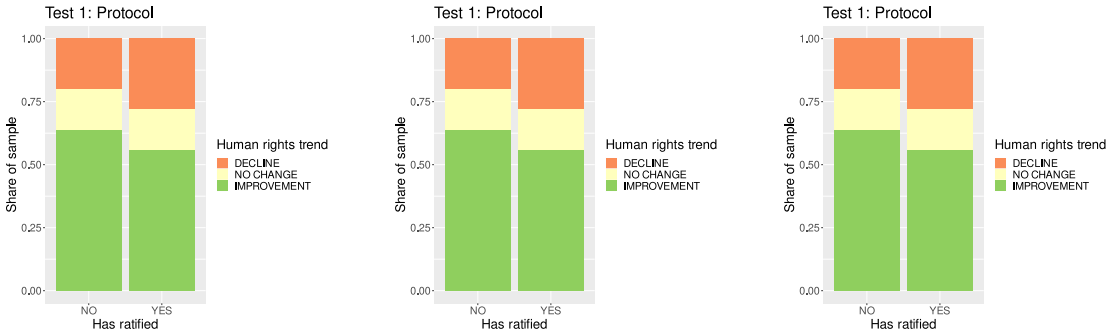
Figure 8: Bar plots showing the human rights trend for ratifiers and nonratifiers of the CCPR when there is a regime change, inclusive version.

	Ratifiers	Non-ratifiers		
	All runs	Run 1	Run 2	Run 3
Improvements	15 (57.7%)	26 (61.9%)	25 (59.5%)	26 (61.9%)
No change	4 (15.4%)	6 (14.3%)	6 (14.3%)	6 (14.3%)
Declines	7 (26.9%)	10 (23.8%)	11 (26.2%)	10 (23.8%)
p-value (χ^2 test)		0.9405	0.9873	0.9405

Table 7: Numerical information for the tests in figure 8 and associated p-values.

It can immediately be seen that ratifiers and nonratifiers are very similar. Whatever difference there is, it is obviously not statistically significant, but the p-values are still presented in table 7. They range from 0.9405 to 0.9873. As we learned in the pure regime change test, when there are equal numbers of democratizations and auotcratizations, improvements are not much more common than declines and make up about 50% of the total. The reason that we now see a large fraction of improvements is because there are 2.25 times as many democratizations as autocratizations.

When ratifiers of the CCPR but not the Optional Protocol are mixed in with nonratifiers of both, a large part of the this group has the same obligations as the ratifier group when it comes to what human rights they have to protect; they just lack the individual complaint mechanism. This may make the results of the groups very similar, as we saw in figure 8. I did predict, in hypothesis 3, that ratifiers of the protocol should see a stronger effect, but this would be even more visible if the group was compared with states having none of the obligations. Figure 9 shows the same test but with populations that either ratified both treaties or none of them. The associated data is in table 8.



(a) Run 1 (b) Run 2 (c) Run 3

Figure 9: Bar plots showing human rights trends for ratifiers and nonratifiers of the Optional Protocol when there is a regime change, strict version.

	Ratifiers All runs	Nonratifiers All runs
Improvements	14 (56%)	16 (64%)
No change	4 (16%)	4 (16%)
Declines	7 (28%)	5 (20%)
p-value (χ^2 test)		0.7919

Table 8: Numerical information for the tests in figure 9 and associated p-values.

Now there seems to be some difference between the ratifiers and nonratifiers but rather than showing more improvements for ratifiers as predicted, it shows that ratifiers of the Optional Protocol tends to see fewer improvements and more declines than nonratifiers of both treaties. This contradicts hypothesis 1.

Prediction 3 is that the protocol will increase the effect of hypothesis 1. For this to be true, the difference between ratifiers and nonratifiers needs to be relatively more towards improvements for ratifiers in this test than in the one presented in figure 6, with samples created based on CCPR ratification. In this case, since there were more declines and fewer improvements for ratifiers of the CCPR compared to nonratifiers, figure 9 needs to have a distribution of human rights trend for ratifiers closer to the same one as the nonratifiers or better than it. Looking at the plots, there seems to be no such difference.

For this test, the results happened to be the same in all runs, which shows that they are not dependent on what sample I happen to obtain. The p-value is 0.7919, as can be seen in the associated table. In other words, the results are not statistically significant.

I have also created histograms for this test, which can be seen in figure 10. These are created using the values from the **mean.index.changes** column of `conreg` for the groups.

(a) Run 1 (b) Run 2 (c) Run 3

Figure 10: Histograms showing the mean index change of ratifiers and nonratifiers of the Optional Protocol when there is a regime change, strict version.

	Run 1	Run 2	Run 3
Nonratifier mean	-0.82	-0.82	-0.82
Ratifier mean	-0.40	-0.36	-0.38
p-value (two-sided t-test)	0.3846	0.3374	0.361

Table 9: Sample means and p-values tests in figure 10.

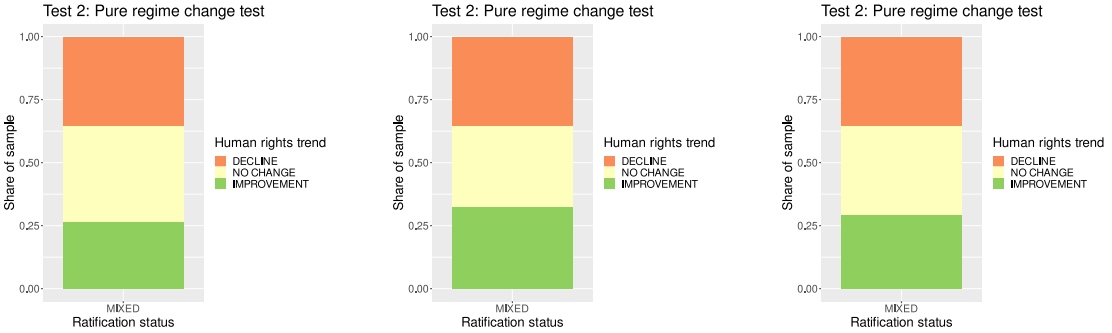
The mean value for the nonratifiers is -0.82 and for ratifiers it ranges between -0.36 and -0.40, which shows that the results are consistent across runs. Just as the bar plots previously, this contradicts hypothesis 1 (a lower index means better standards of human rights). The difference is not statistically significant but the p-values are smaller than for the test based on categories. They can be found in table 9 and range from 0.3374 to 0.3846. It is also possible to see in the plots that the groups overlap to a large extent.

5.2. Results for single regimes tests

The second one of my hypotheses was that ratifiers of the CCPR should see more declines the first years of the regime's existence compared to nonratifiers because they start from a higher level of respect for human rights due to the effects of hypothesis 1 and due to the fact that the cost of maintaining that high level will gradually increase. Since we did not see the initial improvements in the last section, the subsequent decline would probably also be nonexistent but it will be tested whether this really is the case.

5.2.1. Pure regime change test for single regimes

As for test 1, I begin by looking at what the trends tend to be when there are equal numbers of democratizations and autocratizations. The results are in figure 11.



(a) Run 1 (b) Run 2 (c) Run 3

Figure 11: Bar plots showing the human rights trend after regime changes for a sample including equal number of democratizations and autocratizations.

