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Alliance Choices and Their Effects on Interstate Disputes: adopting a new methodological approach to address endogenous treatment and sample selection bias

by

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ABSTRACT

Alliance Choices and Their Effects on Interstate Disputes:
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Do different types of military alliance commitments have different impacts on interstate disputes? In this dissertation, I test the effects of different alliance commitments on interstate disputes, focusing on comparing the effects of having a defense pact and those of having a consultation pact. Another contribution of this dissertation is to address two methodological issues relevant to the research question, endogenous treatment and sample selection bias. Knowing that states choose alliance commitments in the prospect of interstate disputes, one should consider the alliance choices as a treatment variable that cannot be randomly assigned. Furthermore, in studying the latter phases of interstate disputes, the sample of the subsequent decisions is not representative of the entire population unless interstate disputes randomly occur, which is hardly justifiable in the International Relations literature.

The three main chapters of this dissertation can be best described as a demonstration of my learning process. Each chapter shares the same agenda described above but tackles different sets of the problems. Chapter 2 examines the effects of a consultation pact and those of a defense pact on the other states’ decision to attack an alliance member, the alliance member’s decision to attack others, and the alliance member’s decision to respond militarily when attacked. However, this chapter does not address any of the two methodological
challenges described above. Chapter 3 examines the effect of a defense pact on the first and
the third decision using a switching probit model, addressing endogenous treatment issue.
Chapter 4 addresses all of the research agenda mentioned above, except examining the second
foreign policy decision. I develop a Bayesian multiple equation model specifically tailored
to testing the effects of military alliances and use the model to study the effects of alliance
choices on interstate disputes. In conclusion, by adopting several different approaches to the
same research question, I find that a defense pact is negatively associated with the probability
of being a target of interstate disputes. On the other hand, it is not very clear how other
alliance commitments are related to interstate dispute initiation and escalation.
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Chapter 1

Introduction

Do military alliances matter? Because military alliances are about precommitting their signatories to specific security policies, they have been considered to be one of the most important subjects to study in the international relations (IR) literature. At the same time, there have been skeptics about the effect of military alliances on shaping states’ foreign policies, mainly because of the anarchic international system. Although the question seems to be important, the question itself is too vague to draw any meaningful conclusion. Acknowledging that there are various types of alliance commitments and states deliberately choose the commitment that helps them best to meet their policy goals, we can write a better question to ask to ourselves: Do different types of alliance commitments have different impacts on interstate disputes?

The rephrased question is more interesting because it not only allows us to compare the effects of having an alliance to the effects of having none, but also allows us to compare the effect of one alliance commitment to that of the other commitment. Furthermore, it helps us to think more about what the relevant consequences could be. For example, a defensive commitment is closely related to the prospect of being attacked by other states, while a nonaggression commitment may not. Therefore, depending on our variable of interest, the set of relevant alliance commitments may differ.

Of course, this is not the first scholarly work to examine the effects of different alliance commitments. However, most of the previous studies have examined the effects of alliance commitments separately, not often examining them together. This approach makes it harder to compare the effects of different alliance commitments in one paper. Furthermore, doing
so in one model would be even better. Why is the cross-comparison of the effects of alliance choices important? It is because there have been theoretical arguments that one alliance commitment can be an alternative to the other. If one alliance commitment has similar characteristics to the other’s, states can choose one over the other under certain conditions. If this is the case, these two alliance commitments will have comparable effects on relevant foreign policy decisions. Therefore, testing the effects of different alliance commitments in one model and comparing their effects on interstate disputes can enhance our understanding about alliance type choices.

One pair of alliance obligations that I focus on in this project is a defensive commitment and a consultation commitment. A defensive commitment is to precommit its members to provide active military assistance when conflicts occur. While the definition of a defensive commitment is quite straightforward, that of a consultation pact is not. A consultation commitment does not require active military assistance from their members in cases of conflict, but still does commit the members to coordination and communication. In other words, consultation pacts have been considered ‘weaker’ alliances in comparison to defense and offense pacts, which precommit member states to active involvement in conflict (Snyder 1997; Mattes 2012; Chiba, Johnson, and Leeds 2015). However, would a ‘stronger’ alliance be always preferred to a ‘weaker’ one? Do these ‘weaker’ alliance commitments enjoy similar effects to those of defense pacts on interstate disputes? Or, because they are weaker and thus less binding their signatories, is it possible for them to have any effect on interstate disputes? To my understanding, this set of questions has not been fully examined yet, so I investigate the effects of a consultation pact on interstate dispute initiation and escalation, mainly in comparison to those of a defense pact.

Another contribution that I make in this dissertation is to address two methodological issues in testing the effects of alliance choices on interstate disputes. First, knowing that states choose alliance commitments in the prospect of interstate disputes, one should consider the alliance choices as a non-random treatment variable. Controlling for the observable
confounding factors is one way to address the issue, but on top of that, we can also model any remaining confounding factors that we might have failed to control for. If it is the case, we will be able to observe a correlation between our variables of interest and take the correlations into account when calculating estimates. In other words, an endogenous treatment model adds another safety net for the effect of unobservable confounders. Few studies in the alliance literature have adopted this approach, and I examine the effects of different alliance choices on both dispute initiation and escalation using this approach.

Second, by definition, in order to examine the effects of alliance choices on dispute escalation, there should be at least one dispute initiated in the earlier stage. In other words, the sample of dispute escalation is not representative of the entire population unless interstate disputes randomly occur – which is hardly justifiable in the IR literature. Therefore, taking this non-random sample selection mechanism into account helps us to understand the effects of alliance choices on interstate disputes better. Although this dissertation focuses on dispute initiation and escalation, one can expand this approach further and examine the effects of alliance choices on subsequent foreign policy decisions – a decision to intervene in a conflict, a decision to withdraw, a decision to terminate a war, etc.

The three main chapters of this dissertation can be best described as a demonstration of my learning process. Each chapter shares the same agenda described above – to compare the effects of alliance choices on interstate disputes and to adopt more comprehensive methodological tools – but tackles different sets of the problems. Chapter 2 examines the effects of a consultation pact and a defense pact on three different decisions regarding interstate disputes: 1) whether an alliance member is more likely to be attacked by other states (deterrent effect), 2) whether an alliance member is more likely to attack other states (initiation effect), and 3) whether an alliance member is more likely to respond militarily when attacked (escalation effect). This is a replication of Johnson and Leeds (2011) with the consultation obligation as another independent variable. Therefore, this paper is able to compare the effects of the two different alliance obligations but does not address either
endogenous treatment issue or sample selection issue. In this chapter, I find that defense pacts and consultation pacts (without a defensive commitment) have similar levels of deterrent effect. Also, I find that both alliance commitments do not exert initiation effects or escalation effects. As a side note, the unit of analysis of the data set used in this chapter is directed dyad-year, while that of the other two chapters is monadic state-year level.

Chapter 3 examines the effects of a defense pact on the probability of being attacked and the probability of attacking others. It does not examine the effect of a consultation pact on the same set of outcome variables, but does address endogenous treatment issue by adopting a switching probit model. Also, because I use a pre-built model, I am not able to examine the effect of a defense pact on dispute escalation while addressing endogenous treatment issue. In this chapter, I find that the deterrent effect of a defense pact might have been underestimated in the previous studies, which often utilize a single-equation probit model. I find that states who are more likely to form a defense pact are at higher risk of being attacked or attacking others (in general, more likely to be dispute-prone). In other words, ignoring endogenous treatment issue results in putting the states who have defense pacts into a hard test: in a naive model, the higher underlying dispute-prone level that the defensive alliance members have cancels out the deterrence effect of a defense pact. After addressing the issue using the switching probit model, we can find much larger deterrent effect than those of simple probit models. Regarding the probability of attacking others, having a defense pact does not increase the chance of the alliance members’ attacking others. More interestingly, I find that defensive alliance members are less likely to attack other states if we address endogenous treatment issue.

Finally, Chapter 4 examines all of the research agendas mentioned above, except the initiation effect. I develop a Bayesian multiple equation model specifically tailored to testing the effects military alliance choices and validate my model with a Monte Carlo simulation. I apply the model to the same data set used in Chapter 3 to test the effects of a consultation pact, a defense pact, and an offense/neutrality pact (serves as ‘other alliances’) on
the probability of being attacked and the probability of responding militarily when attacked. In summary, all of the alliance commitments are associated with lowering the probability of being attacked. One finding that I can discuss most certainly is the deterrent effect of a defense pact. In most cases, the probable value of the effect is negative. For the other two alliance commitments, there are large variations in terms of their effects. Furthermore, none of the alliance commitments seems to have a clear escalation effect. The distribution of the estimated effects is almost symmetric around zero, sometimes with large variance. Chapter 5 is for conclusion of this dissertation. By adopting several different approaches to the same problem, I argue that it is most likely that a defense pact is negatively associated with the probability of being a target of interstate disputes. On the other hand, it is not very clear how a consultation pact (and an offense/neutrality pact) is related to dispute initiation and escalation. The results of this project leave several questions for future studies, such as why (or under what conditions) states sign a consultation pact.
Chapter 2

A Trade-off between Deterrence and Restraint in Alliance Design? Testing the Effects of Consultation Pacts and Defense Pacts on Interstate Disputes

2.1 Introduction

Do consultation pacts have impacts on states’ decisions, especially on maintaining peace or initiating militarized conflicts? Consultation pacts are alliances that do not require active military assistance from their members in cases of conflict, but still do commit the members to coordination and communication. For example, France and Russia in 1891 signed an alliance agreement that states the following: “In case that peace should actually be in danger, and especially if one of the two parties should be threatened with an aggression, the two parties undertake to reach an understanding on the measures whose immediate and simultaneous adoption would be imposed upon the two governments by the realization of this eventuality” (Hurst 1972, 663). Another example is the Russia-North Korea Friendship formed in 2000, which states that “In case of one of the parties becomes the target of aggression or if there is a threat to security and peace, and when there is a need for consultation and collaboration, the parties promise to contact each other immediately (Art. II).”

Because precommitting this type of policy coordination and communication is less costly for alliance members than precommitting active involvement in conflicts (i.e. stating instead that the two parties shall be bound to lend assistance with all their forces to the party attacked), consultation pacts have been considered ‘weaker’ alliances in comparison to – and

often as alternatives to defense and offense pacts. If we assume that states choose their levels of alliance commitments deliberately (Koremenos et al. 2001), the effects of having a consultation pact will be different from the effects of having other types of alliances. However, there have been few studies that examine what the consequences of choosing these ‘weaker’ alliances over ‘stronger’ alliances are. This paper explores how consultation pacts affect the likelihood that their members become the targets of militarized disputes, the likelihood that they initiate militarized disputes, and the likelihood that they respond militarily to disputes when targeted, compared to defense pacts. There have been few theoretical explanations explicitly about how consultation pacts affect interstate conflicts, but we can extend the existing – sometimes competing – theoretical arguments in the alliance literature to draw hypotheses regarding the effects of consultation pacts. Based on the theoretical argument that interprets military alliances as costly signals to reveal states’ foreign policy preferences (Smith 1995; 1998; Fearon 1997; Yuen 2009; Benson, Meirowitz, and Ramsay 2014), this paper derives the first hypothesis that potential challengers are less deterred by consultation pacts than defense pacts. Promising a consultation pact creates lower ex post reputational costs than promising a defense pact, which lowers the probability of intervention by the third-party allies, thus reducing deterrence. At the same time, because there are certain costs associated with promising a consultation pact, this paper derives another hypothesis.

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*I do not compare the effects of offense pacts on interstate conflicts to those of consultation pacts in this paper. The main goal of this paper is to test empirically whether signing a consultation pact can be an alternative to signing a defense pact. I argue that the purpose of signing an offense pact is different from signing a defense pact, because offense pacts are not about precommitting to defend alliance partners, but are about precommitting to attack non-alliance partners. In coding the data set, I exclude consultation pacts that is only applicable to non-alliance members’ territory. In this way, I remove the consultation pacts that might be formed instead of offense pacts. Another reason to focus on comparing consultation pacts to defense pacts only is that we need a different theoretical argument about choosing between consultation pacts and offense pacts. For example, there is no need to restrain the offensive behavior of allies when a state forms an offense pact. In fact, it is the opposite: offense pacts are about coordinating to attack the adversaries and about enhancing the capability of joint military exercise (Morrow 1994). Furthermore, there is no clear theoretical mechanism that offense pacts lead to greater or lesser probability of being attacked by other states.*

*There are still ongoing debates about the effects of defense pacts on extended deterrence (Leeds 2003a; Johnson and Leeds 2011; Benson 2011; Kenwick, Vasquez, and Powers 2015; Johnson and Leeds 2015), or the effects of any military alliances on deterrence and restraint (Palmer and Morgan 2006; Senese and Vasquez 2008; Pressman 2008; Johnson and Leeds 2011; Benson 2012)."
that potential challengers are more deterred by consultation pacts than the cases where neither consultation pacts nor defense pacts exist.

Another follow-up question to the above hypotheses is, then, why would states sign consultation pacts, only to enjoy lesser deterrence? Again, extending the argument that alliances are as costly signals, this paper derives the second hypothesis that members of consultation pacts are less likely to take aggressive behavior than members of defense pacts. Sending a signal with lower ex post reputational costs could be related to lesser deterrence, but it may also indicate that the salience of the alliance is lower. Therefore, a state whose ally promises a consultation pact is less likely to take its ally’s assistance for granted and is less likely to behave aggressively than a state whose ally promises a defense pact. Both whether defense pacts facilitate aggressive behavior or not (Smith 1995; Palmer and Morgan 2006; Senese and Vasquez 2008; Johnson and Leeds 2011; Fang, Johnson, and Leeds 2014) and whether the aggressive behavior is ‘desirable’ or ‘unintended’ or not (Snyder 1997; Yuen 2009; Kim 2011; Benson, Meirowitz, and Ramsay 2014) are in question, but this possible tradeoff between extended deterrence and the prevention of emboldening the allies has been considered as one of the motives for the third-party allies to choose to promise consultation pacts over defense pacts.

In this paper, I provide an empirical test of the effects of consultation pacts on MID (Militarized Interstate Disputes: Ghosn, Palmer, and Bremer 2004; Maoz 2005) initiations and on MID escalations. Furthermore, I compare the effects of consultation pacts on interstate conflicts to those of defense pacts. In testing these effects and comparing them to the effects of defense pacts, I utilize Johnson and Leeds (2011)’ data set. They analyze the effects of defense pacts on MID initiations and on MID escalations and find that having a relevant defense pact is associated with lower probabilities of being a target of MIDs but is not associated with higher probabilities of initiating MIDs or responding militarily (to be more specific, lower probabilities of both events). This article contributes to the literature by testing the effects of consultation pacts on the same outcome variables, by testing whether
their results remain the same when we take the consultation pacts into account, and by comparing these two sets of effects.

First, I find that defense pacts are associated with greater extended general deterrence, while consultation pacts do not have any deterrence effects that are distinguishable from the non-alliance cases. Second, I find that defense pacts are not associated with the allies’ increasing chance of initiating a militarized dispute (sometimes associated with decreasing chances), while consultation pacts are associated with the allies’ increasing chance of initiating a militarized dispute. Finally, I find that defense pacts are not associated with the targeted allies’ increasing chance of dispute escalations (sometimes associated with decreasing chances) and consultation pacts are associated with the targeted allies’ decreasing chance of dispute escalations. One concern could be that the model of this paper does not acknowledge that alliances are not formed randomly. Acknowledging that limitation, the results of this paper do not provide a support for the argument that consultation pacts could be alternatives to defense pacts when there is a tradeoff between preventing aggression targeting the allies and preventing the allies’ aggressive behavior.

Empirical tests of the effectiveness of consultation pacts can contribute to the ongoing debate in the alliance literature regarding the effects of alliances (and alliance designs) on interstate conflicts. What this paper can contribute to the literature is that it provides an opportunity to revisit existing theoretical arguments regarding the effects of alliances, by extending the logic for another alliance obligation – relatively unexplored to other alliance obligations – and checking whether these arguments are applicable to consultation pacts. Therefore, this work could provide an opportunity for future works explaining the reasons for states to form consultation pacts.

Secondly, examining the effects of consultation pacts allows us to understand a considerable number of military alliances. According to the ATOP (Alliance Treaty Obligations and Provisions) data set (Leeds et al. 2002), alliance agreements whose obligation is only consultation account for 13% of all alliances between 1815 and 2003. Furthermore, more than 40%
of military alliances involve a defensive obligation, which is considered as the alternative to the consultation obligation in this paper. Finally, investigating the effectiveness of consultation pacts can provide useful policy implications. For policymakers who are interested in forming an alliance, how to design an alliance agreement is critical. This paper provides empirical evidence of the effects of consultation pacts and compares it to the empirical evidence of the effects of defense pacts. Therefore, this paper could serve as a reference in their decisions about alliance designs.

2.2 Theory and Hypotheses

States form military alliances, which are “formal agreement[s] among independent states to cooperate militarily in the face of potential or realized military conflict” (Leeds et al. 2002, 4). States can provide military assistance and coordinate their foreign policies with others with or without a formally written alliance agreement, so why would states sign alliances? Existing formal models in the alliance literature suggest that military alliances can work as costly signals a state can use to credibly communicate with other states regarding the state’s foreign policy preferences (Morrow 1994; Smith 1995; Fearon 1997; Yuen 2009; Benson, Meirowitz, and Ramsay 2014). A member of an alliance suffers costs of establishing an alliance relationship with its allies, sometimes costs of policy coordination and coordinated practices during peacetime, and costs of violating alliance terms when invoked. Because a state would not be willing to pay these costs if it is not interested in cooperating with its allies, the state’s statement of commitment to its allies is more convincing than a state’s similar statement of commitment without any cost.

Fearon (1997) explains two specific mechanisms through which a state can send costly signals to reveal its commitment to its allies regarding militarized conflicts. First, a state can reveal its commitment by publicly declaring that it will fulfill the alliance commitment when the alliance obligation is invoked. For example, a state can declare that it will intervene
when its ally becomes a target of a militarized dispute by publicly precommitting to defense (ex post costs: tying-hands costs).

This does not induce costs during peacetime, but the leaders are incentivized to keep their statement due to the reputational costs – domestically and internationally – of failing to fulfill a commitment (Gaubatz 1996; Fearon 1997; Mattes 2012a; 2012b). Domestically, political leaders are incentivized to not violate publicly made commitments by domestic audiences, who want to maintain the international reputations of their states (Fearon 1994). Furthermore, particularly in democracies (or in states with many veto players), political leaders also care about their personal reputations so that they can prove that they are credible when they work with other policymakers. Sticking to their foreign policy commitments is certainly one way to show that they are credible negotiation partners. Finally, also particularly in democracies, voters want their political leaders to respect the rule of law and sometimes disapprove of violating international commitments (Tomz 2007; Mattes 2012a).