	Run 1	Run 2	Run 3
Improvements	9 (26.5%)	11 (32.4%)	10 (29.4%)
No change	13 (38.2%)	11 (32.4%)	12 (35.3%)
Declines	12 (35.3%)	12 (35.3%)	12 (35.3%)

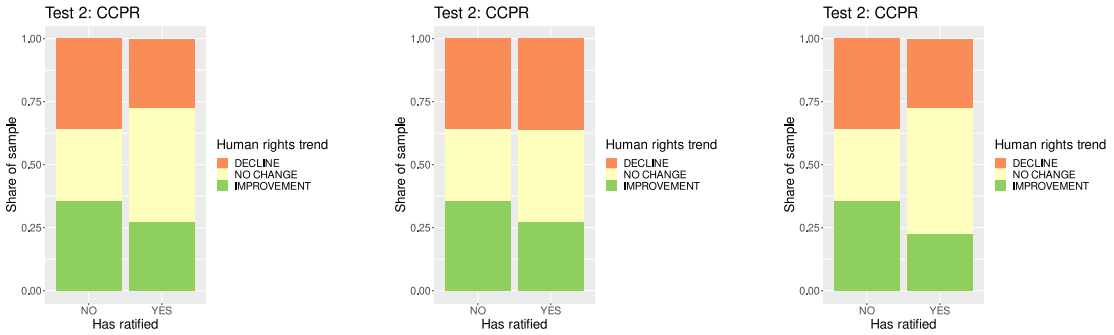
Table 10: Numerical information for the tests in figure 11.

What we see in figure 11 represents the direction of change from the first five years of a regime's existence to the following five years. If the average civil liberties ranking for the first period is lower than the average for the second period, it is categorized as an improvement, and similarly for the other categories.

The sizes of the trend fractions are fairly consistent across the three test runs. Each category tends to make up about a third of cases, but there may be a small tilt towards more declines and no change than improvements.

5.2.2. Single regimes and the CCPR

The plots of figure 12 show the human rights trends for samples taken based on ratification of the CCPR. The associated numbers and p-values can be found in table 11. If hypothesis 2 is true, we should expect to see a tendency towards more declines and fewer improvements for ratifiers.



(a) Run 1 (b) Run 2 (c) Run 3

Figure 12: Bar plots showing the human rights trend for ratifiers and nonratifiers of the CCPR after regime changes.

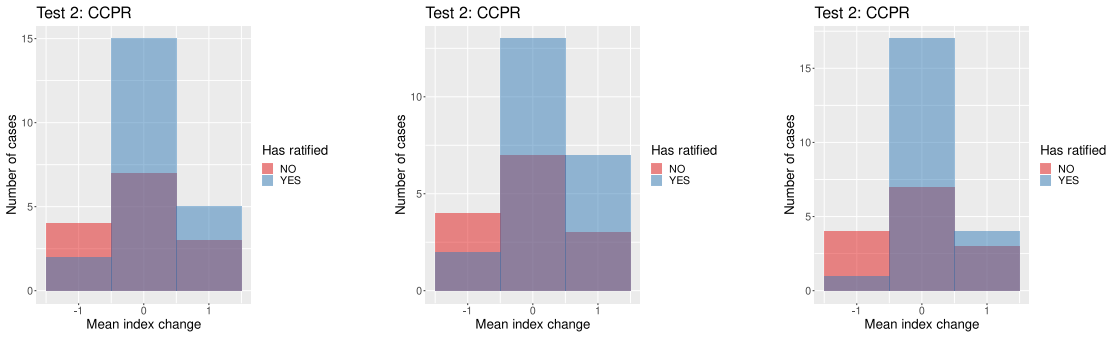
	Ratifiers			Non-ratifiers
	Run 1	Run 2	Run 3	All runs
Improvements	6 (27.3%)	6 (27.3%)	5 (22.7%)	5 (35.7%)
No change	10 (45.5%)	8 (36.4%)	11 (50%)	4 (28.6%))
Declines	6 (27.3%)	8 (36.4%)	6 (27.3%)	5 (35.7%)
p-value (χ^2 test)	0.5987	0.8368	0.4356	

Table 11: Numerical information for the tests in figure 12 and associated p-values.

The results are more or less stable across runs with the NO CHANGE category always being larger for ratifiers than nonratifiers. Their IMPROVEMENT category is always smaller, which would indicate support for hypothesis 2 if it was not for the fact that the DECLINE category also is smaller for ratifiers in all but one case. It seems that ratification makes the trend more stable.

The sample sizes for this test are the smallest this far – there are 22 ratifiers and 14 nonratifiers. This makes the results a little less authoritative than then previous ones. The χ^2 test accounts for this so it is not a problem as long as the frequencies are at least the required minimum mentioned in section 4.6.2. The minimum is 5 in this case, which is true for all but one category, the NO CHANGE category for nonratifiers.

The fact that both improvements and declines are less common for ratifiers than nonratifiers in the bar plot makes it difficult to say anything about how it relates to hypothesis 2. Comparing the exact mean index changes may provide more clues. Figure 13 shows, in histogram format, three runs of the same test but using the values of the **mean.index.change** column of `sinreg` instead of the **trend** column. If hypothesis 2 is true, the distribution for the ratifiers should be to the right of that of the nonratifiers.



(a) Run 1 (b) Run 2 (c) Run 3

Figure 13: Histograms showing the mean index change of ratifiers and nonratifiers of the CCPR after regime changes.

	Run 1	Run 2	Run 3
Nonratifier mean	0.007143	0.007143	0.007143
Ratifier mean	0.01364	0.06818	0.08636
p-value (two-sided t-test)	0.9714	0.7399	0.6182

Table 12: Sample means and p-values for the tests in figure 13.

In all runs of the test, the groups overlap to a large extent and their means are at most 0.025 points apart. The mean of the ratifiers is, however, the larger one in all runs. The p-values varies a lot in this case, which may be an effect of the population sizes being too small. Nevertheless, they all indiciate the same thing – that the results are not statistically significant.

5.2.3. Single regimes and the Optional Protocol

As with the consecutive regimes, when it comes to the test based around the Optional Protocol, I will show the results both with and without ratifiers of only the CCPR included. Figure 14 shows the human rights trends of samples of ratifiers and nonratifiers of the Optional Protocol, not accounting for CCPR ratification. The exact sizes of each category and the p-values for all the runs of the test can be seen in table 13. If hypothesis 2 is true, ratifiers should have more declines and fewer improvements than nonratifiers.

(a) Run 1 (b) Run 2 (c) Run 3

Figure 14: Bar plots showing the human rights trends for ratifiers and nonratifiers of the Optional Protocol after regime changes, inclusive version.

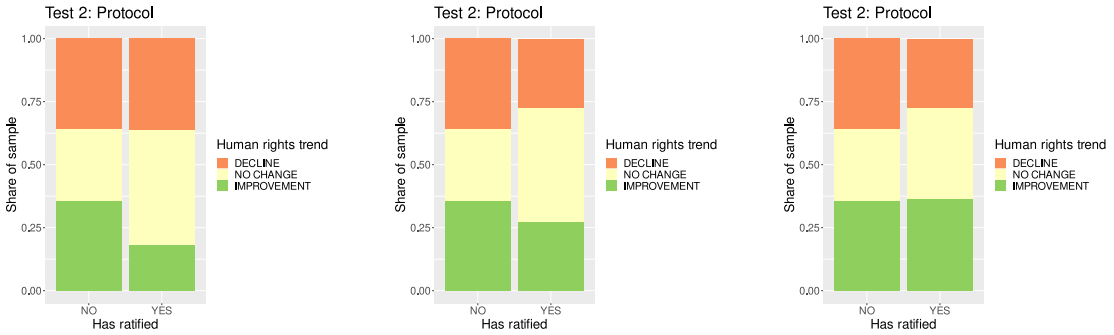
	Ratifiers			Non-ratifiers
	Run 1	Run 2	Run 3	All runs
Improvements	4 (36.4%)	3 (27.3%)	3 (27.3%)	12 (37.5%)
No change	5 (45.5%)	5 (45.5%)	6 (54.5%)	10 (31.2%)
Declines	2 (18.2%)	3 (27.3%)	2 (18.2%)	10 (31.2%)
p-value (χ^2 test)	0.6135	0.6833	0.379	

Table 13: Numerical information for the tests in figure 14 and associated p-values.

As with the test based on ratification of the CCPR, the largest category for the ratifiers is NO CHANGE. It is also larger than the same category for the nonratifiers in all runs, making up around half of cases for the ratifiers. For the nonratifiers, the categories are about equally distributed with slightly more improvements than other groups. Improvements and declines are both less common for ratifiers than nonratifiers in all runs of the test, making it difficult to say how the results relate to the second hypothesis.