Internationally, violating publicly made commitments affect the state’s reputation as a negotiation partner in the future. States can sometimes bluff, but after the other side knows that it was a bluff, it will negatively affect the future communications. Therefore, because states want to communicate with the others credibly and want the others to think that they are credible, they have an incentive to honestly communicate (Sartori 2005). In the alliance context, bluffing can be characterized as signing an alliance and not fulfilling the commitment. One of the biggest concerns in alliance commitments is that these alliance commitments themselves do not have any binding power. Therefore, whether a (potential) alliance partner is credible or not affects the other states’ willingness to sign an alliance (Gibler 2008; Crescenzi et al. 2012) or their request for provisions that increase alliance reliability (Mattes 2012b). Because of these domestic and international reputational costs, the state is more likely to intervene should conflicts occur and this increased probability of intervention changes the potential challengers’ and the allies’ (who are also potential targets) payoff. Both theoretical argument and empirical evidence support the idea that alliances that
impose ex post costs are credible in general. Morrow’s (1994) game theoretic model of alliance commitment shows that sometimes bluffing – precommitting to defense but do not intervene when the alliance is invoked – can occur, but only under certain circumstances (Morrow 1994, 289; 293-4). Most of recent empirical studies have also found that allies do not violate alliance obligations when invoked in most circumstances and these credible alliances can have impacts on states’ interstate conflict behavior (Leeds 2003a; Leeds 2003b; Johnson and Leeds 2011; Wright and Rider 2014; Johnson 2015), while some disagree (Gartzke and Gleditsch 2004).

Second, a state can reveal its commitment by investing costs (i.e. mobilizing troops or building arms) during peacetime. Because the costly actions are taken place in advance (ex ante costs: sunk-costs), it does not affect the state’s decision to intervene or not when conflicts occur, but the state could signal its resolve to potential challengers under incomplete information. Furthermore, investing these costs during peacetime can also make the alliance commitment more credible by enhancing the allies’ ability to fight jointly (Morrow 1994), which increases the allies’ probability of intervention when conflicts occur. By incurring either or both of these costs associated with alliances, an alliance member can affect the amount of demand that potential challengers make and the target’s reservation point.

However, not all alliances are the same and different alliance obligations incur different levels of costs. Consultation pacts, one type of alliance commitment, are formal agreements between states that require their members to communicate and encourage them to develop coordinated responses in the event of conflict. They are clearly different from alignments, which are about sharing common interests in either supporting or opposing other states in future interactions but are not formal agreements, thus do not provide a sense of obligations to

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5When the defender sometimes can figure out the allies’ types according to the alliance tightness (the level of peacetime coordination among allies) they form while other times cannot (semi-separating equilibrium), some states have an incentive to form tighter alliance than their optimal level so that the defender cannot differentiate them from the other perfect deterrence alliance (deterrence is achieved). In the former case, the allies do not intervene should war occur, while in the latter case they do. The potential challenger also knows this (though it cannot tell the exact state types of the allies), so the potential challenger does not find it credible when it observes the tightest alliance that the allies can form.
the parties (Snyder 1997). These consultation pacts are often compared to the fully committed alliances such as defense pacts or offense pacts and are considered as alternatives to the other two full-commitments. Both consultation pacts and defense pacts (and offense pacts) have similarities in that both require policy coordination when the alliance is invoked. However, consultation pacts are different from defense pacts and offense pacts because the latter oblige alliance members to provide active assistance in cases of conflicts, while the former does not require active military assistance and provides more policy discretion. Therefore, the costs of fulfilling the consultation obligation are lower than the costs of fulfilling the defense or offense obligation. Furthermore, the violation of the defensive obligation is easier to observe than the violation of the consultation obligation. In this context, consultation pacts are ‘weaker’ and ‘less clear’ alliance obligations than defense pacts or offense pacts: their level of commitment is lower and they intentionally impose greater uncertainties around the level of allies’ military support.

We can connect these varying degrees of alliance commitments to the argument that alliances are costly signals, especially to the tying-hands costs argument. Both consultation pacts and defense pacts are precommitting alliance members to take coordinated actions when casus foederis arises, thus both are about tying the alliance members’ hands. However, because the degrees of commitments are different between consultation pacts and defense pacts, they do not impose the same amount of reputational costs. As discussed earlier, because consultation pacts do not promise active military support explicitly should conflicts occur as defense pacts (and offense pacts – see footnote 3) do, the ex post reputational cost induced by precommitting to consultation is lower than the reputational cost induced

\footnote{An alliance can incur both ex ante and ex post costs. For example, a defense pact is often accompanied with provisions of troop mobilization. Using an ordinal measure of the level of peacetime military coordination (Leeds and Anac 2005), defense pacts are empirically associated with higher levels of peacetime military coordination. Furthermore, Johnson, Leeds, and Wu (2015) show that for the sample of defense pacts, higher levels of peacetime military coordination are associated with greater extended general deterrence. Therefore, ex ante costs could be another source of greater deterrence of defense pacts, but to my understanding, there is no clear theoretical mechanism that links defense pacts to higher levels of peacetime military coordination. Therefore, this paper focuses on comparing the amount of ex post reputational costs incurred by defense pacts and incurred by consultation pacts.}
by precommitting to defense. This difference in the amount of reputational costs between consultation pacts and defense pacts could cause any difference in the effects of these two alliance types on interstate conflicts.

However, there have been few studies in the alliance literature that examine how consultation pacts could affect states' decision to initiate interstate conflicts, compared to defense pacts. Recent studies (Mattes 2012a; Chiba, Johnson, and Leeds 2015) suggest that consultation pacts could be formed for domestic political reasons. Mattes (2012a) argues that leaders in democracies have an incentive to form defense pacts while they are in office, especially when they are likely to lose office in the near future and/or their potential rivals' policy preferences are considerably different from theirs. Because the costs of foreign policy change are greater in democracies, incumbents in democracies have an incentive to pre-commit to their favored policy while they are in office. Therefore, incumbent leaders in democracies are more likely to promise defense pacts – costlier to the state and thus more difficult to form – instead of consultation pacts, when their rivals' preferences on foreign policy are considerably different from theirs and/or incumbent leaders are likely to lose elections soon.

In addition, Chiba, Johnson, and Leeds (2015) argue that because leaders in democracies are more sensitive to the costs of violation, they are more careful in designing alliances, thus they are more likely to form consultation pacts or defense pacts with specific conditions instead of promising defense pacts without any condition. Even in the above studies, a causal link between the reasons to form consultation pacts and the effects of consultation pacts is nuanced. Based on the argument that the degree of commitment is lower when states sign consultation pacts than when states sign defense pacts, this paper provides a set of hypotheses regarding the effects of consultation pacts on interstate conflicts, especially on deterring potential challengers and preventing the allies’ initiation as well as escalation of militarized disputes.
2.2.1 deterrent effect

Huth (1999, 26) defines deterrence as “the use of threats by one party to convince another party to refrain from initiating some course of action.” Furthermore, Huth provides the typology of deterrence. Depending on 1) whether the deterrence is to prevent attacks against a state’s territory or against other states’ territory (direct or extended deterrence) and 2) whether the deterrence is to respond to preceding acts of aggression or to prevent potential acts of aggression (immediate or general deterrence), there are four types of deterrence. In this article, I focus on measuring the effects of consultation pacts and defense pacts on conflict initiations, especially for the cases where at least one ally is interested in engaging in its signatories’ territory (extended deterrence) and 2) when the ally is interested in preventing potential threats (general deterrence). Therefore, this paper aims to test the effects of alliance commitments on extended-general deterrence. This paper agrees with the idea that alliance members could work as a subset of potential defenders we can identify with ease (especially for defense pacts, but also similarly for consultation pacts) (Johnson and Leeds 2011; Johnson, Leeds, and Wu 2015).

Because defense pacts are alliance commitments to provide active military assistance if their members’ territory becomes a target of militarized disputes, there is a good reason to believe that defense pacts are formed to prevent aggression targeting the allies. Applying the argument that alliances work as costly signals, we can assume a situation where there is a potential challenger, a potential target, and a third-party defender. The defender has an interest in preventing aggression against the potential target and is willing to precommit to alliance obligations to reveal its preference.

In this situation, the most straightforward way to show that the defender is interested in defending the potential target is to sign a defense pact that is applicable to the potential target’s territory. The defender’s precommitment to defense imposes reputational costs to the defender, which changes the payoffs of the defender when conflicts occur. Because the defender will suffer reputational costs when casus foederis is invoked and it fails to intervene,
the defender’s chance of intervention increases compared to cases where there is no defense pact. Because of the increased probability of multilateral war, the potential challenger is now worse-off due to the defensive commitment and is more likely to make smaller demands or less likely to make demands at all (Smith 1995; 1998; Morrow 1994; Leeds 2003a; Johnson and Leeds 2011). Whether the targets with defense pacts are more likely to resist is in question both theoretically and empirically (Smith 1995; 1998; Yuen 2009; Fang, Johnson, and Leeds 2014), but in general, most studies in the alliance literature agree with the idea that defense pacts place their allies in better bargaining positions.

In this context, could precommitting to consultation be an alternative to precommitting to defense? Fearon (1997)’s game theoretic model on deterrence argues that signing a consultation pact cannot be an alternative to signing a defense pact when the defender seeks for greater deterrence by signing an alliance. The model is not specifically about the obligation to consult, but the concepts in the model – “partial commitments” or “half-hearted commitments” – can be matched to consultation pacts because consultation pacts incur lower ex post costs than defense pacts (in this case, full commitments) do.

Fearon (1997) argues that these partial commitments cannot be effective at deterring potential challengers and the defender could be better off by precommitting itself to full commitments for two reasons. First, partial commitments do not convey credible signals to audiences, because not promising a full commitment will be considered as revealing the reluctance of the state to make full commitment. In other words, it only sends a signal that the defender is not serious enough about defending the potential target. Second, there could be concerns about emboldening the targets by promising a full commitment, but even in that case, the defender can promise a conditional full commitment to prevent the target’s aggressive behavior (Fearon 1997; Kim 2011). Therefore, this argument provides predictions that defense pacts can be associated with better deterrence, but consultation pacts may not be associated with any deterrence at all, even though there are certain reputational costs associated with violating consultation pacts. Therefore, Fearon’s model (1997) introduces
an interesting puzzle: if consultation pacts are not effective at deterrence, why would states sign consultation pacts often?

However, it is very difficult to argue that states impose any amount – greater or fewer – of reputational costs only to find that investing the costs is of no use. If states know that consultation pacts do not contribute to deterrence, they would decide not to form any alliance to minimize the costs. Furthermore, Fearon’s model argues that if potential challengers observe consultation pacts, they would think that the defender is not interested in fully defending the potential target, thus no effect on potential challengers’ decision to initiate a conflict. However, because the defender could not have promised any alliance commitment, precommitting to consultation can still send a signal about the aligned interests between the defender and the potential target to potential challengers.

Because consultation pacts do not necessarily provide active military support and violating consultation commitments is difficult to observe, the reputational costs associated with violating the consultation pacts are lower than those associated with violating defense pacts. Therefore, precommitting to consultation still imposes reputational costs to the defender, which still changes the payoffs of the defender when conflicts occur, but to a lesser degree. Therefore, the defender’s chance of intervention increases compared to cases where there is no defense pact, but is smaller than cases where there is a defense pact. Again, the potential challenger updates its beliefs about the defender’s chance of intervention compared to no alliance, the potential challenger is more likely to make smaller demands or less likely to make demands at all. On the other hand, when compared to defense pacts, the potential challenger is more likely to make larger demands or more likely to make demands when it observes the defender’s precommitment to consultation. This argument leads to the first set of hypotheses on the deterrent effect:

H1-1: A potential challenger is less likely to initiate militarized conflicts against a potential target, if the potential target is a member of at least one relevant consultation pact, compared to no alliance.
H1-2: A potential challenger is more likely to initiate militarized conflicts against a potential target, if the potential target is a member of at least one relevant consultation pact, compared to defense pacts.

2.2.2 Initiation Effect

As discussed above, consultation pacts incur fewer ex post reputational costs than defense pacts. This difference in the amount of reputational costs could also affect an ally’s decision to initiate a militarized dispute against other states. Promising a full commitment is a strong signal that the state cares its allies very much, which could help the allies to achieve greater deterrence. However, the signal is not only sent to the potential challengers, but is also sent to the alliance partners. Knowing that their allies are willing to make a full commitment to protect them, the alliance partners might think that the state will come to an aid not only for the cases that are specified in the alliance agreement, but also for the other cases where the military assistance of the state is not specified in the treaty.

For example, one can suspect that even though a defense pact is to be invoked only if the allies are attacked by particular adversary, not if the allies attack the adversary first, the allies could be more likely to initiate military conflicts against the adversary: knowing their salience to their alliance partners, they expect the military assistance of allies in virtually any cases. This is clearly beyond the alliance terms, but the other allies may provide military assistance in this case. In other words, when a state is promised a defensive obligation by the other state, then the state learns that the other state is interested in defending it in accordance with alliance terms. At the same time, the state can think that the other state might provide assistance beyond the alliance terms, because the state knows that their interests are aligned closely. This concern has been framed as a problem of ‘entrapment’ (Snyder 1984; 1997), which means that allies can be brought into unwanted multilateral wars because of the emboldened alliance partners.
This argument of ‘the greater the commitment, the greater the risk’ has been considered in the alliance literature as a reason for states to consider forming a consultation pact as an alternative to forming a defense pact. While defense pacts send strong signals to both potential challengers and the allies regarding the salience of allies, consultation pacts send the same kind but weaker signals to both potential challengers and the allies. By watering down the strength of the signals – promising a consultation pact instead of a defense pact –, a state can circumvent the aggressive behavior of its allies. Achieving deterrence could be directly proportional to the strength of the signal to potential challengers, but the restraining effect could be inversely proportional to the strength of the signal to the allies. Therefore, defense pacts could be preferred to consultation pacts by states who prioritize the deterrent effect at the cost of the restraining effect, and consultation pacts could be preferred by states who prioritize the restraining effect of allies in sacrifice of the deterrent effect. According to the argument, the allies of a consultation pact are more likely to initiate militarized conflicts compared to states without either defense pacts or consultation pacts, because consultation pacts still incur certain reputational costs, which signals relatively low – compared to defense pacts – but certain levels of salience of the allies to their alliance partners. This argument provides the second set of hypotheses:

**H2-1**: A potential challenger is more likely to initiate militarized conflicts if the potential challenger is a member of at least one consultation pact, compared to no alliance.

**H2-2**: A potential challenger is less likely to initiate militarized conflicts if the potential challenger is a member of at least one consultation pact, compared to defense pacts.

Palmer and Morgan (2006) also argue that forming an alliance – without distinguishing alliance types – could increase the likelihood that its allies initiate a militarized dispute, but with a different theoretical mechanism. Palmer and Morgan (2006) argue that states allocate their resources to achieve two foreign policy goals, maintenance and change. In their argument, a militarized dispute initiation is a mean for a state to pursue changes in the status quo. Because military alliances provide extra resources to the allies (or allow
the allies to enjoy the same level of policy goals with spending fewer resources), alliance members are ‘liberated’ and could invest these resources to additional foreign policy regarding either maintenance or change. In this regard, the allies seeking change could initiate a new militarized dispute. Therefore, compared to no alliance cases, a potential challenger is more likely to initiate militarized conflicts if the potential challenger is a member of either a relevant consultation pact or a relevant defense pact. Palmer and Morgan (2006) do not discuss consultation pacts specifically, but we can extend their logic and apply it to consultation pacts. Based on their logic, because defense pacts provide more security benefits than consultation pacts, the signatories of defense pacts can invest greater amount of extra resources to additional foreign policy than the signatories of consultation pacts. Therefore, we can argue that a potential challenger is more likely to initiate militarized conflicts if the potential challenger is a member of a defense pact, compared to cases with consultation pacts.

There are several alternative explanations about the initiation effects of defense pacts and consultation pacts, compared to no alliance. First, several scholars (Fearon 1997; Kim 2011) argue that if a state is concerned about its ally’s aggressive behavior, it could limit the scope of alliance agreement so that such ‘emboldenment’ would not occur. Therefore, unless states do not have a full control of designing alliances – which seldom occurs – consultation pacts could not be preferred to defense pacts just to avoid encouraging aggressive behavior at the cost of reduced deterrence. In other words, why would states form consultation pacts when carefully designed defense pacts (i.e. conditional defense pacts) could be better at deterrence and could restrain their allies as consultation pacts do? Therefore, Kim (2011) argues that states will let their allies to take more aggressive behavior only when they want them to do so. Therefore, this argument suggests that a potential challenger is not particularly more likely to initiate militarized conflicts if the potential target is a member of either a relevant consultation pact or a defense pact, compared to no alliance. Johnson and Leeds (2011) find supporting evidence of this argument: a potential challenger who is a member of a defensive
alliance is not associated with higher chance of initiating disputes; rather, a member of a defensive alliance is less likely to initiate a dispute.

Second, the “steps-to-war” argument of Senese and Vasquez (2008) provides another different prediction about the initiation effect. Based on the idea of security dilemma and power politics, they suggest that a pair of states with territorial issues is more likely to escalate the conflict into wars than other issues, because territorial issues are one of the most salient issues. If one of these two states forms an alliance that is applicable to the territorial issues, the other side will search for counter-alliance to balance. This repeated alliance formation (and arms race) can continuously increase the probability of militarized disputes between the two states, which could lead to dispute initiations. Therefore, this argument expects that a potential challenger is more likely to initiate militarized conflicts if the potential challenger is a member of either a relevant consultation pact or a relevant defense pact, compared to no alliance.

2.2.3 Escalation Effect

As discussed above, the initiation effect does not come from the alliance commitments, but comes from revealing the aligned interests between the defender and the potential target by precommitting to certain alliance obligations. In other words, defense pacts or consultation pacts do not have any independent effect on dispute initiations by the signatories: rather, alliance commitments are indicators of latent interests among the signatories when we discuss the initiation effect.

However, this was not the case in the discussion about the deterrence effect. Still, the underlying interests are likely to affect alliance choices, but the effects of alliance commitments on extended general deterrence comes from the alliance, which imposes costs of violation. For the escalation effect, the increase in the probability of intervention by the allies, given that the alliance is invoked, is important because the allies will actually suffer the costs when
the targeted ally escalates the dispute and its allies fail to intervene.

Therefore, it could be the case where a state will promise a defense pact knowing that the alliance agreement could make its allies more resistant to potential challengers’ demands. Several formal models of military alliances (Morrow 1994; Smith 1995) argue that the targets of militarized disputes are more likely to resist if they have an ally who promises to defend them because of the increased probability of the ally’s intervention. This increased probability of multilateral war also increases the probability of winning when fought jointly, compared to the situation where the targeted allies fight solely with the potential challengers. However, one cannot argue that this is undesirable at all times because this increased resistance is part of the process of achieving better deterrence. Benson et al. (2014) argue that the ‘emboldened’ behavior of alliance partners may discourage the potential challengers from making a demand against the alliance partners, because the potential challenger expects higher probability of resistance with the alliance. Second, similar to the argument above regarding the initiation effect, the targeted allies can learn that their alliance partners value the alliance relationship with them when they are promised a defense pact. Therefore, not necessarily related to specifics of alliance agreements, they could be more resistant to the demands of potential challengers, expecting that their allies will aid them anyway.

H3-1: A target state of militarized conflicts is more likely to resist if the target is a member of at least one relevant consultation pact, compared to no alliance.

H3-2: A target state of militarized conflicts is less likely to resist if the target is a member of at least one relevant consultation pact, compared to defense pacts.

However, it is worth noting that there is no scholarly consensus about whether alliance commitments make the targeted ally more aggressive. Yuen (2009) argues that promising a third-party assistance does not necessarily facilitate the targeted ally’s resistance, because knowing that the probability of multilateral war increased with the commitment of support, the challenger reduces the amount of demand to the target in the first place. Therefore, stronger challengers who make much larger demands are more likely to be deterred than
weaker challengers who make smaller demands, and because only smaller demands are made in most cases, the target is likely to concede. According to this argument, a third-party’s commitment to intervention could achieve better deterrence not by facilitating the targeted ally’s aggressive behavior, but by reducing the size of the demand and deterring stronger challengers who make greater demands. Therefore, because a defense pact increases the third-party’s chance of intervention more than a consultation pact, this study predicts that promising a defense pact would provide a bargaining range more favorable to the target and deter stronger challengers than promising a consultation pact. In conclusion, the chance that the target will concede when it has a defensive ally is at least as likely as when it has an ally who promises consultation. In other words, there is a selection effect on the challenger’s making demands at all or the degree of demands. Because the challenger does not make a demand that are likely to be rejected by the target, we would not see many cases of dispute escalation by the attacked allies once the challenger initiates a dispute.

Finally, one additional argument against the Snyder’s argument of choosing consultation pacts to avoid entrapment could be ‘the greater the commitment, the greater the leverage.’ It is true that allies bear the increased chance of intervening on behalf of their allies, but the fact that the allies invest their resources also gives them a leverage to influence their allies’ behavior. Recent studies have suggested that losing (or the threat of losing) the existing alliance relationship with their allies could be an important factor of predicting their behavior. This could be more important when the targeted allies value the alliance relationship (Fang, Johnson, and Leeds 2014). Even though their argument is not about different degrees of alliance commitments, one can extend the argument and apply it to the comparison of the effects of alliance types.

Let us assume that a state can either dissuade or persuade its alliance partners from taking aggressive behavior through making an advice to the targeted allies. When a potential challenger makes a demand, the targeted allies can persuade the potential target to accept or reject the demand. If the target does not follow the allies’ recommendation, it would damage
the target’s relations with the allies. Whether the target will follow the recommendation depends on many factors, and one of them could be the degree of alliance commitment. Because losing a defense pact could be costlier than losing a consultation pact to the target, ceteris paribus, the allies’ advice to refrain from aggressive behavior could have more impact on the targeted ally’s behavior when the alliance partners have promised a defense pact than when they have promised a consultation pact. Conversely, the allies’ advice to resist could also have more impact on the targeted ally’s behavior when the alliance partners have promised a defense pact. Therefore, promising a defense pact would not necessarily increase the chance of its allies to escalate the conflicts when attacked, compared to a consultation pact. It will depend on the allies’ recommendation (whether to resist or to concede) and the target’s cost of losing alliance relationship, but if we can only focus on the cases where the allies recommend concession to the target, defense pacts could be better at restraining alliance partners than consultation pacts.

2.3 Research Design

In testing the effects of consultation pacts on MID initiations and in comparing those effects to the effects of defense pacts, I utilize the replication data set of Johnson and Leeds (2011). The unit of analysis is a directed dyad-year, which assumes a pair of a potential challenger and a potential target. The states included in the data set are the ones in the Correlates of War project state system membership and the time period for this analysis is from 1816 to 2000.

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At the same time, one can argue that defense pacts incur greater costs of violation or termination led by one side, a threat to terminate a defense pact might not be as credible as a threat to terminate a consultation pact. In other words, stronger alliance commitments create bargaining power by increasing the dependence of the allies on the alliance, but at the same time, “the more firmly one is committed to the alliance, the less credible, and therefore the less effective, are threats to withdraw support from the ally or abandon the alliance” (Snyder 1997, 168). This article does not provide a clear explanation about these arguments. Investigating this relationship further will remain as a future work.
The main difference between this research and Johnson and Leeds (2011) is that this article adds a separate independent variable that indicates whether there is any ally who promises a relevant consultation pact to the potential target in a given year. I use the information from the ATOP data set (Leeds et al. 2002) and adopt the coding rules of Johnson and Leeds (2011) in coding relevant consultation pacts and the list of allies who promise relevant consultation pacts. There is one additional coding rule built for coding relevant consultation pacts – regarding the applicable location of alliances – that will be explained in the following paragraphs.

The details of coding rules for relevant consultation pacts are as follows: First, in parallel to the coding rules of Johnson and Leeds (2011, 52-3), I remove all secret consultation pacts when I build the independent variable used for testing the deterrent effect. On the other hand, when I build the independent variable used for testing the initiation and the escalation effect, I do not remove secret consultation pacts. Second, I code asymmetric consultation pacts as having these effects only in cases where there is at least one ally who promises to consult with the member when the alliance is invoked. Third, consultation pacts conditional on specific adversaries are coded as applicable only in cases where the potential challenger is the adversary described in the alliance text. Fourth, consultation pacts limited to particular ongoing conflicts are applicable only to the potential challengers who are involved in the conflict and are on the opposite side, according to the COW interstate war data set.

Furthermore, I add the fifth rule for coding relevant consultation pacts: consultation pacts limited to specific locations are coded as having such effects only for alliance members in the specified location. This fifth rule does not exist in the coding rules of Johnson and Leeds (2011). By the definition of a defensive alliance in the ATOP data set, every defensive alliance is about “the sovereignty or the territorial integrity of one or more alliance partners” (Leeds 2005, 21). However, some consultation pacts that are conditional on specific locations address concerns about the sovereignty or the territorial integrity of non-members. For example, the understanding between Russia and the United Kingdom in 1844 is signed to
consult on a common course of action regarding the internal order of the Ottoman Empire. In this case, the consultation pact is not applicable to cases in which either Russia or the UK is the potential target.

On the other hand, the Mediterranean agreement among Italy, the United Kingdom, and Austria-Hungary in 1887 states that the signatories will consult on the status quo in the Mediterranean, the Adriatic, the Aegean Sea, and the Black Sea. Because Italy and Austria-Hungary are located in the region, this consultation pact is applicable when either Italy or Austria-Hungary is the potential target. However, because the UK is not located in the region, this alliance is not applicable to cases in which the UK is the potential target. By utilizing this coding rule, I can sort out consultation pacts possibly with ‘offensive’ purposes, which is not the primary concern of this research. Finally, I code all remaining consultation pacts as applicable to all potential challengers in a given year.

In order to compare the effects of consultation pacts to those of defense pacts, I code an alliance that has both defense and consultation obligations as a defense pact. In other words, alliances that promise defense (possibly also promise consultation or other alliance obligations) are coded as defense pacts and alliances that promise consultation but not defense (possibly with other alliance obligations) are coded as consultation pacts. An alternative to this approach is to argue that having consultation pacts and having defense pacts (or having an alliance that encompasses both categories) will have separate effects, and can be estimated by adding the effects of the two. In this way, one could interpret that defense pacts and consultation pacts could have effects on deterrence and/or conflict escalation separately.

However, the theoretical argument of this paper is based on the idea that consultation pacts can be alternatives to defense pacts and they differ in the amount of reputational costs they incur. Therefore, it makes more sense to assume that there is a clear hierarchy between the two alliance obligations, and the effect of consultation pacts, in this case, will be subsumed by the effect of defense pacts. Following the argument, I utilize the independent variable that codes defense pacts from Johnson and Leeds (2011). Furthermore, I code the
independent variable that indicates consultation pacts one only if there is no relevant defense pact but there is a relevant consultation pact. For all the other situations (when there is a relevant defense pact but no consultation pact, when there is a relevant defense pact and a consultation pact, and when neither of them is present), the consultation pacts variable is coded zero.

All the other model choice strategy, including the choice of control variables, of this paper is the same as those of Johnson and Leeds (2011). Because the outcome is a binary indicator variable, I use a probit model for testing the deterrent effect and the initiation effect. Furthermore, because only the targeted states are provided with an opportunity to respond, the variable of target resistance cannot be coded at least there is one dispute. Therefore, I use a censored probit model for testing the escalation effect.

2.4 Results

Table 1 shows the results of the probit analysis of dispute initiation. The first column shows the results of Johnson and Leeds (2011). All results presented in Table 1 and Table 2 assume that consultation pacts are alternatives to defense pacts and there is a clear hierarchy. Again, it means that if there is at least one ally who precommits to defense for the potential target, regardless of whether the ally also promises consultation or not and regardless of whether there are any other allies who promise consultation or not, the observation is coded having a defense pact.

**For alliances that require multiple obligations, as long as one of the obligations is consultation and there is no defensive obligation, I code them consultation pacts. There could be cases where the consultation obligation is accompanied with the other alliance obligations, such as offense, neutrality, and nonaggression. However, I control for the first two alliance obligations by including two independent variables that indicate whether the potential challenger has an offense pact and whether the potential challenger has a neutrality pact. There is no clear theoretical argument that nonaggression pacts are associated with increasing or decreasing chances of dispute initiation outside of the sample of signatories (Mattes and Vonnahme 2010; Lupu and Poast 2016). Furthermore, as discussed in the research design section, I do not code consultation pacts whose alliance obligation is beyond the signatories’ sovereignty. These decisions can be helpful when sorting out the effects of consultation pacts on interstate conflicts and comparing the effects to those of defense pacts.
First, the results in Table 1 show that when a potential target has a relevant consultation pact but does not have a defense pact, the probability of being attacked is not significantly different from the probability when there is neither of them. Second, when a potential target has a relevant defense pact, the probability of being attacked is significantly lower than that when there is neither of them. This result conforms to the finding of Johnson and Leeds (2011) regarding the deterrent effect. Including the independent variable that indicates consultation pacts does not change the findings regarding the deterrence effect of defense pacts.

Comparing the effects of consultation pacts to those of defense pacts could be difficult by simply reading the coefficients. Furthermore, drawing inference from coefficients directly when the outcome variable is a binary indicator can be challenging, too. Therefore, examining the substantive effects of the results could be particularly helpful in these circumstances. Figure 1 shows the differences in predicted probabilities of MID initiations by potential challengers by alliance choices. The error bars represent the 2.5th and the 97.5th percentiles of the simulated predicted probabilities and the dots in the center of the error bars represent their median values. In order to simulate the predicted probabilities of dispute initiation by potential challengers, I follow the steps described in Tomz, Wittenberg, and King (2003) and conduct a pairwise comparison of three sets of predicted probabilities. To construct a representative case, I use the mean values of continuous independent variables (ex. COW CINC scores, the logged value of distance, etc.) and median values of binary indicator independent variables (ex. the existence of a relevant defense pact, joint democracy status, etc.).

First, starting with the error bar at the bottom, the predicted probabilities of dispute initiations by potential challenger when the potential target has a defense pact are considerably lower than the predicted probabilities when the potential target has neither of them. 

††The predicted probabilities are calculated for three distinct cases: when there is no alliance, when there is at least one relevant consultation pact but no defense pacts, and when there is at least one relevant defense pact. After simulating the coefficients and calculating these three sets of predicted probabilities, I subtract one from either of the two for pairwise comparisons.
Therefore, we can see that the difference between the two sets of predicted probabilities are negative and significantly different from zero. Second, the error bar in the middle shows the difference in predicted probabilities of dispute initiation by potential challenger when the potential target has a consultation pact but not a defense pact and when the potential target has neither a defense pact nor a consultation pact. As suggested by the coefficient in Table 1, having a consultation pact is associated with fewer chances of dispute initiations compared to no alliance cases, but the difference between the two is not distinguishable from zero. Finally, if we compare the deterrent effects of consultation pacts to those of defense pacts, the difference tends to be negative, but still not distinguishable from zero. What do these results suggest then? Putting these three sets of effects in order, having neither of them does not enjoy any deterrent effect, having a consultation pact but not a defense pact might enjoy some deterrent effect that is not distinguishable from zero, and having a defense pact enjoys the highest deterrent effect, significantly different from the cases where there is neither of them.

Regarding the initiation effect, the results in Table 1 suggest that consultation pacts are actually associated with higher probabilities of dispute initiation by the alliance member, compared to no alliance cases. Furthermore, they also suggest that there is no significant impact of defense pacts on emboldening the alliance partners. The coefficient is negative but not significantly different from zero. This result is different from that of Johnson and Leeds (2011), because they find clearer negative relationship between having a defense pact and the probability of dispute initiation by the alliance members. Both of these results do not support the hypotheses regarding the initiation effect. The empirical results suggest that consultation pacts have the highest initiation effect, cases where there is neither a defense pact nor a consultation pacts are in the middle, and defense pacts have the lowest initiation effect, though the difference between the latter two is indistinguishable.

Figure 2 shows the differences in the predicted probabilities of dispute initiation by alliance members, in pairwise comparison. As suggested by the coefficients in the model,
defense pacts are associated with lower probabilities of dispute initiation, but the effect is almost close to zero. If we compare the effects of consultation pacts to those of cases where neither of them is present, the alliance members of consultation pacts are more likely to initiate conflicts. Finally, if we compare the effects of consultation pacts and the effects of defense pacts, because defense pacts are associated with the lowest probability of dispute initiation by the allies and consultation pacts are associated with the highest probability of dispute initiation by the allies, defense pacts are better at preventing conflict initiations by their allies, compared to consultation pacts.

Table 2 shows the results from the censored probit model of dispute initiation by potential challengers and dispute escalation by the targeted allies. First, compared to cases where neither of them is present, the targeted alliance member of a relevant defensive alliance is less likely to escalate the dispute. The finding conforms to that of Johnson and Leeds (2011) and the size of coefficient in my model is greater than that of Johnson and Leeds (2011). Second, compared to cases where there is neither a consultation pact or a defense pact, the targeted alliance member of a relevant consultation pact (but not a relevant defense pact) is also less likely to respond to the dispute militarily. In summary, the results do not support any of the two hypotheses regarding the escalation effect. Defense pacts are associated with higher chance of dispute escalation than consultation pacts, which one of the hypotheses predicts, but the difference is not distinguishable from zero.

Figure 3 shows the difference in predicted conditional probabilities of dispute escalation, given that a militarized conflict is initiated. Because this paper uses a censored probit model for testing the escalation effect, calculating the predicted probabilities needs additional steps. Simulating the coefficients and calculating the expected values \((X\beta)\) for the selection stage and for the outcome stage separately are identical to the process of creating Figure 1 and Figure 2. In order to see how the unit change in the independent variable affects dispute escalations, I include the additional term of the inverse-Mills ratio multiplied by the correlation between the error terms in the selection stage and those in the outcome stage.
Following the process, I link the corrected expected values to the cumulative distribution function of the standard normal distribution. I repeat this process for each alliance choices and conduct a pairwise comparison.

As we can see in Figure 3, both consultation pacts and defense pacts are associated with lower probabilities of dispute escalation by the targeted allies given that there is at least one militarized conflict, compared to cases where there is neither of them. In addition, the difference between the effects of consultation pacts and the effects of defense pacts on dispute escalation given the initiation of a militarized dispute is not distinguishable from zero. The effects of consultation pacts on preventing dispute escalation are slightly greater than those of defense pacts, but the difference is not considerable. This result could be in accordance with the argument made in Fang, Johnson, and Leeds (2014), arguing that the more an alliance member values the alliance relationship, the more the allies’ recommendation for acquiescence encourages the member’s concession.

2.5 Conclusion

In conclusion, the empirical evidence does not support the argument that consultation pacts can serve as alternatives to defense pacts by incurring fewer reputational costs very well. There are two things to consider for further examination. First, this could suggest that states form consultation pacts for different reasons, not necessarily to trade-off the allies’ emboldenment with extended general deterrence. As suggested by the empirical results, states with consultation pacts are actually associated with higher probabilities of conflict initiation, and even more interestingly, lower probabilities of conflict escalation compared to states with defense pacts. Therefore, the argument that precommitting to partial commitments can outperform precommitting to full commitments in preventing allies’ aggressive behavior could be misleading. This also provides support to Fearon’s argument that consultation pacts do not lead to better deterrence, but could be formed for other reasons.
Second, ignoring the process of alliance formation stage could provide biased estimates. One concern about this empirical test is that I do not take the nonrandom assignment of treatment – that alliance designs are not chosen randomly – into account. As discussed above, forming and maintaining a military alliance is a very costly action, so states (or political leaders of those states) make decisions to form, maintain, revise, or even terminate alliances deliberately. Furthermore, it is very likely that factors of treatment assignment – alliance type choices – are also factors of the outcome variable, in this case dispute initiations. For example, a state who is threatened by other states has more incentives to search defensive allies, while the level of threat is also an important factor of whether the state becomes a target of militarized disputes or not.