The ratifier group is very small for this test, with only 11 members compared with the 32 members in the nonratifier group. This makes it doubtful whether the χ^2 test can produce reliable p-values. Each category should have at least 5 members when using it but two of the categories for the ratifiers do not fulfill this requirement in all runs of the test. However, the p-values show no statistical significance at the 0.05 level which is also the conclusion one would have to draw just from the population sizes being too small.

The next test, displayed in figure 15, excludes ratifiers of only the CCPR from the nonratifier group. As before, if hypothesis 2 is true, there should be fewer improvements and more declines for the ratifiers than the nonratifiers.



(a) Run 1 (b) Run 2 (c) Run 3

Figure 15: Bar plots showing the human rights trends of ratifiers and nonratifiers of the Optional Protocol after regime changes, strict version.

	Ratifiers			Non-ratifiers
	Run 1	Run 2	Run 3	All runs
Improvements	2 (18.2%)	3 (27.3%)	4 (36.4%)	5 (35.7%)
No change	5 (45.5%)	5 (45.5%)	4 (36.4%)	4 (28.6%)
Declines	4 (36.4%)	3 (27.3%)	3 (27.3%)	5 (35.7%)
p-value (χ^2 test)	0.5586	0.6831	0.8804	

Table 14: Numerical information for the tests in figure 15 and associated p-values.

There seems to be even less consistency across runs when exluding ratifiers of only the CCPR, which is not very surprising since here too, the number of ratifiers and nonratifiers are both very small. As before, there are 11 ratifiers but now the nonratifier group is down to 14 members. This makes the χ^2 test even more problematic since almost no categories have the minimum number of members. This makes it difficult to say whether there is a statistically significant difference between the groups, which is also what is indicated by the p-values of table 14.

However, as before, it seems that ratifiers tend to more often see no change in the human rights level. The fraction made up of the NO CHANGE category for ratifiers varies between 36.4–45.5% whereas it is only 28.6% for the nonratifiers across all runs. None of the runs show a statistically significant result.

Histograms for test 2 using the Optional Protocol and excluding ratifiers of only the CCPR from the nonratifier group can be found in figure 16 with the associated numerican information in table 15.

(a) Run 1 (b) Run 2 (c) Run 3

Figure 16: Histograms showing the mean index change for ratifiers and nonratifiers of the Optional Protocol after regime changes, strict version.

	Run 1	Run 2	Run 3
Nonratifier mean	0.007143	0.007143	0.007143
Ratifier mean	-0.1455	-0.1455	-0.1273
p-value (two-sided t-test)	0.5776	0.5776	0.6315

Table 15: Sample means and p-values for the tests in figure 16.

There is no statistically significant difference between the groups and their means are never further apart than about 0.15 index points. The population sizes are the same as for the test with bar plots so one needs to be careful when interpreting the results.

5.3. Population means

While it is not strictly related to the hypotheses of this thesis, it may still be interesting to know some characteristics of the samples when interpreting the results. Table 16 contains the means of the different possible samples. The ratio of democratizations to autocratizations is 2.125 for all groups from `conreg` and 1.8 for all groups from `sinreg`. Means for nonratifiers of the Optional Protocol do not include ratifiers of only the CCPR.

		Mean for period 1	Mean for period 2
`conreg`	CCPR ratifiers (37)	4.49	4.11
	CCPR nonratifiers (25)	4.74	3.92
	Protocol ratifiers (25)	4.22	3.88
	Protocol nonratifiers (25)	4.74	3.92
`sinreg`	CCPR ratifiers (22)	4.31	4.21
	CCPR nonratifiers (14)	4.17	4.18
	Protocol ratifiers (11)	4.20	3.95
	Protocol nonratifiers (14)	4.17	4.18

Table 16: Means for ratifiers and nonratifiers of both treaties for both tests.

6. Analysis

Overall, the results show no statistically signifiant difference between ratifiers and nonratifiers, both when it comes to regime changes and within regimes. This should be interpreted as the CCPR and its Optional Protocol not affecting state behavior in these circumstances. However, this does not mean that the treaties do not change state behavior in general. It may for example be the case that there are long term effects of ratification for a state. Hathaway does not find long term effects in her study (See section 2.4.1) but it is from 2002 and so was not able to look at effects over periods longer than 26 years at best for the CCPR and the protocol since they became effective in 1976.

It is instructive to look at the histograms if one wants to understand why it is difficult to say anything conclusive about the results – there tends to be a large overlap between the two groups. Oftentimes, the bars for the ratifiers are taller, but that is because they tend to have larger samples. What matters is that the shape is similar and the peak is at more or less the same position on the horisontal axis.

6.1. The results' relation to hypothesis one

Even though the results are not statistically significant, it may be interesting to look at what they may indicate. Hypothesis 1 was that ratifiers of either treaty should see more improvements after a regime change than nonratifiers. What was instead found was that, for both the CCPR and the Optional Protocol, nonratifiers saw more improvements in general. This was true both when looking at the means for the tests using histograms and at the fraction of improvements and declines in the bar plots. In other words, even though the ratifiers were predicted to see more improvements, they saw fewer.

In general, there was good consistency across runs so the lack of statistical significance should not be attributed to chance. There was a good number of regime changes included in the test with sample selection based on CCPR ratification – 37 ratifiers and 25 nonratifiers – which is large enough that a difference should be visible if there was one. For the strict version of the test with samples taken based on ratification of the Optional Protocol, the sample sizes were smaller but not by much; there were 25 nonratifiers and 25 ratifiers.

There is a tendency in all tests for the majority of changes being for the better in both samples. The reason for this can be understood by looking at the pure regime change test in figure 5, which uses samples of equal numbers of democratizations and autocratizations. In that case, half of the regime changes tended to result in improvements and the other half in declines or no change at all. Because democratic changes are more common in my populations, and because I want the sample sizes to be as large as possible, I increased the proportion of democratizations for the other tests as much as possible while keeping it the same for both samples. This generally resulted in about two times as many democratizations as autocratizations which appears in the plots as a larger fraction of improvements or a lower mean of the mean index changes. This carries no relevance for the hypotheses since I am only interested in the difference between them.

The point of including the Optional Protocol was that it lets individuals of states that

have ratified it complain to the United Nations Human Rights Committee when they think that their rights under the CCPR have been breached. Ratifying it should thus put more pressure on states to comply since breaches are more likely to be detected. This relates to hypothesis 3 which says that the Optional Protocol should increase the effects of the other two hypotheses, making both the initial improvement and the subsequent decline greater for those states that have ratified it. However, the results were not statistically significant and there was no detectable visual indication that this hypothesis 3 might be correct.

The tests with ratification of the Optional Protocol as criterion for sampling were performed in two versions. The first, displayed in figure 14, included ratifiers of only the CCPR in the nonratifier group. Thus, some of the nonratifiers had obligations to protect the same human rights as the ratifiers, just without the ability of their populations to make individual complaints. If it was really the case that the Optional Protocol made improvements more likely or likely to be greater, a difference should be seen here too. Instead, this is the plot where the ratifiers and nonratifiers look the most similar. When excluding ratifiers of only the CCPR, I go from a sample size of 42 nonratifiers to 25. The population size of the ratifiers go from 26 to 25. In both cases, they are large enough that a difference should be seen if it was of non-trivial size. Unlike the inclusive test, the strict one does show a difference between the samples, although it is not statistically significant. This can be seen in figure 15.

6.2. The results' relation to hypothesis two

For the second hypothesis, the one that predicted that ratifiers would see a relative decline, the means of the samples may indicate very little support when samples are based on ratification of the CCPR, but not when they are based on the Optional Protocol. The difference between the means is very small in both cases. Note, however, that there is no statistical significance in any of the tests and the sample sizes are much smaller than for the previous tests. Due to these factors, I will refrain from commenting further on what the results may or may not indicate with regards to hypothesis 2.

6.3. The results' relation to hypothesis three

The third hypothesis concerning the effect-enhancing role of the Optional Protocol does not imply that ratifiers always should see more improvements. The effects that were expected to be enhanced were those of the other two hypotheses. In the case of the single regimes, this would be if ratifiers of the Optional Protocol had even more of a tendency towards decline than ratifiers of the CCPR and vice versa for the consecutive regimes. The means for ratifiers and nonratifiers in the test with consecutive regimes are about the same for both treaties, which means that the differences between the means are also about the same. In other words, the predicted effect is not visible. For the single regimes, the means are very close to each other for both threaties, and there are the problems with statistical reliability mentioned earlier so the hypothesis cannot be said to be supported by these tests either.

6.4. Characteristics of different groups

Improvements in human rights when there is a regime change seem to be more common for ratifiers than nonratifiers, for both the tests with the CCPR and with the Optional Protocol. The results were not statistically significant but are in line with Oona Hathaway's finding that ratification of universal human rights treaties show no stastistically significant effect, or sometimes negative effects (see section 2.4.1).

It is surprising that ratifiers tend to have more declines and fewer improvements than nonratifiers so it might be worth investigasting this further. Looking at table 16, we see that nonratifiers generally have a slightly worse ranking on average before the regime change than ratifiers. This could indicate that their rankings are already so low that improvements are easier or declines more difficult since they start from a worse level. However, the difference between them is not more than a few decimals.

After the regime change, the mean index improves for all groups. However, the ratifiers end up with a worse ranking than nonratifiers in the case of the CCPR and better in the case of the Optional Protocol. Since the nonratifier group is the same in both cases their values are identical for both treaties and they are also the group that see the largest change. It is hard to say why this is so.

Within regimes, the difference between the first five years and the subsequent five years tend to be small, as can be seen in table 16. Nevertheless, there is something interesting to say about the means of the different groups here. The nonratifiers are the same both for the CCPR and the protocol, and they see a decline of 0.01 index points. The ratifiers on the other hand see improvements – 0.1 index points for the CCPR ratifiers and 0.25 index points for the protocol ratifiers. This may indicate that within the same regime, ratification may have a positive effect on the development of human rights. However, it has to be kept in mind that the means are for samples taken the same way as for the tests, and since no statistically significant difference was found for the changes there, none should be assumed here either.

The two preceeding paragraphs suggest that human rights improvements are larger for nonratifiers when there is a regime change but larger for ratifiers during the existence of the regime. One explanation of this could be that for nonratifiers, there is no legal way for individuals and organizations to improve human rights so when improvements do happen, they happen through regime changes.

Finally, I will say something about what the means look like when democracy is not accounted for. This carries no relevance for the hypotheses but may be interesting for alternative explanations of the results so I briefly present it here instead of in the results. When looking at all ratifiers of the CCPR, the mean of their mean indices for the first five years of their existence is 3.73. For ratifiers of the Optional Protocol, the same number is 3.7. Nonratifiers of the CCPR and the Optional Protocol have value for this period of 4.17 and 4.34, respectively. Taken together with the fact that, as seen in section 5.2, ratifiers tend to have a larger fraction of cases with no change from the first five years to the subsequent five years, this indicates that in general, being a ratifier means remaining stable at a higher ranking, whereas being a nonratifier means more volatility starting from a worse ranking.

7. Conclusion

I have tried to apply, to the best of my abilities, Andrew Guzman's theory to the case of human rights compliance at times of regime change. The lack of statistically significant results means that I find no support for the theory being correct.

However, one should not be too hasty too reject the theory. One important part of it is its expectation that international law works only at the margins, not completely constraining the behavior of states but pushing them slightly closer to what the law prescribes. It is recognized that only the reputational mechanism for compliance and not the others is available for human rights treaties so this part of international law should be expected to be especially weak. The compartmentalization of reputation also makes reputation in issue areas that may be more valuable for states, such as free trade, more important to comply with. Just how much a reputation for compliance with human rights obligations affect these other areas is hard to say. Despite this, it is somewhat surprising to see indications that ratification may lead to more declines in levels of respect for human rights.

Other potential reasons that the hypotheses find no support are the somewhat small sample sizes used for the analysis which may be the cause of the lack of statistical significance. This is especially true when the effect is expected to be small, as explained in section 4.6. It could also be that separating ratifiers of the CCPR from its nonratifiers is not a good way to separate states having the obligations corresponding to the Freedom House indicators from those that do not. This has been discussed in section 3.6.1. I have assumed that states use on-the-ground respect for human rights rather than just court cases or decisions from the Human Rights Committee to form their view of other states. This seems like it should be true, because states should want to take into account all available information in order to form accurate beliefs, but there may be reasons why they would give less weight to the Freedom House rankings.

The tendency for ratifiers to have worse trends may make a small discussion of the expressive role of treaties relevant. We saw in the end of the analysis that ratifiers in general have better human rights ratings than nonratifiers so joining the treaty may indeed signal that one is part of the group of states that care about human rights. Once one is a member, however, there is less pressure for actual improvements, unlike for nonmembers. This results in nonmembers putting in more effort to improve.

In conclusion, the purpose of this thesis has been to construct falsifiable hypotheses of how states act at times of regime change and to test these quantitatively in order to learn what the effects of international law are in practice. The tests did not show any statistically significant difference between ratifiers and nonratifiers of the International Covenant on Civil and Political Rights or its Optional Protocol, and consequently no effect of these treaties on state behavior was found. There have on the other hand been indications that the treaties have an expressive role and future research should investigate this further.