If one can argue that after controlling for all observable independent variables alliance type choices are uncorrelated with MID initiations, then there would be no bias in estimates. However, this is not often the case, especially in observational studies. There are many factors that could affect both treatment assignments and the outcome variable, but some of them might not be fully observable. It could be because the data quality is very low, because there could be serious measurement errors, because the data set is not available at all, and/or because the concept is very difficult to measure inherently. For example, measuring a political leader’s subjective assessment of specific threat could be extremely difficult, even though it could have an important impact on the leader’s decisions regarding military alliances and militarized disputes. In this case, because of the set of common unobservable independent variables, the model of alliance type choices and the model of militarized dispute initiation could be correlated. Therefore, overlooking the model of alliance type choices and running a single regression of militarized disputes initiation on alliance type choices could provide biased estimates, thus either underestimating or overestimating the effects of various alliance types on dispute initiations. This could be one reason to observe empirical results that do not conform to the hypotheses, which remains as a future work.
Table 2.1 : Probit Analysis of Dispute Initiation: Consultation Pacts as Alternatives to Defense Pacts

<table>
<thead>
<tr>
<th>Model</th>
<th>Johnson and Leeds (2011)</th>
<th>Model 1</th>
</tr>
</thead>
<tbody>
<tr>
<td>deterrent effect of a defense pact</td>
<td>(-0.062^{**})</td>
<td>(-0.066^{**})</td>
</tr>
<tr>
<td></td>
<td>(0.015)</td>
<td>(0.016)</td>
</tr>
<tr>
<td>deterrent effect of a consultation pact</td>
<td>(-0.021)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td></td>
</tr>
<tr>
<td>Initiation effect of a defense pact</td>
<td>(-0.068^{**})</td>
<td>(-0.028)</td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td>(0.017)</td>
</tr>
<tr>
<td>Initiation effect of a consultation pact</td>
<td>(0.138^{**})</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.025)</td>
<td></td>
</tr>
<tr>
<td>Potential challenger has a relevant offensive alliance</td>
<td>(0.276^{**})</td>
<td>(0.263^{**})</td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td>(0.029)</td>
</tr>
<tr>
<td>Potential challenger has a relevant neutrality pact</td>
<td>(0.315^{**})</td>
<td>(0.285^{**})</td>
</tr>
<tr>
<td></td>
<td>(0.025)</td>
<td>(0.026)</td>
</tr>
<tr>
<td>Challenger’s likelihood of winning</td>
<td>(0.161^{**})</td>
<td>(0.148^{**})</td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
<td>(0.023)</td>
</tr>
<tr>
<td>Distance</td>
<td>(-0.384^{**})</td>
<td>(-0.383^{**})</td>
</tr>
<tr>
<td></td>
<td>(0.006)</td>
<td>(0.006)</td>
</tr>
<tr>
<td>Similarity in alliance portfolios</td>
<td>(-0.455^{**})</td>
<td>(-0.449^{**})</td>
</tr>
<tr>
<td></td>
<td>(0.041)</td>
<td>(0.041)</td>
</tr>
<tr>
<td>Joint democracy</td>
<td>(-0.155^{**})</td>
<td>(-0.162^{**})</td>
</tr>
<tr>
<td></td>
<td>(0.027)</td>
<td>(0.027)</td>
</tr>
<tr>
<td>Constant</td>
<td>(0.827^{**})</td>
<td>(0.799^{**})</td>
</tr>
<tr>
<td></td>
<td>(0.068)</td>
<td>(0.068)</td>
</tr>
<tr>
<td>Observations</td>
<td>1,077,992</td>
<td>1,077,992</td>
</tr>
<tr>
<td>Pseudo $R^2$</td>
<td>0.160</td>
<td>0.161</td>
</tr>
</tbody>
</table>

(Notes: Standard errors in parentheses; ** p<0.001, * p<0.01, ‘p<0.1; peace years, peace years$^2$, peace years$^3$ included in estimation)
Table 2.2: Censored Probit Analysis of Dispute Initiation and Resistance 1816-2000: Consultation Pacts as Alternatives to Defense Pacts

<table>
<thead>
<tr>
<th>Target resistance</th>
<th>Johnson and Leeds (2011)</th>
<th>Model 1</th>
</tr>
</thead>
<tbody>
<tr>
<td>Escalation effect of a defensive alliance</td>
<td>-0.103’ (0.048)</td>
<td>-0.141* (0.050)</td>
</tr>
<tr>
<td>Escalation effect of a consultation pact</td>
<td></td>
<td>-0.265* (0.093)</td>
</tr>
<tr>
<td>Potential challenger has a relevant offensive alliance</td>
<td>-0.241* (0.084)</td>
<td>-0.251* (0.084)</td>
</tr>
<tr>
<td>Potential challenger has a relevant neutrality pact</td>
<td>-0.261* (0.075)</td>
<td>-0.243* (0.076)</td>
</tr>
<tr>
<td>Challenger’s likelihood of winning</td>
<td>-0.304** (0.074)</td>
<td>-0.314** (0.074)</td>
</tr>
<tr>
<td>Constant</td>
<td>1.672** (0.119)</td>
<td>1.705** (0.120)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Dispute Initiation</th>
<th>Johnson and Leeds (2011)</th>
<th>Model 1</th>
</tr>
</thead>
<tbody>
<tr>
<td>deterrent effect of a relevant defensive alliance</td>
<td>-0.060** (0.015)</td>
<td>-0.065** (0.016)</td>
</tr>
<tr>
<td>deterrent effect of a consultation pact</td>
<td></td>
<td>-0.019 (0.029)</td>
</tr>
<tr>
<td>Initiation effect of a defensive alliance</td>
<td>-0.066** (0.015)</td>
<td>-0.028 (0.017)</td>
</tr>
<tr>
<td>Initiation effect of a consultation pact</td>
<td></td>
<td>0.128** (0.024)</td>
</tr>
<tr>
<td>Potential challenger has a relevant offensive alliance</td>
<td>0.276** (0.029)</td>
<td>0.263** (0.029)</td>
</tr>
<tr>
<td>Potential challenger has a relevant neutrality pact</td>
<td>0.315** (0.025)</td>
<td>0.288** (0.026)</td>
</tr>
<tr>
<td>Challenger’s likelihood of winning</td>
<td>0.161** (0.022)</td>
<td>0.148** (0.023)</td>
</tr>
<tr>
<td>Distance</td>
<td>-0.383** (0.006)</td>
<td>-0.383** (0.006)</td>
</tr>
<tr>
<td>Similarity in alliance portfolios</td>
<td>-0.449** (0.041)</td>
<td>-0.443** (0.041)</td>
</tr>
<tr>
<td>Joint democracy</td>
<td>-0.168** (0.027)</td>
<td>-0.174** (0.027)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.818** (0.067)</td>
<td>0.793** (0.068)</td>
</tr>
<tr>
<td>Observations</td>
<td>1,077,992</td>
<td>1,077,992</td>
</tr>
<tr>
<td>Uncensored observations</td>
<td>2,354</td>
<td>2,354</td>
</tr>
<tr>
<td>Rho</td>
<td>-0.588 (0.052)</td>
<td>-0.526 (0.037)</td>
</tr>
</tbody>
</table>

(Notes: Standard errors in parentheses; ** p<0.001, * p<0.01, ’p<0.1; peace years, peace years$^2$, peace years$^3$ included in estimation)
Figure 2.1: Differences in the Probabilities of Dispute Initiation by Potential Challengers (deterrent effect)
Figure 2.2: Differences in the Probabilities of Dispute Initiation by Alliance Members (Initiation Effect)
Figure 2.3: Differences in the Conditional Probabilities of Dispute Escalation by Targeted Allies (Escalation Effect)
Chapter 3

Taking Alliance Formations Seriously: testing the effects of defense pacts on dispute initiation with an endogenous treatment model

3.1 Introduction

Since the seminal piece of Koremenos, Lipson, and Snidal (2001) on the design of international institutions, almost every article about institutional designs assumes that states choose institutional features deliberately. In other words, international institutions are not formed randomly; military alliances are no exceptions. This concern about nonrandom choices of international institutions has almost always been raised in the alliance literature. Many scholars have argued that states form alliances with other states in the expectation of affecting their or others’ decisions to initiate, escalate, intervene in, or terminate interstate conflicts. However, the argument that states do not form institutions randomly has raised a crucial concern about empirically testing the effects of international institutions on state behavior. How can we examine the effects of military alliances on interstate conflicts, acknowledging the fact that alliances are the results of states’ deliberate foreign policy choices?

There have been novel developments in both developing theoretical arguments and collecting data in the alliance literature that allow researchers to capture the alliance formation process better, but there have been few studies that address the nonrandom alliance formation process methodologically. Because conventional methods such as multivariate regressions or matching methods assume conditional independence of alliance formation after controlling for the observables, they do not resolve the concern about the nonrandom process of alliance formation.
To address the nonrandom process of alliance formation better, I adopt a statistical model that accounts for endogenous treatments and test the effects of defense pacts on the initiation of militarized interstate disputes (MIDs). The statistical model utilized in this paper addresses the non-random assignment of alliance formation by allowing correlations between the treatment assignment stage and the outcome stage even after controlling for the observable confounders. The correlations can exist due to the set of common unobservable confounders, which affect both the formation of defense pacts and the initiation of MIDs. By estimating this system of equations, we can have less biased estimates of the effects of defense pacts on states’ decision to initiate militarized disputes.

I test the effects of defense pacts on the initiation of MIDs in two ways: First, I test whether a member of defense pacts is more likely to become a target of MIDs. This refers to the deterrent effect of defense pacts. Second, I test whether a member of defense pacts is more likely to initiate MIDs. This test speaks to the concern about the “emboldening effect” of defense pacts. In conducting the empirical tests, I utilize a binary treatment model built by Miranda and Rabe-Hesketh (2006). The model assumes that there are unobservable confounders that affect both the decision to form defense pacts and the decision to initiate MIDs. A failure to control for these unobservable confounders (and assuming that alliance formation and the initiation of MIDs are conditionally independent after controlling for observable confounders) is a potential source of biased estimates, which is the methodological concern described in the above paragraph.

Utilizing the endogenous treatment model, I find the following results. First, the results of this paper suggest that states who have at least one defensive ally are at higher risk of being targets of MIDs and are also at higher risk of being initiators of MIDs, compared to the states who do not have defensive allies. As many previous studies have suggested, there could be a systematic difference between the members of defense pacts and the states who do not have defense pacts, which is attributable to unobserved confounders (the omitted variable bias issue). In other words, states who expect higher chances of being involved in
conflicts are more likely to form or join defense pacts – even with more policy concessions in exchange for an alliance (Johnson 2015) –, while states who do not see high chances of being involved in interstate conflicts have fewer incentives to seek alliances actively. Second, taking the difference between the signatories and the nonsignatories of defense pacts into account, having at least one defensive ally is associated with lower chance of being targets of MIDs, compared to the nonsignatories of defense pacts. That is, I find supporting evidence of the deterrence effect of defense pacts. Third, acknowledging the difference between the members and the non-members of defense pacts, having at least one defensive ally is also associated with lower chance of being initiators of MIDs, compared to the case where there is no defensive ally. Contrary to the concerns about emboldening the alliance partners, the results in this paper suggest the opposite: I do not find support for the initiation effect of defense pacts. On the other hand, this result might suggest that alliances can work as leverages to restrain alliance partners (Fang, Johnson, and Leeds 2014). As shown in the results section, conventional probit models underestimate the effects of defense pacts on the initiation of MIDs. Therefore, utilizing the endogenous treatment model can be useful if a researcher is concerned about the nonrandomness of treatment assignments.

This paper can contribute to the literature both methodologically and substantively. First, this paper contributes to the literature by providing an example of utilizing the endogenous treatment model in testing the effects of international institutions. This model can be applicable to many studies that model institutions, which rarely assume that a researcher can observe a complete set of common confounders of the institutional choice (treatment) and states’ behavior (outcome). Furthermore, this paper can contribute to the literature by providing additional empirical evidence to the ongoing debate about the effects of defense pacts on interstate conflicts (Leeds 2003; Johnson and Leeds 2011; Kenwick, Vasquez, and Powers 2015; Leeds and Johnson 2017; Kenwick and Vasquez 2017). Because the model by Miranda and Rabe-Hesketh (2006) only allows for binary endogenous treatment and does not allow dependence between observations (i.e. hierarchical model), the choice of independent
variables and the unit of analysis are different from those of the earlier works. At the same time, this paper introduces more rigorous estimation strategies to investigate the relationship between the formation of defense pacts and the initiation of MIDs.

### 3.2 Theory and Hypotheses

#### 3.2.1 Perspectives on the effects of defense pacts

Alliances are “formal agreement[s] among independent states to cooperate militarily in the face of potential or realized military conflict” (Leeds et al. 2002, 4). Defense pacts differ from other alliance types by obligating their members to provide active military assistance to an ally when the ally’s sovereignty or territorial integrity is attacked. (ATOP codebook, 9). Therefore, defense pacts are only invoked when at least one of the allies become a target of a military conflict but are not invoked when one of the allies attack other states. These defense pacts constitute a considerable portion of alliances. According to the ATOP data set (Leeds et al. 2002), defense pacts constitute 40% of all alliances in the data set. Given that a large proportion of alliances are about committing to defend the allies, understanding how defense pacts affect interstate conflicts with more rigorous methodological approach is important.

#### 3.2.2 The bargaining model approach: alliances as costly and credible signals

Of course, any state can provide military assistance to another state regardless of whether the state has committed to defend the other state who became a target of disputes, so why do states sign alliances? Scholars in the alliance literature have argued that alliances are costly signals to credibly convey information to the others about their members’ preferences on foreign policy (Morrow 1994; Smith 1995, 1998; Fearon 1997; Yuen 2009). By imposing
costs of violating alliance commitments on themselves, states can credibly reveal their foreign policy alignments with other states. Furthermore, military coordination during peacetime can enhance the effectiveness of joint fighting, thus increases the chance of winning in multilateral wars. This is another source of the increase in the allies’ probability of intervention. Therefore, alliances can be more than a mere indicator of aligned interests; alliances can have an impact on states’ conflict behavior.

In this context, defense pacts can send a credible signal to potential challengers that alliance members are interested in protecting the other members’ territorial integrity or sovereignty. Knowing that alliance members have common interests and the chance of alliance members’ intervention increases by committing to defense, potential challengers become less likely to make demands. This is called the deterrent effect of defense pacts. Many empirical studies on defense pacts show that defense pacts are generally reliable and are associated with decreasing chance of being a target of militarized disputes (Leeds 2003; Johnson and Leeds 2011; Wright and Rider 2014; Johnson, Leeds, and Wu 2015; Gartzke and Gleditsch (2004) is an exception, while Benson (2011) finds the deterrent effect for a subset of defense pacts).

**H1-1: A potential challenger is less likely to initiate militarized conflicts against a potential target, if the potential target is a member of at least one relevant defense pact, compared to states without a defense pact (deterrent effect).**

Even though most of the arguments based on the bargaining framework agree that making a commitment to defend a state decreases the chance of the state’s being a target of interstate conflicts, these arguments have not reached a consensus about the effects of defense pacts on their members’ likelihood of initiating a militarized dispute. The argument that considers defense pacts as credible signals regarding defense explicitly suggests that there is no side effect of defense pacts on the allies’ likelihood of initiating a militarized dispute. In other words, because defense pacts are only for defending the allies’ sovereignty or territory, defense pacts are not invoked when the allies attack other states and thus should have no effect on
the allies’ likelihood of initiating militarized disputes. If the allies wanted to cooperate on attacking other states and want to increase their chances of winning, they could have also committed themselves to the obligation to provide offensive support, with or without the commitment to provide defensive support. Therefore, the probability of initiating militarized disputes can be better explained by having offense pacts, not necessarily by having defense pacts. Johnson and Leeds (2011) find supporting evidence for this argument: they find that being a member of defense pacts does not increase its probability of attacking the others; in contrast, they find that the probability of a member of defense pacts initiating a militarized dispute is lower than that of a state without defense pacts.

H2-1: A potential challenger is as likely to initiate militarized conflicts if the potential challenger is a member of at least one defense pact, compared to states without a defense pact (no initiation effect).

On the other hand, Palmer and Morgan (2006) argue that having a defense pact could lower the chance of being a target of interstate conflicts, but there is a side effect of having defense pacts; having defense pacts could increase the allies’ chance of attacking other states. Having a defense pact can increase one state’s security without investing as many resources as it should without the defense pact, so the members of a defense pact can use the remaining resources to pursue additional foreign policy. This additional investment in foreign policy can be about changing the status quo, so in this case, the members of a defense pact will be more likely to initiate militarized disputes. This has been referred to as one source of the side effects of defense pacts, namely the initiation effect.

*Palmer and Morgan (2006) do not distinguish alliance types; their argument about alliances being either maintenance-seeking or change-seeking is not about types of alliance commitments, but about the alliance member’s relative capabilities compared to its allies. At the same time, I agree with the idea of Johnson and Leeds (2011, 48), which argues that because defense pacts are specifically about protecting the sovereignty of the allies, defense pacts are more likely to promote maintenance than change. Lowering the chance of being a target of interstate conflicts is one of the maintenance-seeking foreign policy goals.

†Again, Palmer and Morgan (2006) argue that any alliances will let the signatories to pursue additional foreign policy goals. Therefore, their argument should be applicable to a subset of all alliances, defense pacts in this case. In their empirical analysis, they find that joining the alliance is associated with the increasing probability of MID initiation, compared to non-allied cases (Palmer and Morgan 2006, 158).
**H2-2:** A potential challenger is more likely to initiate militarized conflicts if the potential challenger is a member of at least one defense pact, compared to states without a defense pact (initiation effect).

### 3.2.3 The steps to war: alliances as triggers of the security dilemma

In contrast, the steps-to-war argument by Senese and Vasquez (2008) assumes that forming any alliances, including defense pacts, increases both the chance of being a target of interstate conflicts and the chance of initiating interstate conflicts. According to their argument, any alliance formation makes the alliance members’ potential adversaries feel insecure, thus eventually increases the insecurity of the alliance members, instead of decreasing it. If one state forms an alliance, even in cases where the alliance members clearly state that the alliance is solely for defense, it raises other states’ security concerns by changing the distribution of national capabilities in aggregate. These concerned states would seek military alliances to bring the altered bargaining structure back or would increase their military capacity. These efforts to counter the change in the status quo could result in arms races or repeated alliance formations, which ultimately increase the perception of threat between the two parties and reduce the chance of compromise. Therefore, states belong to either of the two parties are more likely to have hardliners as their political leaders and can easily become hostile. As a result, interstate disputes are more likely to be initiated by either of the two parties. Ironically, Senese and Vasquez (2008) provides the prediction that the efforts to increase security can undermine the security.