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A. Code

code/project.R

```
load_data = 0

source("src/add_ages.R")
source("src/get_find_replace_list.R")
source("src/country_name_replace.R")
source("src/get_fh_indices.R")
source("src/process_ratification_data.R")
source("src/get_average_index.R")
source("src/get_ratifier_column.R")
source("src/display_info.R")
source("src/extract_consecutives.R")
source("src/populations.R")

library(gdata)
library(tibble)
library(lubridate)
library(ggplot2)
library(xtable)

if (load_data)
{
raw_freedom_house = read.xls(
    "data/Country_and_Territory_Ratings_and_Statuses_FIW1973-2019.xls",
    sheet=2, stringsAsFactors=FALSE,
    header=FALSE,na.string="-")
writeLines("Freedom House data read")

raw_polity_iv = read.xls("data/p4v2018.xls", sheet=1,
                         stringsAsFactors=FALSE)
writeLines("Polity IV data read")

raw_ratification_data =
    read.xls("data/UnderlyingData_ICCPR_OHCHR_12_02_2020.xls",
             stringsAsFactors=FALSE, skip=1, nrow=198)
writeLines("CCPR ratification data read")

raw_protocol_ratification_data =
    read.xls("data/UnderlyingData_ICCPR-OP1_OHCHR_07_02_2020.xls",
             stringsAsFactors=FALSE, skip=1, nrow=198)
writeLines("CCPR Optional Protocol ratification data read\n")
}

###############
## SETTINGS ##
###############

min_age_sinreg = 10
min_age_conreg = 3
n_p1_years_single = 5
```

```
50  n_years_conreg = 2
51  start_year = 1948
52  fh_start_year = 1972
53  max_gap = 3
54
55  ## Working copy of Polity dataset.
56  regime_changes = raw_polity_iv
57
58  # Keep only rows where there is regime change data (D4=TRUE).
59  regime_changes = regime_changes[regime_changes$d4 %in% TRUE,]
60
61  # Keep only rows with data for years after a start year.
62  regime_changes = regime_changes[regime_changes$year >= start_year,]
63
64  ######################
65  ## SPECIAL CASES ##
66  ######################
67
68  # Change name of "Sudan-North" in the Polity data because it is just
69  # called "Sudan" in the other datasets.
70  regime_changes[regime_changes$country == "Sudan-North","country"] = "Sudan"
71  SUD2011 = regime_changes$country == "Sudan" & regime_changes$year == 2011
72  regime_changes = regime_changes[!SUD2011,]
73
74  # Removing Kosovo because of too little Freedom House data.
75  regime_changes = regime_changes[regime_changes$country != "Kosovo",]
76
77  # Myanmar seems to have an error in the data, the "byear" of the
78  # regime is year 6.
79  MYA2016 = regime_changes$country == "Myanmar (Burma)" &
80      regime_changes$year == 2016
81  regime_changes[MYA2016, "byear"] = 2016
82  regime_changes[MYA2016, "bmonth"] = 01
83
84  # There's no begin-data for Syria in 1961 so removing that entry.
85  regime_changes = regime_changes[!(regime_changes$country == "Syria" &
86                                    regime_changes$year == "1961"),]
87
88  ########################
89  ## REGIME FILTERING ##
90  ########################
91
92  # If the regime transition has REGTRANS = 0, exclude it and count it
93  # as the same regime as before the transition.
94  regime_changes = regime_changes[regime_changes$regtrans != 0,]
95
96  regime_changes = add_ages(regime_changes)
97
98  # Create dataset for test 1.
99  conreg = extract_consecutives(regime_changes, years(min_age_conreg))
100 conreg = conreg[year(conreg$regime1.edate) >= fh_start_year +
101                 n_years_conreg,]
102
103 too_long_interruption = conreg$regime2.bdate - conreg$regime1.edate >
```

```
104 years(max_gap)
105 conreg = conreg[!too_long_interruption,]
106 writeLines(paste(sum(too_long_interruption), "consecutive regimes",
107                  "filtered because more than", max_gap,
108                  "years between them"))
109
110 # Define periods for analysis.
111 conreg$p1.bdate = floor_date(conreg$regime1.edate, unit = 'year') -
112     years(n_years_conreg)
113 conreg$p1.edate = floor_date(conreg$regime1.edate, unit = 'year') - days(1)
114 conreg$p2.bdate = ceiling_date(conreg$regime2.bdate, unit = 'year')
115 conreg$p2.edate = ceiling_date(conreg$regime2.bdate, unit = 'year') +
116     years(n_years_conreg) - days(1)
117
118 # Create dataset for test 2.
119 durable_with_data = regime_changes$age >= years(min_age_sinreg) &
120     regime_changes$byear >= fh_start_year
121 columns_to_keep = c("country", "year", "bdate",
122                     "edate", "regtrans", "age")
123 sinreg = regime_changes[durable_with_data, columns_to_keep]
124
125 # Define periods for analysis.
126 sinreg$p1.bdate = ceiling_date(sinreg$bdate, unit = 'years')
127 sinreg$p1.edate = ceiling_date(sinreg$bdate, unit = 'years') +
128     years(n_p1_years_single) - days(1)
129 sinreg$p2.bdate = floor_date(sinreg$bdate, unit = 'years') +
130     years(n_p1_years_single + 1)
131 sinreg$p2.edate = ceiling_date(sinreg$bdate, unit = 'years') +
132     years(min_age_sinreg) - days(1)
133
134 ######################
135 ## FREEDOM HOUSE ##
136 ######################
137
138 # Replace country names in the Freedom House table so they match names
139 # in Polity data.
140 find_replace_fh     = get_find_replace_list('fh')
141 included_countries  = unique(append(conreg$country, sinreg$country))
142 fh_data             = raw_freedom_house
143 fh_data[,1]         = country_name_replace(fh_data[,1], included_countries,
144                                            find_replace_fh)
145 writeLines("Freedom House country names replaced")
146
147 # Clean up the Freedom House data.
148 fh_data[3, "V1"] = "Information type"
149 rownames(fh_data) = fh_data$V1
150 fh_data = fh_data[-1,-1]
151 fh_data["Information type",] = trimws(fh_data["Information type",])
152
153 # It's the same country, just a name change.
154 fh_data["Yugoslavia", 91:99] = fh_data["Serbia and Montenegro", 91:99]
155
156 indices = get_fh_indices(fh_data, "CL")
157 # indices = (get_fh_indices(fh_data, "CL") +
```

```
158 #             get_fh_indices(fh_data, "PR")) / 2.0
159
160 # Make column for describing human rights trend based in mean index change.
161 get_trend_column = function(mean_change)
162 {
163     trend = ifelse(mean_change < 0, "IMPROVEMENT",
164                    ifelse(mean_change > 0, "DECLINE", "NO CHANGE"))
165     trend = factor(trend, c("DECLINE", "NO CHANGE", "IMPROVEMENT"))
166     return (trend)
167 }
168
169 # Get average index for period 1 and period 2 for sinreg.
170 sinreg$p1.mean = get_average_index(year(sinreg$p1.bdate),
171                                    year(sinreg$p1.edate),
172                                    sinreg$country, indices)
173 sinreg$p2.mean   = get_average_index(year(sinreg$p2.bdate),
174                                      year(sinreg$p2.edate),
175                                      sinreg$country, indices)
176 sinreg$mean.index.change = sinreg$p2.mean - sinreg$p1.mean
177 sinreg$trend = get_trend_column(sinreg$mean.index.change)
178
179 # Get average index for period 1 and period 2 for conreg.
180 conreg$p1.mean = get_average_index(year(conreg$p1.bdate),
181                                    year(conreg$p1.edate),
182                                      conreg$country, indices)
183 conreg$p2.mean  = get_average_index(year(conreg$p2.bdate),
184                                     year(conreg$p2.edate),
185                                      conreg$country, indices)
186 conreg$mean.index.change = conreg$p2.mean - conreg$p1.mean
187 conreg$trend = get_trend_column(conreg$mean.index.change)
188
189 ########################
190 ## RATIFIER COLUMN ##
191 ########################
192
193 # Make columns for CCPR ratification.
194 ccpr_ratifications = process_ratification_data(raw_ratification_data,
195                                                included_countries, "CCPR")
196 conreg$ccpr.ratifier    = get_ratifier_column(ccpr_ratifications, conreg)
197 sinreg$ccpr.ratifier    = get_ratifier_column(ccpr_ratifications, sinreg)
198
199 # Make columns for Optional Protocol ratification.
200 protocol_ratifications =
201     process_ratification_data(raw_protocol_ratification_data,
202                               included_countries, "PROTOCOL")
203 conreg$protocol.ratifier = get_ratifier_column(protocol_ratifications,
204                                                conreg)
205 sinreg$protocol.ratifier = get_ratifier_column(protocol_ratifications,
206                                                sinreg)
207
208 ############
209 ## TESTS ##
210 ############
211
```

```
212 # Only using major democratic transitions and adverse regime changes
213 sinreg = sinreg[sinreg$regtrans %in% c(-2, 3),]
214 conreg = conreg[conreg$regtrans %in% c(-2, 3),]
215
216 # Plot style.
217 bars_theme   = theme(text=element_text(family="sans,Helvetica", size=20))
218 bars_palette = scale_fill_brewer(palette = "RdYlGn")
219
220 # Plot label names.
221 bars_labs    = labs(x    = "Has ratified",
222                     y    = "Share of sample",
223                     fill = "Human rights trend")
224 histogram_labs = labs(x     = "Mean index change",
225                       y     = "Number of cases",
226                       fill  = "Has ratified")
227
228 # Titles for plots.
229 init1_bars_title = ggtitle(label = "Test 1: Pure regime change test")
230 init2_bars_title = ggtitle(label = "Test 2: Pure regime change test")
231 main1_ccpr_bars_title = ggtitle(label = "Test 1: CCPR")
232 main2_ccpr_bars_title = ggtitle(label = "Test 2: CCPR")
233 main1_protocol_bars_title = ggtitle(label = "Test 1: Protocol")
234 main2_protocol_bars_title = ggtitle(label = "Test 2: Protocol")
235
236
237 show_histogram = function(populations, treaty, bars_title)
238 {
239     ggplot(populations,aes_string("mean.index.change", fill=treaty)) +
240         bars_theme + histogram_labs + bars_title +
241         geom_histogram(alpha = 0.5, binwidth = 1,
242         position = "identity") +
243         # geom_density(alpha = 0.2) +
244         scale_fill_brewer(palette = "Set1")
245 }
246
247 show_bars = function(populations, treaty, bars_title)
248 {
249     return(ggplot(populations,aes_string(treaty, fill="trend")) +
250             bars_theme + bars_labs + bars_title + bars_palette +
251             geom_bar(position = "fill"))
252 }
253
254 run_chi_test = function(populations, group_criterion)
255 {
256     contingency_table = table(populations[, group_criterion],
            populations$trend)
257     rownames(contingency_table) = c("Nonratifiers", "Ratifiers")
258     # contingency_table = t(contingency_table)
259     # writeLines("")
260     # print(xtable(contingency_table))
261     # writeLines("")
262     # print(contingency_table)
263     # writeLines("")
264     print(chisq.test(contingency_table))
```

```
265 }
266
267 run_pure_regime_change_test = function(TEST_NUMBER)
268 {
269     writeLines(paste("\nRunning initial test for test", TEST_NUMBER, "\n"))
270
271     main_dataset = switch(TEST_NUMBER, conreg, sinreg,
272                           stop("No test with that number."))
273     populations = get_populations_initial_test(main_dataset)
274
275     # ccpr.ratifier just used because an x variable is required.
276     treaty = "ccpr.ratifier"
277     populations[,treaty] = "MIXED"
278
279     # Show statistics for population.
280     writeLines(paste("Information for mixed sample of ratifiers,",
281                      "non-ratifiers and those who joined."))
282     writeLines(paste("The ratio of democratizations to autocratizations",
283                      "in this group is 1."))
284     HR_improvements(populations)
285
286   # Save plot to disk
287   svgname = paste0("t", TEST_NUMBER, "-initial-bars-0.svg")
288     # svg(filename = svgname)
289
290     plot_title = switch(TEST_NUMBER, init1_bars_title, init2_bars_title)
291     show_bars(populations, treaty, plot_title)
292 }
293
294 run_main_test = function(TEST_NUMBER, TREATY, GEOMETRY = "BARS")
295 {
296     writeLines(paste("\nRunning main test for test", TEST_NUMBER,
297                      "with ratification categorization based\non", TREATY, "\n
                             "))
298     treaty_column = switch(TREATY,
299                            "CCPR" = "ccpr.ratifier",
300                            "PROTOCOL" = "protocol.ratifier",
301                            stop("No treaty with that code."))
302     main_dataset = switch(TEST_NUMBER, conreg, sinreg,
303                           stop("No test with that number."))
304
305     populations = get_populations_main_test(main_dataset, treaty_column)
306     populations$trend = get_trend_column(populations$mean.index.change)
307
308     plot_title = switch(paste0(TREATY, TEST_NUMBER),
309                         CCPR1 = main1_ccpr_bars_title,
310                         CCPR2 = main2_ccpr_bars_title,
311                         PROTOCOL1 = main1_protocol_bars_title,
312                         PROTOCOL2 = main2_protocol_bars_title)
313
314     # Show populations statistics.
315     HR_improvements_ratification_status(populations, TREATY)
316
317   # Save plot to disk
```

```
318     svgname = paste0("t", TEST_NUMBER, "-main-", tolower(TREATY), "-",
319                      tolower(GEOMETRY), "-0.svg")
320     # svg(filename = svgname)
321
322     switch(GEOMETRY,
323         BARS = {
324             HR_improvements_ratification_status(populations, TREATY)
325             # run_chi_test(populations, treaty_column)
326             show_bars(populations, treaty_column, plot_title)
327         },
328         HISTOGRAM = {
329             print(t.test(populations[,"mean.index.change"] ~
330                          populations[,treaty_column], var.equal=TRUE))
331             show_histogram(populations, treaty_column, plot_title)
332         },
333         stop("No such plot geometry.")
334     )
335 }
```