**H1-2:** A potential challenger is more likely to initiate militarized conflicts against a potential target, if the potential target is a member of at least one relevant defense pact, compared to states without a defense pact (no deterrent effect).
3.2.4 Caveats for the exogenous treatment assumption

Recent exchanges by Leeds and Johnson (2017) and Kenwick, Vasquez, and Powers (2015) as well as Kenwick and Vasquez (2017) contributed to the literature by (re)examining the deterrent effect of defense pacts, using different research designs. For example, Kenwick, Vasquez, and Powers (2015) decide to include the first five years after the formation of a defense pact in their data set, while Johnson and Leeds (2011) and Leeds and Johnson (2017) include all the years since the formation of a defense pact until its termination. Of course, both agree that the difference in their research design is mainly driven by the difference in their theoretical arguments. While the works of Leeds and Johnson (2011; 2017) aim to test general deterrent effect, Kenwick, Vasquez, and Powers (2015) and Kenwick and Vasquez (2017) attempt to test the immediate effect of the formation of defense pacts. Given that their theoretical motivations and their research designs are different, it is not surprising to see different empirical results regarding the effect of defense pacts on the initiation of MIDs.

Acknowledging the difference in their theoretical arguments and research designs, I would like to point out another methodological concern about the findings of these recent debates (and the findings of most of the studies that examine the effects of alliances on interstate conflicts): the exogenous treatment assumption. In other words, both works by Leeds and Johnson (2011; 2017) and works by Kenwick, Vasquez, and Powers (2015; 2017 for Kenwick and Vasquez) assume that the treatment – the formation (and continuation) of defense pacts – is not correlated with the decision to initiate militarized conflicts, after controlling for other independent variables. With the exogenous treatment assumption, they both adopt regression analyses and matching techniques as their estimation strategies. Both of these strategies are able to sort out certain dependence between the formation of defense pacts and the initiation of interstate disputes.

However, it is worth questioning that whether making the exogenous treatment assumption after controlling for the observables is justifiable or not. If any researcher cannot come up with a full set of control variables (that are identifiable), the failure to control for un-
observable confounders can only partially remove correlations between the decision to form defense pacts and the decision to initiate militarized disputes. Depending on the correlation structure, the remaining but overlooked correlation between the error terms in the defense pact formation equation and the error terms in the dispute initiation equation will either drive the estimates of the treatment effect upward or downward (Clarke 2005). Both sets of studies acknowledge that there are potential confounding factors that they have not controlled for, and they can inhibit discovering the relationship between having a defense pact and interstate dispute initiation. Despite their rigorous efforts, a potential source of bias may still remain. By utilizing the endogenous treatment model, this paper attempts to account for the potential bias caused by the failure to control for unobservables.

3.2.5 Taking alliance formations seriously: the endogenous treatment assumption

3.2.6 Alliance Formations affected by both observables and unobservables

If we assume that international institutions are established on purpose, to achieve particular policy goals, then we need to understand the process of forming international institutions first. Based on our understanding of the formation process and conditioning on the process, a researcher can further investigate the effects of these institutions on state behavior. In alliance examples, it would not make sense if one argues that states sign alliances for no particular reason. As discussed above, forming and maintaining a military alliance is a very costly decision, so states (or political leaders of those states) would be extremely deliberate when they decide to form, maintain, revise, or even terminate alliances.

Many empirical studies on alliance formation have argued that alliances, including defense pacts, are not formed randomly. The decisions to form alliances are likely to be affected by several factors, including the level of external threat (Leeds et al. 2002; Gibler and Sarkees 2004; Johnson 2015), a state’s own capabilities as well as its capabilities relative to the
capabilities of its alliance partners (Morrow 1991), the similarity of interests (Lai and Reiter 2000; Chiba, Johnson, and Leeds 2015 on alliance designs), and the credibility of the alliance (Leeds and Morgan 2012). At the same time, these factors are also good predictors of whether a state becomes a target of militarized disputes. As the levels of external threat increases, the state is more likely to be a target of interstate conflicts. In addition, some studies suggest that democracies may respond differently to disputes (Reiter and Stam 2002; Bueno de Mesquita et al. 2003) and states are more likely to form alliances with other states who share similar interests, but are less likely to initiate disputes targeting them (Signorino and Ritter 1999). Finally, the relative contribution of allies in terms of aggregated capabilities and the similarity of interests among allies make defensive alliances more capable and credible, thus deterring potential challengers (Johnson, Leeds, and Wu 2015).

As long as we can identify these confounding variables and have good measures of these variables, we can simply control for these variables and we can be confident about our understanding of the effects of defense pacts on interstate disputes because the “assignment” of defense pacts becomes random after controlling for these variables. Most of observational studies in the alliance literature that adopt either a matching method or a single-equation regression analysis make this conditional independence assumption. However, it is hard to meet the conditional independence assumption in observational studies. There are many factors that affect both alliance formation and the initiation of interstate conflicts, but some of them might not be fully observable. This omitted variable bias issue has been a major concern for researchers who conduct empirical analysis.

There are several reasons for omitting independent variables in empirical analyses: First, measurement errors and measures that are not robust prevent us from having reliable results. Every possible error in the process of creating a data set – imperfect coding rules, intercoder differences, and mere coding mistakes, etc. – is a source of unreliable empirical results, not only for the estimate of the variable, but also for the estimates of the other independent variables. Furthermore, for the variables whose operationalizations are difficult, having “ro-
bust” measures of the variables is difficult. For example, IR scholars have put much effort to operationalize the concept of external threat and have built data sets that capture the concept. Some studies provide a direct measure of the level of external threat (Bennett 1997; Leeds and Savun 2007; Chiba, Johnson, and Leeds 2015) while others identify a set of states (or dyads) that are more threatening to others (or with higher levels of baseline threat, i.e., rivalries). Of course, because these measures capture and often emphasize particular aspects of the levels of threat, some measurements can be more appropriate than others, depending on research questions. At the same time, given the broad definition of external threat and given the differences in coding rules across data sets, choosing one measure over the other is a tough choice and could yield very different empirical results. By assuming that these measures could be imperfect and let the model capture the remaining dependence (if there is any), researchers can have more robust results.

Second, some control variables are extremely difficult to capture. For example, other than the level of external threat, a political leader’s subjective assessment of threat is also likely to affect the leader’s decision to form a defense pact and the decision to initiate a militarized dispute. These two concepts can capture different dynamics; the subjective assessment of threat can differ across political leaders and their personal characteristics can affect this subjective assessment, while the level of external threat is mainly about the characteristics at the country level, at the dyadic level, or at the system level. Still, measuring the subjective assessment of threat, especially across years, is almost impossible. Conducting surveys or interviews on political leaders may not be feasible for every researcher, but there is no guarantee that the leaders will respond honestly, and worse yet, we can only collect limited information about their subjective assessment from documents if they are deceased.

Of course, one should be careful when she includes an additional control variable in a model. First and foremost, a researcher should have a good theoretical reason for including the variable. Pearl (2009) provides a good guidance on deciding which variable a researcher should control for; controlling for a variable should block a “backdoor path,” not opening
one. Furthermore, even though researchers have a good theoretical argument that the additional control variable is a confounding factor, as long as there is a risk of omitted variable bias, introducing a new control variable into the model may increase or decrease the bias in estimated effects of the treatment variable (Clarke 2005), depending on the correlation structure between independent variables. Clarke (2005) emphasizes the importance of rigorous research design as a practical suggestion, which is perfectly valid. At the same time, we can also think about a statistical model that estimates the correlation structure. The literature of nonrandom sample selection provides us a hint about that possibility.

3.2.7 Addressing nonrandom alliance formation as an omitted variable bias problem

Since Heckman (1979), there has been significant progress in extending his sample selection model and making them readily available to researchers in built-in commands in various statistical software. Interestingly, the concern of sample selection models is the same as the concern about omitted variable bias: there are unobservable confounding factors that affect both the sample selection process and the outcome variable of interest. The nonrandom sample selection can be problematic only if there are unobservable confounders. Similarly, the nonrandom assignment of treatment will lead to biased estimates only if there are unobservable confounding factors that affect both the treatment assignment and the outcome of interest.

The sample selection models address the issue of unobservable confounders – omitted variable bias in estimating both the selection equation and the outcome equation – by allowing the error terms in the two equations to be correlated. In other words, the expectation of error terms in one equation conditional on the error terms in the other equation is not zero. The model assumes that the correlation between two sets of error terms come from the common unobservable factors. For example, a set of common unobservables can increase
the probability of being selected and can also increase the expected value of the outcome of interest; in this case, we can see positive correlations between the two sets of error terms. If the common unobservables that increase the probability of being selected decreases the expected value of the outcome of interests, we will see negative correlations.

In the context of alliances, if a noisy measure of the level of external threat can only partly capture the “true” level of external threat, even after controlling for the external threat level by using the best measure possible, the correlation between the decision to form alliances and the decision to initiate militarized disputes can still remain. Assuming that higher levels of external threat increases the chances of forming a defense pact and the chances of being a target of MIDs, the remaining correlation would be positive and different from zero. Because the states who seek (and thus form) defense pacts have higher baseline threat level (and higher baseline probability of being a target of MIDs), overlooking the positive correlation would underestimate the deterrent effect of defense pacts.

In the same way, it is possible to build a model with two equations, the one that predicts treatment assignment and the one that predicts the outcome of interest and let their error terms be correlated. The major difference between sample selection models and the endogenous treatment model is that in the endogenous treatment model, the outcome variable in the first equation – predicting treatment assignment – explicitly becomes the main independent variable in the second equation. In addition, in the endogenous treatment model, all observations present in the first stage are present in the second stage.

Different from this, von Stein’s work (2005) indirectly captures the nonrandom treatment effect by estimating separate outcome equations across two different treatment values. The endogenous treatment model is in accordance with the idea of von Stein’s (2005), but only estimates one outcome equation. The endogenous treatment model can be more efficient than the von Stein’s approach because it estimates fewer coefficients given the same amount of data. On the other hand, von Stein’s approach is more appropriate if one assumes that the data generating processes are substantially different across different treatment values. In
this paper, I assume that the effects of other control variables on the initiation of interstate conflicts do not significantly vary across treatment status. For example, I hypothesize that the level of external threat is positively associated with the probability of dispute initiation, for both the signatories and the nonsignatories of defense pacts, largely to the same degree. If one can assume that the signs of effects are opposite or the effect sizes considerably vary across the treatment status, using the von Stein's approach (at the cost of efficiency) can be more appropriate.

Some recent studies in the IR literature adopt a two-part model (2PM: Vance and Ritter 2014; Chiba, Johnson, and Leeds 2015). This model estimates both processes simultaneously, but the model also makes the conditional independence assumption. Because the 2PM assumes that the two processes are uncorrelated after controlling for observable confounders, there is no need to meet the exclusion restriction. This assumption is both the advantage and the limitation of the model; substantively, they will yield the same results as the ones from two separate regressions because the two equations are conditionally independent. As described above, this endogenous treatment model can do better than the 2PM model (also hurdle models, which are special cases of the 2PM model) because the correlation between the two sets of error terms (alliance formation equation and the dispute initiation equation) does not necessarily have to be zero. Therefore, researchers could be better off by using the endogenous treatment model. In addition, as discussed later, the exclusion restriction does not have to be met for the endogenous treatment model with discrete outcomes.
3.3 Modeling strategy: endogenous treatment model

3.3.1 Explanation of the endogenous treatment model

In this article, I adopt the endogenous treatment model of Miranda and Rabe-Hesketh (2006). They develop a Stata package ssm\(^\dagger\) which allows researchers to estimate endogenous treatment models or sample selection models using maximum likelihood. Again, their package can address either of these cases (either endogenous treatment model or sample selection models) because both issues are essentially the same problem: the failure to control for unobservable confounders. Their endogenous treatment model takes binary treatment variable and binary, ordinal, and count variables for the outcome of interest. They call their model “endogenous switching” because depending on the value of the binary treatment, either zero or one, the model switches to a model where both treatment and outcome variables are “stacked” for the treated or to another model for the untreated.

Miranda and Rabe-Hesketh (2006, 287-9) introduce their endogenous switching model as follows:

\[
y^*_i = x_i \beta + \theta S_i + u_i \\
y_i = \begin{cases} 
1 & \text{if } y^*_i > 0 \\
0 & \text{otherwise}
\end{cases}
\]

\[
S^*_i = z_i \gamma + v_i \\
S_i = \begin{cases} 
1 & \text{if } S^*_i > 0 \\
0 & \text{otherwise}
\end{cases}
\]

where \(y_i\) is the binary outcome of interest and \(S_i\) is the endogenous binary treatment. In

\(^\dagger\)Their package is accompanied with another Stata package gllamm.
this paper, \( y_i \) is the MID initiation and \( S_i \) is being a member of a defense pact. As discussed, the correlated error terms, \( u_i \) and \( v_i \), can be expressed as functions of common error terms; this logic is similar to estimating random effects. Therefore, the conditional mean of \( u_i \) given \( v_i \) or the conditional mean of \( v_i \) given \( u_i \) would not be zero. In this way, they establish the dependence between the two error terms. The variance of the error terms in the treatment selection equation is equal to two if one uses a logistic link function; the variance equals one if one uses a probit link function. Depending on the reparameterization of the variance, the estimated coefficients should be rescaled, too.

\[
\begin{align*}
  u_i &= \lambda \epsilon_i + \tau_i \\
  v_i &= \epsilon_i + \zeta_i
\end{align*}
\]

\[
\text{Cov}\{(u_i, v_i)\}' \equiv \Sigma = \begin{pmatrix} \lambda^2 + 1 & \lambda \\ \lambda & 2 \end{pmatrix}
\]

Finally, the full model for the conditional mean of the “stacked” variable \( q_{ij} \) is as follows:

\[
\eta_{ji} = g_j(E(q_{ij}|\epsilon_i)) = d_{1ji}(x_i \beta + \theta S_i + \lambda \epsilon_i) + d_{1ji}(z_i \gamma + \epsilon_i)
\]

where \( g_j \) is the link function for the outcome of interest and the endogenous treatment. The link function for the outcome of interest is a probit function, while the link function for the endogenous treatment can be either a logistic or a probit function. In this paper, I use the probit link function for both equations.

Recently, Stata also added endogenous treatment models similar to this (etieffects; StataCorp 2015), but these built-in models cannot take both a binary treatment variable and a binary outcome variable. In this paper, we want to test whether being a member of a defense
Impact (binary: member or non-member) has an impact on the initiation of disputes (binary: dispute initiation or no dispute). Therefore, it would be more appropriate to use the ssm model to investigate this research question.

3.3.2 Discussion on the exclusion restriction

For every multiple-equations model, the identification issue always concerns researchers. The identification problem is that when a researcher derives estimates from a system of linear equations (seemingly unrelated regressions (SUR) or simultaneous equations models (SEM)), the system of linear equations cannot have the exactly same set of independent variables. For example, in a two-equations setting, the endogenous treatment equation should have at least one independent variable that is not in the outcome equation. The same logic also applies to sample selection models. This is particularly the case when the outcome variables in each equation is expressed as the linear function of the independent variables and the coefficients. Because both the endogenous treatment model and the sample selection model address the dependence between equations by modeling the conditional mean of the error terms of the switching/selection equation, the linear mapping of the error terms conditioning on the same set of independent variables raise concerns about multicollinearity and make the coefficient estimates inconsistent. Therefore, it is highly encouraged to find an identification variable in this case.

However, when the treatment and the outcome are both binary variables, the (parametric) endogenous treatment model can be identified without the exclusion restriction (Heckman 1979; Vella and Verbeek 1999; Miranda and Rabe-Hesketh 2006) because the mapping of the independent variables in the selection/switching equation to the probability of being selected/the probability of receiving the treatment is nonlinear. In other words, the main reason is that by taking the underlying latent variable approach ($y_i^*$ being a latent variable of the dichotomous variable $y_i$), the residuals in the outcome equation would be expressed
in terms of $S^*_i$ (latent continuous variable), not $S_i$ (discrete binary variable). Therefore, it is possible to separately estimate the coefficient of $S^*_i$ – essentially the covariance between the error terms – and the coefficient of $S_i$, without the concern about multicollinearity. Furthermore, the variance of $y^*_i$ and the variance of $S^*_i$ enables researchers to differentiate the residuals from the binary treatment regressor. The criticism about not meeting the exclusion restriction is valid when there is little variation in the independent variables. In this case, they are sufficiently close to constants, but in many empirical cases, this is not the case.

### 3.4 Research Design

This paper conducts a quantitative analysis of the effect of defense pacts on the initiation of militarized interstate disputes. The unit of analysis is a state-year. Previous studies have examined the deterrence effect and the initiation effect usually in directed-dyadic settings, which allow researchers to capture directed-dyadic characteristics between a potential challenger and a potential target. However, I conduct a quantitative analysis on the monadic level data set, a potential target-year for testing the deterrent effect and a potential challenger-year for testing the initiation effect. This is mainly because modeling both the alliance formation and the initiation of MIDs in the directed-dyadic settings can take apart the data generating process (especially the DGP of the alliance formation) more than necessary. For example, defense pacts that do not specify potential challengers are quite common. Once these alliances are formed, the alliances are applicable to every state in the international system if we keep the directed-dyadic settings. However, it is often the case where these alliances have a small number of potential challengers in mind but do not specify the names of potential challengers in the alliance agreement. In this case, most of the other states do not impose significant threat against the state, but are coded as the potential challengers of the alliances. Consequently, even low levels of external threat could be attributed to the
formation of defense pacts if we run a regression in the directed-dyadic setting, which could overestimate the effects of external threats on the formation of defense pacts. This concern is applicable to other directed-dyadic characteristics; their effects on the alliance formation could be either overestimated or underestimated. Collapsing the directed-dyadic data set into monadic ones can alleviate this concern by using the average/overall values of these independent variables.

Related to the above issue, alliances are likely to be clustered (and sometimes duplicated) in the directed-dyadic setting. Again, if a state signs a defense pact that does not specify particular adversary, then the alliance is applicable to all of the directed-dyadic observations whose potential target (or potential challenger) is the state. Unless specified, it makes sense to argue that an unconditional alliance is applicable to any potential challengers, but these ‘treatments’ are all clustered (and are likely to have the same value) for the same the target state, despite variations in the directed dyadic characteristics between the potential target and every potential challenger. This concern might be addressed by using hierarchical models, but the current ssm package does not provide any clustering option. Although I lose much information regarding directed-dyadic characteristics of states, using a monadic-level data set is less susceptible to the clustered treatment assignment issue. The states included in the data set are from the COW (Correlates of War) project state system membership data set (Correlates of War Project 2011). The time period of this analysis is from 1816 to 2000, mainly due to the data availability.