code/src/add_ages.R

```
1  add_ages = function(changes)
2  {
3
4      get_end_date = function(a_change)
5      {
6          return (as.Date(paste0(a_change[1,"eyear"], "-",
7                                 a_change[1,"emonth"], "-",
8                                 a_change[1,"eday"])))
9      }
10
11     get_begin_date = function(a_change)
12     {
13         return (as.Date(paste0(a_change[1,"byear"], "-",
14                                a_change[1,"bmonth"], "-",
15                                a_change[1,"bday"])))
16     }
17
18
19     # Add regime age, begin date and end date for regimes.
20     for (i in 1:(nrow(changes)-1))
21     {
22         same_country = changes[i,"ccode"] == changes[i+1, "ccode"]
23
24         # Calculate the age of regime.
25         if (same_country)
26         {
27             bdate = get_begin_date(changes[i,])
28             edate = get_end_date(changes[i+1,])
29             changes[i,"age"] = dseconds(interval(bdate, edate))
30             changes[i, "bdate"] = bdate
31             changes[i, "edate"] = edate
32         }
33         # Special case: last row for a country.
```

```
34          else
35          {
36              is_end_of_state = changes[i, "regtrans"] == 98 |
37                  changes[i, "regtrans"] == 96
38
39              if (is_end_of_state)
40              {
41                  # This row does not contain a new regime and is only
42                  # used for getting the end date of the last regime.
43                  # Setting its age to zero or one day removes it when
44                  # filtering for age later.
45                  changes[i, "age"] = 0
46                  changes[i, "bdate"] = ymd("20200101")
47                  changes[i, "edate"] = ymd("20200102")
48              }
49              else
50              {
51                  # This row is the present regime so count it as ending
52                  # today.
53                  bdate = get_begin_date(changes[i,])
54                  edate = as.Date("2018-12-31")
55                  changes[i,"age"] = dseconds(interval(bdate, edate))
56                  changes[i, "bdate"] = bdate
57                  changes[i, "edate"] = edate
58              }
59          }
60      }
61
62      # The last regime which is ignored in the loop and it is today's Fijian
63      # regime so give it edate = December 31, 2018.
64      bdate = get_begin_date(changes[nrow(changes),])
65      edate = as.Date("2018-12-31")
66      changes[nrow(changes),"age"] = dseconds(interval(bdate, edate))
67      changes[nrow(changes), "bdate"] = bdate
68      changes[nrow(changes), "edate"] = edate
69
70      return (changes)
71  }
```

code/src/country_name_replace.R

```
1  # country_names is the row or column of countries from a data.frame that
2  # should be changed (if they are going to be used).
3  #
4  # included_countries is the list of countries which have a regime in the
5  # regime_change variable after filtering for age and such (so only these
6  # need to have their names changed).
7  #
8  # replace_key is the key for knowing which country names in country_names
9  # to replace and what to replace them with.
10 country_name_replace = function(country_names, included_countries,
11                                  replace_key)
12 {
13
```

```
14      print_list = function(the_list)
15      {
16          size = length(the_list)
17          if (size == 0)
18          {
19              writeLines("\tNone")
20          }
21          else if (size == 1)
22          {
23              cat("\t")
24              cat(the_list[1], "\n")
25          }
26          else
27          {
28              cat("\t")
29              cat(the_list[-size], "", sep=", ")
30              cat(the_list[size], "\n")
31          }
32      }
33
34      # Find countries that have different names or are missing in dataset.
35      missing_countries = included_countries[!(included_countries %in%
36                                               country_names)]
37
38      for (i in 1:nrow(replace_key))
39      {
40          find_country = replace_key[i, "find"]
41          replace_country = replace_key[i, "replace"]
42
43          country_names[country_names==find_country] = replace_country
44      }
45
46      # Are there still non-matching countries left after changing the names?
47      missing_countries = included_countries[!(included_countries %in%
48                                               country_names)]
49
50      if (length(missing_countries) > 0)
51      {
52          warning("Some countries still have the wrong name")
53          print_list(missing_countries)
54      }
55
56      return (country_names)
57  }
```