3.4.1 Testing the deterrent effect of a Defense Pact

The dependent variable of the first set of hypotheses is operationalized in two ways. First, I create a binary variable that codes whether there is any MID targeting the state in a given

\[\text{§} \]

\[\text{§This could be particularly problematic for multilateral alliances. In a directed-dyadic setting, the observations whose potential target is a member of NATO consist of 15\% of all directed dyadic observations whose target has at least one defensive ally (Johnson, Leeds, and Wu 2015, 12).} \]
year. I adopt two measures of the binary MID variable. First, I create a binary variable that codes whether there is any MID targeting the state in a given year, regardless of whether the state has any defense pact or the characteristics of the defense pact (if there is any). The results using this measurement are presented in Table 3 and the substantive effects are shown in Figure 4 and Figure 5. In addition, considering the nature of defense pacts, I create an alternative measure of the binary MID variable. For the second measurement, only “relevant” MIDs are coded. For states who do not have any defense pact, all the other states in the COW system are applicable challengers. For states who have a defense pact, applicable challengers are states whose attack can invoke the alliance obligation. If an alliance member becomes a target of a MID but the defense pact is not invoked with the MID, then this MID initiation should not be attributed to the deterrence failure of the defense pact.

This second operationalization can favor my deterrence hypothesis (H1-1) by dropping some MID cases targeting members of defense pacts, so I present the results with the first measure as my main results. The results with the second measure are presented in Table 4. The substantive findings, especially for the endogenous treatment model approach, remain the same. This variable is coded by using Zeev Maoz’s dyadic version of the Correlates of War MID data set (Ghosn, Palmer, and Bremer 2004; Maoz 2005). The MID variable is coded one if any other state threatens, displays, or uses force against the state in a given year. The joiners are not coded because joining disputes that have already occurred is a very different decision from the decision of initiating a dispute (ex. the joiners may participate to fulfill their alliance obligations) and including joiners may attribute deterrence failure to the cause of joining the dispute (Johnson and Leeds 2011, 51).

The key independent variable for testing the deterrent effect is whether there is an ally who promises defensive obligation to the potential target in a given year. First, I employ a binary variable about whether a state has at least one ally who promises relevant defensive obligation in a given year. I utilize the information from the ATOP data set (Leeds et al. 2002), and code relevant defense pacts and relevant allies using the same coding rule of
Johnson and Leeds (2011) and Johnson, Leeds, and Wu (2015). If I find negative coefficient for the defense pact variable in the outcome equation, it provides supports for the hypothesis H1-1.

I also include several control variables which can affect both the initiation of disputes and alliance formation. First, I include the state’s national capability by using the COW CINC (Composite Index of National Capability) index (Singer et al. 1972; Singer 1987). A state’s capability can affect the types of alliances the state can choose and can also affect the probability that the state is attacked. Second, I include the level of threat the state faces in a given year. Measuring the level of threat is a complicated issue; for this paper, I utilize the coding rule of Chiba, Johnson, and Leeds (2015). I start the coding by sorting out the states who are either a major power (COW state system) or are contiguous to the potential target state. Next, using the S-score calculated with the ATOP data set (Chiba, Johnson, and Leeds 2015), I exclude the states who have higher S-scores with the potential target states than the median S-score of the population (0.8421053). As a final step, I aggregate the capabilities of the remaining states and use this measure as a measurement of the threat level. Because the level of threat a state faces is known to motivate alliance formation (Johnson 2015) and the level of threat can be an important factor predicting the initiation of disputes, controlling for the threat level is important.

Third, I control for the number of countries who share borders with the potential target state. Senese and Vasquez (2008) argue that contiguous states are more likely to be involved in conflicts because of contentious territorial issues, which also affects alliance formation. Fourth, I take into account whether the potential target state is democratic or not, using the threshold of six or higher on the Polity2 variable (Marshall, Jaggers, and Gurr 2010). Even though there is no clear consensus about whether democracies are associated with

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*Alliance choices are inherently bilateral or multilateral decision processes, but because of the monadic setting of this empirical test, I only include the state’s capability as a factor of alliance formation and the initiation of disputes.

†It is because these states may not be the source of potential threat, given that they have similar interests to those of the potential target.
peace at the monadic level (Maoz 1997; Russett and Starr 2000), it is possible that a state’s
democracy status affects the probability of being a target of MIDs. Furthermore, Mattes
(2012) and Chiba, Johnson, and Leeds (2015) argue that domestic political situations or
regime types affect alliance formation.

Fifth, I include three count variables regarding the other alliance types: the number of
allies who promises an offense pact, a neutrality pact, or a nonaggression pact to the potential
target state. They are not treated as endogenous treatments, though they are highly likely to
be. Due to the limitations of the available models, I cannot specify more than two categories
for the endogenous treatment variable. At the same time, one can consider these other
alliance obligations as additional factors, because the nature of obligations of these three
alliance commitments is quite different from those of defense pacts. It could be the case
that having a nonaggression pact is likely to affect the state’s probability to be attacked, but
committing to nonaggression is different from committing to defending the alliance partner.
Finally, following the logic of Senese and Vasquez (2008) that territorial claims lead to
increasing chance of militarized disputes, I include a count variable of territorial claims
whose target is the state in a given year (ongoing or new) and another count variable of
territorial claim whose challenger is the state in a given year. I utilize the Issues Correlates
of War Project (ICOW), the provisional version 1.01 of the territorial claims data set (Hensel
et al. 2008; Frederick, Hensel, and Macaulay 2014), mainly due to their coverage of the time
period 1816-2000.

3.4.2 Testing Initiation Effects

The dependent variable of the second set of hypotheses is a dummy variable of whether a
state initiates any MID or not in a given year. Therefore, the unit of analysis is interpreted as
a potential challenger-year. I do not code applicable potential targets because the theoretical
argument about the initiation effect does not argue that there are specific sets of potential
states that are likely to be targeted by the members of defense pacts. Similar to the test of the deterrence effect, this variable is also coded by using Zeev Maoz’s dyadic version of the Correlates of War MID data set (Ghosn, Palmer, and Bremer 2004; Maoz 2005). The dependent variable is coded for initiators, not joiners for the same reason above.

The key independent variable codes whether a state has at least one ally who promises a defensive obligation in a given year. One difference in coding is that this variable also includes secret alliances and does not distinguish relevant and irrelevant alliances, while the variable used for testing the deterrent effect only includes public and relevant alliances. For extended deterrence, a potential challenger should be aware of the alliance so that it can update its knowledge about the allies’ foreign policy preferences. On the other hand, in testing the initiation effect, there is no clear argument that connects specific alliance terms (either secret or public) to the allies’ probability of dispute initiation. Therefore, I code every defense pact in testing the initiation effect. Again, I utilize the information from the ATOP data set (Leeds et al. 2002) and adopt the same coding rule of Johnson and Leeds (2011). For the control variables, I utilize the same set of control variables I use for testing the deterrent effect also for testing the initiation effect and predicting alliance formation.

3.5 Results

3.5.1 deterrent effect

Table 3 shows the empirical results regarding the deterrent effect. The first column shows the results of two separate models: a probit regression of being a target of MIDs and a probit regression of forming a defense pact. The coefficient for the defense pact variable is -0.013, which suggests a negative relationship but not distinguishable from zero at the conventional significance level of \( p < 0.05 \). Therefore, from the single probit regression, one may find that the potential targets who have at least one defensive ally does not necessarily enjoy greater deterrence than the states who do not have any defensive ally. On the other hand,
the second column shows the results of the endogenous binary treatment model. Again, the binary treatment in this model is whether there is at least one ally who promises to defend the potential target in a given year. The coefficient for the defense pact variable in the second column is negative (-0.573) and is distinguishable from zero even at higher significance level ($p < 0.01$). This result provides supporting evidence that defense pacts are associated with lower probability of being a target of MIDs, when we take the nonrandom process of the formation of defense pacts into account.

Why is the coefficient estimate of the defense pact variable in the single probit regression smaller than that in the endogenous treatment model? In order to answer the question, we can check the correlation between the error terms in the dispute initiation equation and those in the alliance formation equation. The correlation is 0.337 and is statistically significant at $p < 0.01$ level. This suggests that the unobservable confounders that facilitate the formation of defense pacts are also positively associated with increasing chance of being a target of MIDs. Revisiting the example of an imperfect measure of the level of threat, the external threat level that cannot be fully captured by the control variable would make a political leader more likely to search for defensive allies. At the same time, the external threat level will also be positively associated with the chance of becoming a target of MIDs. Substantively, this positive correlation suggests that states who form defense pacts are exposed to higher risk of being a target of MIDs. Therefore, ignoring the difference in baseline probability of being a target of MIDs results in underestimating the deterrent effect of defense pacts.

Furthermore, the estimated effects of control variables on dispute initiation is almost the same across models. First, as the national capabilities of a state increase, the state is more likely to become a target of MIDs. This finding is different from the general consensus in the international conflicts literature regarding the capabilities, which usually suggests that increasing national capabilities are associated with greater deterrence. At the same time, there are arguments that major powers are more likely to be involved in interstate conflicts and this could be one factor that drives the result, especially considering the monadic setting
of this research design. As can be seen in Table 5, increasing national capabilities is associated with increasing chance of initiating MIDs greatly (the coefficient is 3.549 for the endogenous treatment model). Of course, being involved in conflicts does not necessarily mean becoming a target of conflicts, so further investigation is needed. Another possibility is that relatively weak states may concede even before the MID initiation, so it is difficult to “observe” more MIDs targeting these relatively weak states.

Furthermore, increasing levels of threat, the number of borders, the number of territorial claims made by the state or the number of territorial claims whose target is the state are positively associated with the probability of being a target of MIDs. These findings conform to the existing theoretical arguments about interstate disputes and are consistent with most of the previous findings. Finally, regarding the effects of the other alliance commitments on the probability of being a target of MIDs, the numbers of allies who promise offense pacts and neutrality pacts do not seem to be associated with the probability of being a target of MIDs. The number of allies who promise nonaggression pacts is positive and statistically significant for the endogenous treatment model. There are few theoretical arguments that connect offense or neutrality pacts to the chance of being a target of militarized dispute, so these results conform to the existing alliance literature. On the other hand, previous results have suggested that nonaggression pacts are associated with lower chance of conflicts (Long et al. 2007; Mattes and Vonnahme 2010). However, direct comparison of results may not be appropriate given the monadic setting of the data.

Because the endogenous treatment model simultaneously estimates two equations whose dependent variables are binary, examining the substantive effects of the existence of defense pacts on dispute initiation is useful. Given the settings of the endogenous treatment model, we want to calculate the difference in predicted probabilities of dispute initiation, conditioning on the formation of defense pacts. In accordance with the other endogenous treatment models (von Stein 2005; Braumoeller et al. forthcoming), there are two possible counterfactuals that we can think of: 1) given that all states have at least one defense pact (baseline
1), we can compare the probability of being a target of MIDs when they have defense pacts to the probability of being a target of MIDs when they do not have any defense pact and 2) given that all states do not have any defense pact (baseline 2), we can compare the probability of being a target of MIDs as they do not have any defense pact to the probability of being a target of MIDs when they have at least one defense pact. Conditioning on the formation of defense pacts (=setting the baseline probability of alliance formation the same) allows us to calculate the counterfactual effect of defense pacts on dispute initiation, even though there is no such case in actual data set. The formulas for the two approaches are given by

Approach 1: Assuming that all states have at least one defense pact

\[
Pr(dispute = 1 with defense | defense = 1) - Pr(dispute = 1 without defense | defense = 1)
\]

\[
Pr(u_i < x_i \beta + \theta_{def} | v_i < z_i \gamma) - Pr(u_i < x_i \beta | v_i < z_i \gamma) = \frac{\Phi_2(x_i \beta + \theta_{def}, z_i \gamma, \rho)}{\Phi(z_i \gamma)} - \frac{\Phi_2(x_i \beta, z_i \gamma, \rho)}{\Phi(z_i \gamma)}
\]

Approach 2: Assuming that all states do not have any defense pact

\[
Pr(dispute = 1 with defense | defense = 0) - Pr(dispute = 1 without defense | defense = 0)
\]

\[
Pr(u_i < x_i \beta + \theta_{def} | v_i > z_i \gamma) - Pr(u_i < x_i \beta | v_i > z_i \gamma) = \frac{\Phi_2(x_i \beta + \theta_{def}, -z_i \gamma, -\rho)}{\Phi(-z_i \gamma)} - \frac{\Phi_2(x_i \beta, -z_i \gamma, -\rho)}{\Phi(-z_i \gamma)}
\]
where $\Phi$ and $\Phi_2$ are the univariate and bivariate standard normal cumulative distribution functions. Using the coefficient estimates and the variance-covariance matrix, I draw 1,000 simulations of beta coefficients and calculate the predicted probabilities using the typical values, in accordance with the idea of Tomz, Wittenberg, and King (2003)’s parametric bootstrapping. For the approach 1, I set the typical values of control variables (for both the alliance formation stage and the dispute initiation stage) at the mean (continuous variables) or the median (discrete variables) values of the observations that have at least one defense pact. Similarly, for the approach 2, I set the typical values of control variables at the mean or the median values of the observations that do not have any defense pact.

Figure 4 shows the effects of defense pacts on the probability of being a target of MIDs when I adopt the first approach. Starting with the baseline 1, assuming that all states have defense pacts, I take away the defensive alliance membership for some states and recalculate the predicted probability of being a target of MIDs for those states. As shown in Figure 4, taking away the defensive alliance membership results in significant (at $p < 0.05$ significance level) increase in the predicted probabilities of being a target of MIDs. The error bar represents the 2.5th and the 97.5th percentiles of the simulated predicted probabilities and the dot in the middle represent the mean. This result suggests that after taking the nonrandom formation of defense pacts into account, losing a defense pact is associated with higher probabilities of being a target of MIDs.

Figure 5 shows the effects of defense pacts on the probability of being a target of MIDs when I adopt the second approach. In the approach 2, I assume that no state has a defense pact. Conditioning on the baseline, I assign a defense pact to some of the states and examine the predicted probability of being a target of MIDs for the states. In accordance with the findings in Figure 4, assigning a defense pact lowers the predicted probabilities of being a target of MIDs. The difference between the two groups is statistically significant at $p < 0.05$ significance level. For both approaches, we can reach the same conclusion that having a defense pact is associated with lower chance of being a target of MIDs, conditioning on the
formation of defense pacts.

Table 4 shows another results regarding the deterrent effect. The alternative dependent variable indicates whether the state becomes a target of MID s in a given year, but I only utilize MID s that are applicable to existing defense pacts. Using the alternative variable, both coefficients for defense pacts obtained from the single probit regression and the endogenous treatment model are negative and statistically significant, indicating that we can detect deterrent effect more clearly. Still, the size of the coefficient obtained from the endogenous treatment model (-0.678) is greater than the coefficient obtained from the single probit regression (-0.108), suggesting that not taking the alliance formation process into account can underestimate the deterrent effect. Even though the results in Table 4 capture the effect of defense pacts on the initiation of MID s more explicitly by limiting the MID s to the ones that are applicable to the defense pacts, there could be a concern that this research design favors negative relationship than the research design in Table 3. Therefore, I present both results but focus more on the results in Table 3. The substantive interpretation of the effects of control variables on the formation of defense pacts and on the initiation of MID s and the correlation of the error terms is the almost the same.

3.5.2 Initiation Effect

Table 4 shows the empirical results regarding the initiation effect. Similar to Table 3, the first column shows the results of a probit regression of the binary indicator of the initiation of disputes and a probit regression of a defense pact. In addition, the second column shows the results of the endogenous treatment model. The coefficient estimate of the single-equation probit regression is positive but not distinguishable from zero (0.039), while the coefficient estimate of the endogenous treatment model is negative and distinguishable from zero at $p < 0.01$ level (-0.461). While the above hypotheses regarding the initiation effect expect positive or no relationship between being a member of defense pacts and the probability of initiating
disputes, both empirical results suggest that there is a negative relationship between the two. Even though both results suggest the same conclusion, the results obtained from the endogenous treatment model is more appropriate because it accounts for the nonrandom formation of defense pacts.

Interestingly, the correlation between the error terms in the two equations is positive (0.316) and statistically significant, indicating that the unobservable factors that facilitate signing a defense pact can also increase the chance of initiating militarized disputes. This positive correlation suggests that the signatories of defense pacts have a higher baseline probability of initiating MIDs toward any other state. Acknowledging that the signatories have higher baseline probabilities of initiating MIDs than the nonsignatories, the independent effect of defense pacts on the signatories’ probability of initiating MIDs is negative and statistically significant. In other words, there is a “restraining effect” of defense pacts on their members’ initiation of MIDs (Fang, Johnson, and Leeds 2014). If we ignore the difference in baseline probabilities of initiating MIDs between the signatories and the nonsignatories, we see no relationship – positive but not statistically significant – between having a defense pact and the probability of initiating MIDs.

Figure 6 describes the effects of defense pacts on the probability of initiating MIDs, conditioning on the formation of defense pacts (approach 1). Again, starting with the baseline that all states have defense pacts, I take the defensive alliance membership away for some states and calculate the predicted probabilities of initiating MIDs. Figure 6 shows that taking the defensive alliance membership away increases the predicted probability of initiating MIDs, compared to the predicted probabilities for those who keep having defense pacts. The differences in the predicted probabilities are positive and statistically significant at $p < 0.05$ level. Furthermore, Figure 7 shows the substantive effects of the initiation effect for the second approach. Assuming that no state has a defense pact, I assign the membership of a defense pact to some states and calculate the predicted probabilities of initiating MIDs. As can be seen from the graph, assigning the defensive alliance membership is associated with
lower predicted probabilities of initiating MIDs, compared to the predicted probabilities for those who do not have any defense pact. Consequently, for both approaches, I reach the same substantive conclusion that having a defense pact is also associated with lower chance of initiating MIDs, conditioning on the formation of defense pacts.

3.6 Conclusion

Investigating the effects of international institutions on states’ behavior has always been an important subject in IR. However, because international institutions are formed for certain purposes, sorting out the effect of international institutions from the latent propensity of forming the institutions has been a major challenge in the literature. Alliance studies are not free from this concern; signing alliances is a very costly commitment, thus states will deliberately make their decisions. However, despite rigorous developments in theoretical arguments and in the collection of alliance data sets, there have been few studies that approach this issue methodologically. Under the rational design of institutions framework, this concern has prevented us from having more confidence in empirical findings and evaluating conflicting empirical results.

This paper attempts to provide an empirical finding to the ongoing debate by examining the effects of defense pacts on the initiation of MIDs with a new methodological approach. By using a model that can deal with endogenous binary treatment, this paper finds supporting evidence for the deterrent effect of defense pacts but does not find supporting evidence for the initiation effect of defense pacts. Specifically, a state with defensive allies is less likely to be a target of MIDs than states without a defense pact, while the results with the endogenous treatment model suggests larger deterrent effect of defense pacts on preventing MIDs than those of simple probit regression. On the other hand, a single-equation probit regression finds no evidence of the initiation effect and the endogenous treatment model finds negative and significant relationship between the membership of defense pacts and the probability of
initiating MIDs. In other words, a state with defensive allies is less likely to initiate MIDs than states without a defense pact, when one addresses the issue of unobservable confounders.

The empirical finding of this paper conforms to those of several existing studies. For the deterrent effects, I find that allies of defense pacts enjoy greater deterrence than states without defense pacts (Leeds 2003; Johnson and Leeds 2011; Wright and Rider 2014; Leeds and Johnson 2017). Therefore, the results of this paper provides support to the argument that defense pacts can convey credible signals to potential challengers and are associated with lower chance of being a target of militarized disputes. It is also interesting that I find similar effects to those of directed-dyadic studies despite my data being monadic.

Furthermore, regarding the initiation effect, I find that allies of defense pacts are not more likely to initiate disputes compared to states without a defense pact, and indeed, are less likely to initiate disputes. Johnson and Leeds (2011) find similar results in directed dyadic settings. The decreasing chance of dispute initiation with defense pacts may look counterintuitive, but recent developments in the alliance literature regarding restraining effects of allies (Fang, Johnson, and Leeds 2014) could be one alternative explanation for the finding. Fang, Johnson, and Leeds (2014) find that for defensive allies, the more an ally values the alliance relationship, the less the ally is likely to escalate conflicts. Consequently, the empirical finding in this paper suggests that the concern about undesirable side effects of defense pacts (Palmer and Morgan 2006; Senese and Vasquez 2008; Johnson and Leeds 2011) needs to be investigated further.

Finally, the results of this paper raise questions for future research. First, one can further investigate the concerns about the undesirable side effects of defense pacts both theoretically and empirically. Second, a defense pact can be better at restraining alliance partners by credibly threatening to abrogate or terminate the alliance (Fang, Johnson, and Leeds 2014), but given that defense pacts are very costly signals, to what extent the threat to abrogate or terminate the defense pact can be credible is worth investigating. Third, developing a hierarchical endogenous treatment model that addresses the dependence among observations
can be a methodological contribution. In doing so, we can check whether the finding of this paper holds for different units of observations, possibly for directed-dyadic or triadic settings. Investigating these questions could contribute to the alliance literature by enhancing our understanding on the effects of alliances on state behavior.
Table 3: Probit Regression of Dispute Initiation: deterrent effect
(all MIDs; standard errors in parentheses; *p < 0.01, ’p < 0.05)

<table>
<thead>
<tr>
<th>Being a Target of Militarized Interstate Disputes</th>
<th>Separate Regressions</th>
<th>Probit with endogenous binary treatment</th>
</tr>
</thead>
<tbody>
<tr>
<td>Defense pact</td>
<td>-0.013</td>
<td>-0.573*</td>
</tr>
<tr>
<td></td>
<td>(0.038)</td>
<td>(0.107)</td>
</tr>
<tr>
<td>Capabilities</td>
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<td>0.777’</td>
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<tr>
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<td>(0.367)</td>
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<tr>
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<td>0.964*</td>
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<tr>
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<td>(0.121)</td>
</tr>
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<td>0.045*</td>
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<tr>
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<td>(0.006)</td>
</tr>
<tr>
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<td>-0.011</td>
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<td>(0.035)</td>
</tr>
<tr>
<td>Territorial claim</td>
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<td>0.074*</td>
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<tr>
<td></td>
<td>(0.009)</td>
<td>(0.009)</td>
</tr>
<tr>
<td>Claimed territory</td>
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<td>0.085*</td>
</tr>
<tr>
<td></td>
<td>(0.006)</td>
<td>(0.006)</td>
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<tr>
<td>Offense pact</td>
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<td>Nonaggression pact</td>
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<tr>
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<td>(0.041)</td>
<td>(0.043)</td>
</tr>
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</table>

<table>
<thead>
<tr>
<th>Having at Least One Ally Committed to Defending the State</th>
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<th></th>
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</thead>
<tbody>
<tr>
<td>Capabilities</td>
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<tr>
<td></td>
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<td>(0.441)</td>
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<tr>
<td>Threat level</td>
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<td>2.688*</td>
</tr>
<tr>
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<td>(2.685)</td>
<td>(2.688)</td>
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Table 4: Probit Regression of Dispute Initiation: deterrent effect (alternative measure)
(MIDs only applicable to defense pacts; standard errors in parentheses; *p < 0.01, ’p < 0.05)

<table>
<thead>
<tr>
<th></th>
<th>Separate Regressions</th>
<th>Probit with endogenous binary treatment</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Observations 11,793</td>
<td>Observations 11,793</td>
</tr>
<tr>
<td>Constant</td>
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<td>-1.734*</td>
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<td>(0.042)</td>
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<td>Observations</td>
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<td>11,793</td>
</tr>
<tr>
<td>Log Likelihood</td>
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<td>-9881.42</td>
</tr>
<tr>
<td>Observations</td>
<td>11,793</td>
<td>11,793</td>
</tr>
</tbody>
</table>

<p>|                                | (MIDs only applicable to defense pacts; standard errors in parentheses; <em>p &lt; 0.01, ’p &lt; 0.05) |
|                                |                                            |
| Contiguous states              | 0.067</em>                                     | 0.066*                                  |
|                                | (0.006)                                   | (0.006)                                 |
| Democracy                      | -0.198*                                    | -0.206*                                 |
|                                | (0.034)                                   | (0.034)                                 |
| Territorial claim              | -0.054*                                    | -0.055*                                 |
|                                | (0.009)                                   | (0.009)                                 |
| Claimed territory              | 0.043*                                     | 0.043*                                  |
|                                | (0.007)                                   | (0.007)                                 |
| Offense pact                   | 0.100*                                     | 0.100*                                  |
|                                | (0.008)                                   | (0.008)                                 |
| Neutrality pact                | -0.222*                                    | -0.221*                                 |
|                                | (0.018)                                   | (0.018)                                 |
| Nonaggression pact             | 0.079*                                     | 0.080*                                  |
|                                | (0.001)                                   | (0.001)                                 |
| Constant                       | -1.733*                                    | -1.734*                                 |
|                                | (0.042)                                   | (0.042)                                 |
| Observations                   | 11,793                                     | 11,793                                  |
| Log Likelihood                 | -9881.42                                   | -9881.42                                |</p>
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<td>(0.369)</td>
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<td>(0.006)</td>
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<td>(0.035)</td>
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<td><strong>Democracy</strong></td>
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<tr>
<td><strong>Territorial claim</strong></td>
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<tr>
<td><strong>Log Likelihood</strong></td>
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</tr>
</tbody>
</table>

Table 5: Probit Regression of Dispute Initiation: Initiation Effect

(all MIDs; standard errors in parentheses; *p < 0.01, ’p < 0.05)
<p>| | | |</p>
<table>
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<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
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<td>0.067*</td>
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<td>(0.006)</td>
</tr>
<tr>
<td></td>
<td>-0.092*</td>
<td>-0.130*</td>
</tr>
<tr>
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Figure 3.1: Predicted Probabilities of Being a Target of MIDs (deterrent effect, approach 1)

Potential Target's Defensive Alliance Membership (baseline 1: conditioning on receiving treatment i.e. if all states have defense pacts)
Figure 3.2: Predicted Probabilities of Being a Target of MIDs (deterrent effect, approach 2)

Potential Target's Defensive Alliance Membership (baseline 2: conditioning on not receiving treatment i.e. if all states do not have a defense pact)
Figure 3.3: Predicted Probabilities of Initiating MIDs (initiation effect, approach 1)

Potential Challenger’s Defensive Alliance Membership (baseline 1: conditioning on receiving treatment i.e. if all states have defense pacts)
Figure 3.4: Predicted Probabilities of Initiating MIDs (initiation effect, approach 2)
Chapter 4

Introducing a Bayesian model of categorical endogenous treatment and sample selection: reexamining the effects of consultation and defense pacts on interstate disputes

4.1 Introduction

How can we examine the effects of international institutions on states’ behavior? Examining whether (or, to what extent) international institutions have an impact on shaping states’ foreign policies is important because states have clear goals and purposes when they design the institutions (Koremenos et al. 2001), and they invest their resources to create and maintain the institutions. Therefore, it is important to check whether the institutions function as the creators intended, and in any case, have any unintended consequences. The effects of military alliances, formal international agreements established to influence a state’s military/security policies, have been subjects of study in the IR literature. To be more specific, scholars have studied how and whether (different types of) military alliances affect states’ decisions regarding interstate war occurrence – whether they have any impact on states’ decisions to initiate militarized interstate conflicts as well as the decisions to respond militarily given the initiated conflicts.

However, for empirical tests of international institutions on state behavior, there are two major methodological challenges that have often been neglected in relevant empirical studies: the non-random assignment of treatment (endogenous treatment) and the non-random sample selection problem. First, the endogenous treatment issue arises when we

*In general, the endogenous treatment issue is applicable to testing the effects of international institutions on any relevant foreign policy decisions, while the non-random selection issue is applicable to testing the
cannot claim that our main independent variable is independent of the dependent variable conditioning on other observable control variables. The issue is relevant to the literature of institutional effectiveness because the features of international institutions are tailored to meet certain goals by their signatories. In other words, when examining the effects of these institutional features, one should first pay attention to the conditions under which certain features are adopted, while others are not. Regarding military alliances, for example, it is not difficult to assume that states who are likely to be attacked by other states are more likely to seek defensive alliance partners and are willing to pay greater costs (e.g., policy concessions) for the defensive commitment. In such cases, failing to address the fact that states who sign defense pacts are at higher risks of being attacked would result in underestimating the deterrent effect of defense pacts, only to find naive positive correlations between having defense pacts and dispute initiation. Therefore, we should first acknowledge the different circumstances that states face when they enter each alliance obligation, and given the different baselines, we can examine the effect of the alliance obligation on the probability of being attacked and the probability of resisting.

Second, the non-random sample selection issue arises when a researcher suspects that her data are neither the entire population nor random samples from the population, thus empirical finding from the sample cannot be generalized to the population of interest. As mentioned earlier, the non-random sample selection does not exist when testing the effects of institutions on a state’s initial foreign policy decision (only the endogenous treatment issue exists), but the issue prevails for subsequent decisions. One example is a decision tree of foreign policies. Depending on the initial path of a decision tree, the options a state can choose vary and we cannot tell whether the state would have chosen the same option if it went to “The Road Not Taken.” Relating this to the alliance literature, an interstate dispute consists of at least two actions: one side initiating a conflict and the other side responding effects on subsequent decisions that follow an initial decision, when the initial decision limits the scope of the following decisions.
to the action militarily. If neither side initiates a conflict, the option “to respond militarily” does not exist in the other side’s policy choices; For these states, we cannot figure out how the states would have responded if they had been attacked. In other words, only the targets of militarized disputes are provided with an opportunity to resist. In this case, the data set for testing the effect of military alliances on the target’s response is limited to the set of cases where there is at least one militarized dispute. Because there are reasons for a state to become a target of militarized disputes, we cannot treat the sample of states who are attacked as the same as the sample of other states who are not attacked (thus the non-random sample selection). Therefore, we need to model a state’s likelihood of becoming a target of interstate disputes, and after accounting for the fact, we can examine whether the target’s military alliance portfolio affects its decision to respond militarily or not. As mentioned above, this issue is applicable to other issues in the IR literature. One example is an imposition of economic sanctions. When a state makes a threat of imposing sanctions against another state and later imposes a sanction, the sample to test the effect of the economic sanction is limited to the sample of states who were already threatened. Assuming that the threats are also policy decisions that are deliberately chosen by policymakers, ignoring the potential systematic differences between the entire population of potential sanction targets and the sample of threatened states can be problematic.

This paper contributes to the literature by introducing a novel statistical estimator that takes both issues into account. Although the existing theoretical arguments have suggested these issues, few empirical studies have addressed both issues in their analyses. As briefly discussed above, the two issues can be illustrated as the same problem: omitted variable bias. The issue becomes problematic when we are not good at controlling for the baseline likelihood of being attacked and/or the baseline likelihood of resisting when attacked (through regressions or matching methods) across different alliance choices. Again, these two issues are reasonable and important concerns in observational studies: we might not have good understanding about what the control variables are and what their relationships
are. Even worse, even if we do, we often have incomplete or noisy data. In such cases, naive estimates of the effects of alliance choices can be biased upward or downward, depending on the correlation structure among the outcome variables. Identifying control variables based on theory and having good measurements of the controls are ideal solutions, but when this is not feasible, modeling a correlation between the endogenous treatment and the outcome variable(s) to capture any remaining confounding effects is a methodological work-around. Existing endogenous treatment regression models and Heckman sample selection models are developed from this approach. However, each of the models can address one issue at a time, while there have been recent developments on addressing both issues simultaneously in one statistical model (Stata 15). In accordance with the approach, I introduce a new Bayesian statistical model that addresses both issues and extends the existing models by allowing the endogenous treatment to be categorical. I validate my model with simulation studies.

I leverage this methodological contribution and apply the model to examine how different alliance obligations affect states’ decisions to initiate and escalate interstate disputes differently. In particular, I compare the effects of defense pacts on interstate disputes to those of consultation pacts. Based on the signaling argument in IR, public defense pacts have been considered to deter potential challengers by 1) increasing the defensive allies’ probability of intervention and 2) increasing the effectiveness of joint fighting through peacetime military coordination. Although many empirical studies have found supporting evidence for the deterrent effect of a defense pact, there have been several studies that show different results or that raise concerns about whether a defense pact makes targeted allies more aggressive or not. Related to the concern, some scholars have suggested that a consultation pact can

\[I\text{ understand that the model does not assume simultaneity (i.e., alliance choices affecting dispute initiation and dispute initiation affecting alliance choices at the same time). To attenuate the concern, I only include the alliances that are formed before there is a conflict for a potential target in a given year. In other words, if there is a conflict in June for a state in a given year, an alliance the same target formed in September during the same year does not appear in the data set. Because the unit of analysis is a state-year and I might lose many observations if I use a lagged variable, I take this approach. As described later, the relevant information is coded at the triadic level (potential challenger-potential target-the ally of the potential target) and the data set is collapsed to the monadic level (a potential target-year).}\]
be alternatives to a defense pact. The logic is that the ally’s aggressiveness induced by alliance commitments should be proportional to the degree of the commitments: promising a consultation pact, which is a weaker commitment than the defensive commitment, would not encourage the ally’s aggressive foreign policy at the expense of its lower deterrent effect. However, the effects of a consultation pact on interstate disputes – compared to both non-alliance cases and to the cases with a defense pact – have not been examined empirically.

Using my model, which accounts for the strategic and endogenous choice of alliance types, I find supporting evidence for the hypothesis that defense pacts deter potential challengers compared to non-alliance cases. At the same time, I find no clear empirical evidence for the effects of other alliance obligations, including consultation obligations, on their signatories’ probability of being targets of interstate disputes or the signatories’ probability of responding militarily. Furthermore, by taking both the endogenous treatment and the sample selection issues into account, I find supporting evidence for the argument that states who are at risk of being targets of interstate disputes are more likely to seek alliance partners, especially defense pacts and offense/neutrality pacts. This paper not only provides another empirical finding with methodological fixes to the existing alliance literature, but also raises a question for future research about the reasons for states to sign consultation pacts, given that there is no clear relationship between having consultation pacts and interstate disputes.

4.2 Motivation for a Bayesian Model of Categorical Endogenous Treatment and Sample Selection

4.2.1 Methodological Challenges in Testing the Effects of International Institutions on States’ Behavior

As described in the introduction, examining the effects of international institutions has been considered as one of the most important questions in the IR literature, but at the same
time, one of the most difficult questions to test empirically. It is because states form, join, and leave international institutions under certain circumstances. In other words, states’ decisions to sign an international agreement cannot be treated as random because it is a costly decision: committing oneself to an agreement that involves certain obligations imposes restrictions on the state’s future foreign policy decisions, and the state may have to pay domestic and international reputational costs if it fails to comply with the rules of the institutions. Assuming that states have finite resources and want to maximize their utilities by optimizing their usage of resources, there is no reason for them to take the burden of joining and maintaining their memberships of international institutions if they cannot benefit from them. Therefore, states will form or join international institutions only if there are certain policy goals that they want to achieve through the institutions. States may want to achieve domestic policy goals (by locking-in their foreign policy), international policy goals (by influencing other states’ foreign policy decisions), or both – one international institution may be effective at achieving multiple policy goals. Still, there should be good reasons for the states to join (and to leave) international institutions. Under this rational design of international institutions framework (Koremenos et al. 2001), we can hardly assume that our treatment variable – states’ decisions to create and join certain international institutions – could be randomly assigned, as experimental studies usually assume.

Military alliances are no exceptions to this issue. Military alliances are formal international agreements signed among independent states for military cooperation (Leeds 2002). To examine how military alliances affect interstate disputes, I envision the following situation. First, there is a state who may or may not be concerned about being attacked by other states. Given the state’s baseline risk of being attacked, the potential target state chooses its military alliance type. Second, observing the alliance choice, the other states decide

\footnote{To be more specific, there is a third actor behind this process: an ally who signs the alliance with the potential target state. I do not explicitly model the negotiation process of alliance formation in this paper, assuming that alliances are only formed when all the parties (including the potential target) agree with the terms.}
whether to initiate an interstate dispute targeting the potential target state. Third, given
the alliance choice the targeted state has and given that a dispute is initiated by any of the
other states, the targeted state decides whether to respond militarily or not. Even though
this way of describing how interstate disputes start and escalate is quite common in the IR
literature, there have been few statistical estimators that are directly motivated from the
above model description and specifically designed to analyze the process.