code/src/display_info.R

```
1   HR_improvements = function(dataset)
2   {
3       n = nrow(dataset)
4
5       improvements = sum(dataset$mean.index.change < 0)
6       nochange     = sum(dataset$mean.index.change == 0)
7       decline      = sum(dataset$mean.index.change > 0)
```

```
8
9       writeLines(paste0("This dataset has ", n, " members"))
10      writeLines(paste0(improvements," (",round(improvements/n*100,digits=1),
11                        "%) saw improvements in HR after regime change."))
12      writeLines(paste0(nochange, " (", round(nochange/n*100, digits=1),
13                        "%) saw no change in HR after regime change."))
14      writeLines(paste0(decline, " (", round(decline/n*100, digits=1),
15                        "%) saw a decline in HR after regime change."))
16      writeLines(paste("The mean of the mean.index.change column is:    ",
17                       round(mean(dataset$mean.index.change), digits=4)))
18      writeLines(paste("The variance of the mean.index.change column is:",
19                       round(var(dataset$mean.index.change ), digits=4)))
20
21      # writeLines("\nFor LaTeX:\n")
22
23    writeLines(paste0(" & ", improvements, " (", round(improvements/n*100,
          digits=1), "\\%)"))
24    writeLines(paste0(" & ", nochange, " (", round(nochange/n*100,digits=1),
          "\\%)"))
25    writeLines(paste0(" & ", decline, " (", round(decline/n*100,digits=1),
          "\\%)"))
26    writeLines(paste0(" & ", round(mean(dataset$mean.index.change), digits=4)
          ))
27    writeLines(paste0(" & ", round(var(dataset$mean.index.change), digits=4))
          )
28  }
29
30  HR_improvements_ratification_status = function(dataset, TREATY)
31  {
32      column = switch(TREATY,
33                      "CCPR" = "ccpr.ratifier",
34                      "PROTOCOL" = "protocol.ratifier")
35
36      ratifiers = dataset[dataset[, column] == "YES",]
37      nonratifiers = dataset[dataset[, column] == "NO",]
38
39      writeLines("")
40      writeLines("Information for ratifiers:")
41      HR_improvements(ratifiers)
42      writeLines("")
43      writeLines("Information for non-ratifiers:")
44      HR_improvements(nonratifiers)
45  }
```

code/src/extract_consecutives.R

```
1   extract_consecutives = function(regimes, minimum_age)
2   {
3
4       # Empty data frame to be filled with consecutive regimes.
5       consecutives = data.frame("country"         = character(0),
6                                 "regime1.bdate"   = numeric(0),
7                                 "regime1.edate"   = numeric(0),
8                                 "regime1.age"     = numeric(0),
```

```
                                "regime2.bdate"   = numeric(0),
                                "regime2.edate"   = numeric(0),
                                "regime2.age"     = numeric(0),
                                "regtrans"        = numeric(0))

    # If two consecutive regimes in one country are both over min_age,
    # combine to one row and add to new data.frame.
    for (countryname in unique(regimes$country))
    {
        # One loop for each country so that no consecutive regimes are from
        # different countries.
        country = regimes[regimes$country==countryname,]

        if (nrow(country) > 1)
        {
            for (i in 1:(nrow(country)-1))
            {
                # Both regimes need to be at least the minimum age.
                if (country[i,"age"] >= minimum_age &
                    country[i+1, "age"] >= minimum_age)
                {
                    new_row = data.frame(
                        "country"       = as.character(countryname),
                        "regime1.bdate" = country[i, "bdate"],
                        "regime1.edate" = country[i, "edate"],
                        "regime1.age"   = country[i, "age"],
                        "regime2.bdate" = country[i+1, "bdate"],
                        "regime2.edate" = country[i+1, "edate"],
                        "regime2.age"   = country[i+1, "age"],
                        "regtrans"      = country[i+1, "regtrans"],
                        stringsAsFactors=FALSE)
                    consecutives = rbind(consecutives, new_row)
                }
            }
        }
    }
    return (consecutives)
}
```

code/src/get_average_index.R

```
get_average_index = function(start_year, end_year, country, indices)
{
    index_sum = rep(0, length(country))

    for (i in 1:length(index_sum))
    {
        for (year in start_year[i]:end_year[i])
        {
            current_index = indices[country[i], as.character(year)]

            if (is.na(current_index))
            {
                stop(paste("No index for", country[i], year))
```

```
            }

            index_sum[i] = index_sum[i] + current_index
        }
    }
    return (index_sum / (end_year - start_year + 1))
}
```

code/src/get_fh_indices.R

```
get_fh_indices = function(freedom_house_data, index_type)
{
    # Select Civil Liberties or Political Rights.
    is_index_type = freedom_house_data["Information type",] == index_type

    # Remove rows that are not indices.
    index_table = freedom_house_data[-c(1,2), is_index_type]

    # South Africa had different ranking for black and white population in
    # 1972 but in order to process the data a single number is required so
    # I use the one for the black population (third character in string).
    index_table["South Africa", 1] = substr(index_table["South Africa", 1],
                                            3, 3)

    index_table[] = lapply(index_table, as.numeric)
    index_table_1982 = rowMeans(index_table[,10:11])
    index_table = add_column(index_table, index_table_1982, .after = 10)
    colnames(index_table) = 1972:2018

    return (index_table)
}
```

code/src/get_find_replace_list.R

```
get_find_replace_list = function(target_data)
{

    if (target_data == 'fh')
    {
        # What it's called in Freedom House dataset.
        find_name = c(
            "The Gambia",
            "Congo (Brazzaville)",
            "Eswatini",
            "Yemen, N.",
            "North Korea",
            "South Korea",
            "Vietnam, N.",
            "Vietnam, S.",
            "Yemen, S.",
            "Slovakia",
            "Bosnia and Herzegovina",
            "Cote d'Ivoire",
            "Congo (Kinshasa)",
```

```
21              "Myanmar",
22              "Timor-Leste",
23              "United Arab Emirates"
24          )
25
26          # What it's called in Polity dataset.
27          replace_name = c(
28              "Gambia",
29              "Congo Brazzaville",
30              "Swaziland",
31              "Yemen North",
32              "Korea North",
33              "Korea South",
34              "Vietnam North",
35              "Vietnam South",
36              "Yemen South",
37              "Slovak Republic",
38              "Bosnia",
39              "Ivory Coast",
40              "Congo Kinshasa",
41              "Myanmar (Burma)",
42              "East Timor",
43              "UAE"
44          )
45      }
46      else if (target_data == 'un')
47      {
48          # What it's called in UN dataset.
49          find_name = c(
50              "Bolivia (Plurinational State of)",
51              "Slovakia",
52              "The former Yugoslav Republic of Macedonia",
53              "Bosnia and Herzegovina",
54              "Republic of Moldova",
55              "Russian Federation",
56              "Cabo Verde",
57              "C\xf4te d'Ivoire",
58              "Congo",
59              "Democratic Republic of the Congo",
60              "United Republic of Tanzania",
61              "Eswatini",
62              "Iran (Islamic Republic of)",
63              "Republic of Korea",
64              "Lao People's Democratic Republic",
65              "Viet Nam",
66              "Timor-Leste",
67              "Venezuela (Bolivarian Republic of)",
68              "Myanmar",
69              "Syrian Arab Republic",
70              "United Arab Emirates",
71              "United States of America",
72              "Democratic People's Republic of Korea"
73          )
74
```

```
75          # What it's called in Polity dataset.
76          replace_name = c(
77              "Bolivia",
78              "Slovak Republic",
79              "Macedonia",
80              "Bosnia",
81              "Moldova",
82              "Russia",
83              "Cape Verde",
84              "Ivory Coast",
85              "Congo Brazzaville",
86              "Congo Kinshasa",
87              "Tanzania",
88              "Swaziland",
89              "Iran",
90              "Korea South",
91              "Laos",
92              "Vietnam",
93              "East Timor",
94              "Venezuela",
95              "Myanmar (Burma)",
96              "Syria",
97              "UAE",
98              "United States",
99              "Korea North"
100         )
101     }
102     else
103     {
104         stop("Wrong argument to get_find_replace_list.")
105         return(0)
106     }
107
108     return(data.frame("find" = find_name,
109                 "replace" = replace_name,
110                 stringsAsFactors = FALSE))
111 }
```

code/src/get_ratifier_column.R

```
1  # YES if country joined before the start of the first period under
2  # analysis, NO if it joined after the last period under analysis and JOINED
3  # if in between.
4  get_ratifier_column = function(ratifications, regimes)
5  {
6      ratifier_column = vector(mode = "character", length = nrow(regimes))
7      for (i in 1:nrow(regimes))
8      {
9          country = regimes[i, "country"]
10         date_of_effect = ratifications[ratifications$country == country,
11                                        "date.of.effect"]
12         if (date_of_effect < regimes[i, "p1.bdate"])
13         {
14             ratifier = "YES"
```

```
15          }
16          else if (date_of_effect <= regimes[i, "p2.edate"])
17          {
18              ratifier = "JOINED"
19          }
20          else
21          {
22              ratifier = "NO"
23          }
24          ratifier_column[i] = ratifier
25      }
26      return (ratifier_column)
27  }
```

code/src/populations.R

```
1   get_populations_initial_test = function(dataset)
2   {
3       dem_rows = dataset$regtrans %in%  3
4       aut_rows = dataset$regtrans %in% -2
5
6       # Use all members of smaller group and the same number of members
7       # sampled from the other group.
8       smaller_group = min(sum(dem_rows), sum(aut_rows))
9       democratizations = dataset[sample(which(dem_rows), smaller_group),]
10      autocratizations = dataset[sample(which(aut_rows), smaller_group),]
11
12      return(rbind(democratizations, autocratizations))
13  }
14
15  # Returns columns saying what rows belong to which population.
16  get_pop_rows = function(dataset, treaty)
17  {
18      dem   = dataset[,"regtrans"] %in% 3
19      aut   = dataset[,"regtrans"] %in% -2
20      rat   = dataset[, treaty]    %in% "YES"
21      norat = dataset[, treaty]    %in% "NO"
22
23      dem_rat   = dem == TRUE & rat == TRUE
24      aut_rat   = aut == TRUE & rat == TRUE
25      dem_norat = dem == TRUE & norat == TRUE
26      aut_norat = aut == TRUE & norat == TRUE
27
28      rows = data.frame(dem_rat, aut_rat, dem_norat, aut_norat)
29
30      return(rows)
31  }
32
33  # Returns number of members fulfilling the requirements for each population
34  # used in the tests.
35  get_pop_sizes = function(dataset, treaty)
36  {
37      n_democratization_ratifier     = nrow(dataset[dataset[,"regtrans"] ==  3
38                                                    & dataset[,treaty] == "YES",])
```

```
    n_autocratization_ratifier    = nrow(dataset[dataset[,"regtrans"] == -2
                                         & dataset[,treaty] == "YES",])
    n_democratization_nonratifier = nrow(dataset[dataset[,"regtrans"] ==  3
                                         & dataset[,treaty] == "NO",])
    n_autocratization_nonratifier = nrow(dataset[dataset[,"regtrans"] == -2
                                         & dataset[,treaty] == "NO",])

    statistics = data.frame("rat" = c(n_democratization_ratifier,
                                      n_autocratization_ratifier),
                            "norat" =
                                c(n_democratization_nonratifier,
                                  n_autocratization_nonratifier))

    rownames(statistics) = c("dem", "aut")

    return(statistics)
}

get_populations_main_test = function(dataset, treaty)
{
  # For protocol test: compare only
  if (treaty == "protocol.ratifier")
  {
    ratified_both = dataset$ccpr.ratifier == dataset$protocol.ratifier
    dataset = dataset[ratified_both,]
  }

    sizes = get_pop_sizes(dataset, treaty)
    rows  = get_pop_rows(dataset, treaty)

    # Use smallest ratio of democratizations to autocratizations in order
    # to have enough of them when sampling. The ratio should be the same
    # for both ratifiers and non-ratifiers.
    ratio = min(sizes["dem",] / sizes["aut",])

    ratifiers = rbind(
             dataset[sample(which(rows$dem_rat), min(sizes$rat) * ratio),],
             dataset[sample(which(rows$aut_rat), min(sizes$rat)),])

    nonratifiers = rbind(
         dataset[sample(which(rows$dem_norat), min(sizes$norat) * ratio),],
         dataset[sample(which(rows$aut_norat), min(sizes$norat)),])

    writeLines(paste("Returning", nrow(ratifiers), "ratifiers and",
                     nrow(nonratifiers), "non-ratifiers with a ratio of",
                     "\ndemocratizations to autocratizations of", ratio))

    return(rbind(ratifiers, nonratifiers))
}
```

code/src/process_ratification_data.R

```
# raw is the raw ratification data to processed, country_names is the list
# of countries that are used in either test and treaty is either CCPR or
```

```
3  # the optional protocol, which determines what the special cases are.
4  process_ratification_data = function(raw, country_names, treaty)
5  {
6    # Keep only the columns for country and ratification date
7    rd = raw[,c(1,3)]
8    colnames(rd) = c("country", "ratification.date")
9    rd$ratification.date = as.Date(rd$ratification.date, format='%Y/%m/%d')
10
11   # Not in list of ratifications.
12   rd[nrow(rd)+1, "country"] = "Taiwan"
13   rd[nrow(rd)+1, "country"] = "Kosovo"
14   rd[nrow(rd)+1, "country"] = "Vietnam North"
15   rd[nrow(rd)+1, "country"] = "Vietnam South"
16   rd[nrow(rd)+1, "country"] = "Yemen North"
17
18   # Change all NA to date past any regime end date in order to not get
19   # problems with if-statements. This is okay because NA mens they
20   # haven't ratified and will be categorized as such because their made
21   # up ratification date is past the regimes end date.
22   rd[is.na(rd$ratification.date), "ratification.date"] =
23     as.Date("2020-12-31")
24
25
26   # Ratification dates (or lack thereof) for no longer existing countries
27   additional_countries_ccpr = data.frame(
28     "country" = c("Germany West",
29             "Yugoslavia",
30             "Czechoslovakia",
31             "Yemen South"),
32     "ratification.date" = c(as.Date("1973-12-17"),
33                 as.Date("1971-06-02"),
34                 as.Date("1975-12-23"),
35                 as.Date("1987-02-09")))
36
37   additional_countries_protocol = data.frame(
38     "country" = c("Yugoslavia",
39             "Yemen South"),
40     "ratification.date" = c(as.Date("1990-03-14"),
41                 as.Date("2020-12-31")))
42
43   if (treaty == "CCPR")
44   {
45     rd = rbind(rd, additional_countries_ccpr)
46   }
47   else if (treaty == "PROTOCOL")
48   {
49     rd = rbind(rd, additional_countries_protocol)
50   }
51   else
52   {
53     stop("Wrong treaty code in argument. Stopping.")
54   }
55
56   treaty_entry_into_force = ymd("1976-03-23")
```

```
57    rd[,"date.of.effect"] = rd[,"ratification.date"] + days(90)
58    rd[rd$date.of.effect < treaty_entry_into_force, "date.of.effect"] =
59      treaty_entry_into_force
60
61    # Change names so that they correspond to the ones used in the Polity
62    # dataset.
63    find_replace_un = get_find_replace_list('un')
64    rd[,"country"] = country_name_replace(rd[,"country"], country_names,
65                          find_replace_un)
66    writeLines(paste("UN country names replaced for", treaty))
67
68    return(rd)
69  }
```