In the above situation, the prospect of conflict initiation is likely to influence both the
potential target state’s decisions to seek alliance partners and the potential alliance partners’
decisions to precommit themselves to certain alliance obligations. On the one hand, potential
target states under higher levels of security threat are more likely to offer policy concessions
to attract potential defender states, preferring stronger partners (Morrow 1991; Johnson
2015) and/or reliable partners (Leeds 1999; Johnson et al. 2015). On the other hand, the
potential alliance partners, especially the strong states, are more likely to make alliance
commitments who are related to the partners’ self-interest (e.g., trade, shared values, etc.),
in addition to the policy concessions by forming alliances (Fordham 2010; Poast 2012). Given
that both the potential target and the potential alliance partner are in trade of security and
potential benefit, both sides will carefully calibrate the types of alliance commitment, just
enough to achieve their goals (Morrow 1994). Therefore, accounting for the fact that states
choose different alliance types under different circumstances is a key to understanding the
effects of these alliance types better: before comparing cases across alliance types, we first
have to examine what are the different circumstances that states are in, when they choose
such alliance types.

Furthermore, the prospect of conflict escalation is likely to influence the decisions of all
of the three actors in the scenario mentioned above: the targeted ally, the challenger, and
the target’s alliance partners. Depending on alliance choices and terms, the targeted ally’s
resistance may or may not invoke the alliance. Therefore, if the target’s alliance partner
is concerned about whether promising a certain alliance commitment makes the targeted
ally more aggressive or not, it will tune the specifics of the alliance text to prevent the situation or choose another alliance commitment (Snyder 1997; Kim 2011). On the other hand, the potential target can settle the specific language that might limit the scope of the alliance partner’s commitment or demand other alliance commitments during the negotiation. Finally, the potential target’s likelihood of resisting, along with its alliance choice, affects the potential challenger’s expected utility of making a demand (Morrow 1994). Taking that into account, potential challengers decide whether to make a demand, and if they do, the amount of demand they make.

In short, we can reasonably assume that the prospect of conflict initiation and conflict escalation decisions weigh powerfully in the initial alliance choices. At the same time, the ready-to-use statistical models, such as multivariate regressions or matching methods may not fully capture the underlying differences in circumstances under which states choose one alliance type over the other. Both models create comparable counterfactuals by removing the differences in other relevant characteristics. However, identifying and controlling for the relevant characteristics that represent the differences in circumstances are very difficult in observational studies, due to incomplete theoretical guide for choosing control variables or incomplete data.

In such cases, endogenous treatment models can be good methodological approaches to examine the effects of international institutions on states’ behavior. Acknowledging that our current understanding of the problem and available data sets can be limited, the endogenous treatment model relaxes the assumption that we can consider the alliance choices as exogenous (i.e, alliance choices and the states’ decisions to initiate and escalate conflicts are independent) given the other observable control variables. By allowing the alliance choices, dispute initiation, and dispute escalation to be correlated even after controlling for the other relevant factors, the endogenous treatment model captures any confounding effects caused by the unobservable confounders that are not included in the model. Therefore, endogenous treatment models are generally more robust than simple regression models, providing another
empirical test of the exogenous treatment assumption. If the correlation estimates between the decisions are essentially zero, then it provides supporting evidence for the argument that we can treat the assignment of alliance choices as random if we condition on the confounders. In this case, we can resort to simpler statistical models without the endogenous treatment assumption. On the other hand, if the correlation estimates are significantly different from zero, there might be issues with correctly identifying control variables, thus using endogenous treatment models can be a better choice. Also, the estimates can tell how the decisions are related to each other. For example, a positive correlation between forming a defense pact and the prospect of being attacked (conditioning on the other factors) suggests that the confounders missing in the model make forming a defense pact more likely as well as increase the probability of being attacked. Similarly, a negative correlation suggests that the unobservable confounders make one decision more likely, while making the other decision less likely. Although the estimates cannot tell the source of correlations as any other statistical models, they inform scholars about the existence of correlations and motivate future research for developing rigorous theoretical arguments for the missing covariates.

In addition to the endogenous treatment issue, there is another methodological issue with examining the effects of alliance choices on dispute escalation: the nonrandom sample selection issue. As described earlier, the potential challenger’s decision to initiate disputes is determined by many factors, both observables and unobservables, and its decision is also endogenous to the prospect of the target’s resistance. In this case, the dispute initiation works as an endogenous censoring mechanism (not as a treatment variable). In other words, even though we can theoretically depict the prospect of potential target’s resistance when attacked for any potential targets, what we actually observe is the target’s resistance only when conflicts occur. If there is no country attacking the potential target, there is no way for us to figure out whether the potential target would have responded militarily in counterfactuals. Again, because only the states who become the targets of militarized disputes can either resist or acquiesce and dispute initiation is also not a random decision, we have to account for
the fact that we are able to analyze only a subset of states. The proposed model is able
to address this issue along with the endogenous treatment issue by directly modeling the
dispute initiation process prior to analyzing dispute escalation. Therefore, it is useful to
examine how alliance choices affect a series of decisions regarding interstate conflicts, not
just the very first decision (dispute initiation), while considering how one decision shapes
the following decisions. The statistical model developed in this paper is motivated by the
following question: Acknowledging that states who are under different circumstances choose
different types of military alliances, how can we estimate the effects of the non-random
alliance choice on interstate dispute initiation and escalation?

In this paper, I develop a Bayesian statistical model that describes the three-stage process
as a special case of the simultaneous equations model (SEM) (Zellner and Theil 1962; Ando
and Zellner 2010). The SEM model estimates parameters from a set of (generalized) linear
equations, where at least one dependent variable in an equation becomes the independent
variable in one or more of the other equations. This approach is useful to investigate the
effect of alliance types on interstate disputes because choosing an alliance type over the
others is determined by other factors (e.g., national capabilities, regime type, geographical
characteristics, etc.) – so the alliance choice is the dependent variable in the first stage – and
once the alliance is formed, it affects both the potential challenger’s decision to initiate a
conflict and the targeted ally’s decision to escalate (now the alliance choice is an explanatory
variable of interstate disputes). This paper’s model not only better captures the three-stage
decision making processes better than the existing SEM models, but also contributes to the
literature by 1) allowing the endogenous treatment variable to be a discrete multiple choice,
where the preferences between the two choices can be affected by introducing other options,
2) addressing both the endogenous treatment issue and the sample selection in one model,
and 3) developing a Bayesian model to address these issues. A short depiction of the model
is shown below.
a potential target’s alliance choice = \( f(\text{the state’s characteristics}) \)

the probability of being attacked = \( f(\text{the state’s alliance choice} + \text{other controls}) \)

the probability of resisting = \( f(\text{the state’s alliance choice} + \text{other controls}) \)

### 4.2.2 Why Categorical Endogenous Treatment? Unordered but Correlated Alliance Choices

This paper examines how different alliance types affect interstate disputes differently, and the alliance types are discrete choices. Reading the obligations specified in the alliance agreement text, IR scholars have sorted various alliance obligations into several categories. For example, Leeds et al. (2002) identify five distinct alliance obligations in the ATOP (Alliance Treaty Obligations and Provisions) data set: defense, offense, neutrality, nonaggression, and consultation. Another data set about military alliances, the Correlates of War (COW) formal alliance data set (Gibler 2009) identifies four alliance obligations: defense, neutrality, nonaggression, and entente. Because these alliance obligations are to fulfill different policy goals and promising one obligation can affect including or removing other alliance obligations, the best operationalization for the alliance choices is to treat them as a unordered but potentially correlated discrete variable. Therefore, I build an endogenous treatment model that incorporates a multinomial probit model (a MNP model) to investigate alliance choices.

In this article, I argue that a multinomial probit model is better than a multinomial logit model (that makes the IIA assumption) in analyzing different alliance choices for two reasons:

1. First, theoretical arguments explaining alliance choices suggest that the IIA assumption is not always valid.

A multinomial logit model is a simpler alternative to the MNP model. The MNL model allows the dependent variable to be unordered and categorical, thus scholars have used this model often to explore the conditions under which an actor choose one option over the others (e.g., vote choice, types of dispute resolution, etc.). The MNL model is more suitable than the ordered logit model to explore the effect of each alliance type separately, and it does not require complex estimation methods. At the same time, one important concern about the MNL model is the independence of irrelevant alternatives (IIA) assumption: Introducing or removing a third option should not change one’s preference orderings for the existing two
umption can be violated. Second, a MNP model is inherently more flexible than a MNL model, so starting from a MNP model (and switch to a MNL model if necessary) could be a better practice. Third, even if the IIA assumption is met, a MNP model is still more robust than with addressing the omitted variable bias issue in the alliance choice stage. When one potential ally takes one alliance obligation off the table (or introducing another obligation) during negotiations, it can send a signal to the other states, thus changing their preference orderings over alliance choices. For example, to motivate the rest of this section, I propose a hypothetical situation in which we assume two potential alliance partners and three alliance obligations on the negotiation table: a defensive obligation, an offensive/neutrality obligation (without the defensive obligation), and a consultation obligation (neither of the aforementioned two obligations).

What does it mean if the protege takes the offense/neutrality option off the table, arguing that the defender does not have to commit itself to these alliance commitments? Both offense and neutrality pacts are about exerting their military power actively, even beyond the signatories’ borders. Therefore, by removing the option, the protege can send a signal that the protege is not interested in receiving military assistance from the defender for conflicts that occur outside of the protege’s borders. Snyder (1997) argues that one of the concerns that the defender has about promising a strong alliance commitment is that the protege can be aggressive even beyond the alliance terms, because promising a strong defensive commitment suggests that the defender has strong interests in keeping the protege

options (If an option A is preferred to another option B, then introducing or removing the third option C should not make B preferrable to A). In practice, the MNL model assumes that the odds of choosing one option over the other (the odds for every pairwise comparison among the alternatives) stay the same, regardless of the availability of all the other options. While several studies argue that this assumption is often met in empirical analyses (and thus a MNL model can be quite robust; Dow and Endersby 2004; Kropko 2008), there have been criticisms about the IIA assumption, especially when one of the options is introduced/removed in reality (i.e., candidates resign or enter during election campaigns) or one of the options is a close alternative to the other (Alvarez and Nagler 1994; 1998).

A standalone offensive obligation can be a better example for thought experiments, but knowing that (sole) offense pacts are quite rare in the real world, I also include the neutrality obligation. In general, both offense and neutrality pacts are about exert their military power actively even beyond the signatories' borders. This distinction also conforms to the operationalization of alliance choices that I use for empirical analyses.
state, thus will come to an aid even if the protege initiates an interstate conflict. If the defender was concerned about whether promising a defense pact would increase the protege’s probability of attacking others, the defender could prefer a consultation pact to a defense pact. However, if the protege can assure the defender state that it is not interested in attacking others by taking the offense/neutrality option out, the defender could be more likely to offer a defense pact instead of a consultation pact, compared to the initial situation. In this case, removing one option provides additional information to the defender, potentially affecting the defender’s preferences over a defense pact and a consultation pact.

In addition, focusing on the practical meaning of the IIA assumption in multinomial choice models – that the odds for pairwise comparison of options stay the same, unrelated to the availability of the other options – many scholars suggested that introducing a close substitute for the existing option can change the odds of choosing the existing option over the other option. A classic example is choosing between a car and a bus: introducing another bus with the exact same route could change the relative probability of taking the bus compared to taking the car (Alvarez and Nagler 1998). For testing the effects of alliance choices, a consultation obligation (without a defensive obligation) can be an alternative to a defensive obligation, especially for the states who do not want to commit themselves to fully defending the others. Therefore, introducing or removing a consultation obligation can change the odds of pairwise comparisons of alliance obligations, especially the ones that involve the defensive obligation.

Second, the restrictions of the MNL model allow it to be parsimonious, but also make it more susceptible to providing biased estimates. In particular, using a MNP model is more advisable when we have a good theoretical conjecture about the violation of the IIA assumption.

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1 Of course, removing the offense/neutrality option does not relieve the defender’s another concern about the protege’s responding more aggressively when attacked.

**I ran a multinomial logit model that examines the alliance choice (i.e., the first stage of my three-stage model) and ran the Hausman test of IIA assumption. For the four-category alliance choice variable, omitting the offense/neutrality pact rejects the null hypothesis that the IIA assumption is met at the conventional 0.05 significance level. The above argument could be one explanation for the finding.**
assumption, and/or when our substantive interest is in accordance with the availability of the options. As explained earlier, a MNP model is more flexible than a MNL model because it estimates correlations between the choices. One can prefer a MNL model to a MNP model if the researcher has a good theoretical argument suggesting that the discrete choices are not correlated. However, it is very difficult to develop such an argument in general. Also, using a MNL model can be justified when both models provide substantively the same results; if that is the case, a MNL model can be preferred to a MNP model because the former is much simpler and takes less computational cost. In general, researchers who use a MNL model are advised to conduct a post-estimation test of the IIA assumption. Sometimes scholars fail to reject the IIA assumption (Wright 2008), but it may not always be the case. Kropko (2008) compares a restricted MNP model to a conditional logit model and conclude that the latter is quite consistent and apparently more efficient, but because of the additional restrictions imposed in the former, it is difficult to argue that the latter works well in general.[II]

Third, even if we have a good theory that supports the IIA assumption, using a MNP can still be more robust than using a MNL model, because there is another source of correlations between choices: omitted variable bias. Put differently, whether alliance choices are not correlated after accounting for the relevant factors (that can be supported by theoretical arguments) and whether we can properly control the factors in empirical analyses are different questions. Even if the choices are not correlated in the true DGP, if we fail to account for the factors of alliance choices in our model specification while making the IIA assumption, we can have biased estimates. In other words, a MNP model is doubly robust in capturing correlations; the source of correlations can be the true DGP itself or the limitation of the data we have. Of course, the model does not tell where the correlation come from, but at

[II] As explained in his paper, the MNP model that he uses (asmprobit) restricts the variances of the first and the second choices and assumes that the correlations between the first choice and the rest of the choices are zero. Because his simulation is about three choices, the MNP model estimates only two additional parameters (the variance of the third choice and the correlation between the second and the third choices) compared to the CL model, instead of six parameters in the DGP. Therefore, in his simulation, the MNP model seems to be effective only when there is a considerable amount of correlation between the second and the third choices.
least is able to detect the correlations. This is not possible for a MNL model.

Given the concerns above about the potential violation of the IIA assumption and after running a couple of tests for the IIA assumption with my data set, I build an endogenous treatment model that takes unordered but (potentially) correlated unordered categorical choices as its endogenous treatment variable. In general, the MNP model is more robust than the multinomial model: if we do not find any correlations between the choices, we can go back to a MNL model. The drawback of a MNP model is that it is more difficult to estimate (e.g., is computationally heavier and can have an identification issue) than the MNL model. Despite the computational costs, I decide to adopt and extend the multinomial probit model because 1) it is more robust than the MNL model and 2) utilizing the feature of the MNP model that the error terms are normally distributed (instead of assuming that they are logistically distributed) fits well when I combine the endogenous treatment part and the sample selection part for my model.

There are mainly two approaches in the Bayesian MNP model: the exact likelihood approach (McCulloch, Polson, and Rossi 2000) and the parameter expansion approach (Imai and van Dyk 2005; Burgette and Nordheim 2012; Fong et al. 2016). Both approaches are similar in the sense that they set the variance of the error terms in the first utility function to one and calculate the variances of the error terms in the other utility functions in relative to the first variance. On the other hand, the exact likelihood approach calculates the off-diagonal covariances as the function of the variances and the error terms in the first utility function. The parameter expansion approach uses the data augmentation and calculates the marginal likelihood of the MNP model. Imai and van Dyk (2005) argue that their approach can be less sensitive to prior decisions. Especially in combining the MNP model and the sample selection model, the parameter expansion approach could be very helpful because the sample selection part can be easily modeled by the same parameter expansion approach. Burgette and Nordheim (2012) is in accordance with Imai and van Dyk (2005), but provides more efficient way of reparameterizing the variance-covariance matrix. Instead of fixing the
The trace restriction method provides more consistent results than those of Imai and van Dyk (2005). However, the model in this article is based on the Imai and van Dyk (2005)’s approach. In other words, I fix the variance of the first error terms to one. Adopting the trace restriction method and improving the model can be a future project. At the same time, I am not fully confident whether the improvement will change the results considerably, because the variances of the other two equations in my model (predicting dispute initiation and escalation) are going to be unidentified in either reparameterization method: they have to be fixed to one or will eventually be rescaled to one. Still, adopting the trace restriction approach can be beneficial for predicting alliance choices better.

4.2.3 Why Both? Combining Endogenous Treatment Model and Sample Selection Model

As described earlier, we need to take both endogeneity and sample selection issues into account. Furthermore, I described that both issues can be illustrated by the same fundamental problem, which is omitted variable bias. Although there have been developments in Bayesian sample selection models (van Hasselt 2011, Ding 2014, Zhang, Inder, and Zhang 2015) and endogenous treatment models (Chib, Greenberg, and Jeliazkov 2009; Burgette and Nordheim 2010) separately, there have been few models that address both processes in one model. Chib, Greenberg, and Jeliazkov (2009) develops a nonparametric Bayesian model that addresses both endogenous treatment and sample selection. Their statistical model can be better than this paper’s model in terms of being nonparametric, but in their model, the endogenous treatment only affects the censored outcome, not the outcome variable that governs the selection mechanism. In other words, if I were to apply their model to this paper, I should argue that alliance choices affect interstate dispute escalation, but not dispute initiation. Because this assumption does not make much sense based on the
existing IR literature, there is a need for developing a new model. Furthermore, they discuss the possible extensions of their model in terms of qualitative variables, but the discussion is mostly dedicated to binary or ordinal variables. Therefore, for the analysis of categorical alliance type choices and their effects on dispute initiation and escalation, this model can suit better.

Burgette and Nordheim (2010) develop a Bayesian multinomial probit switching model (Burgette 2010; R package endogMNP) that is close to von Stein (2005; Schultz and von Stein 2005)’s approach. However, to my knowledge, their model is a two-stage model so cannot estimate the effects of alliance choices on dispute escalation. Therefore, the model proposed in this paper is be better at illustrating the motivating case, especially for estimating the decision to escalate the conflict. Furthermore, another limitation of their model is that as the number of categorical treatments increase, the number of equations (and the number of parameters) to estimate increases rapidly. It is because the model investigates every possible situation. For example, an equation predicting dispute initiation given a defense pact is different from the one that predicts dispute initiation given a consultation pact. Therefore, estimating separate outcome equations for each value of the endogenous treatment variable may not be as efficient as my approach, because my approach estimates only one outcome equation. This could be particularly concerning given that MID initiations (and even more, MID escalations) are rare events. There might not be enough variation in MID initiations or MID escalations if we subset the observations by alliance type choices.

Finally, Stata 15 (StataCorp 2017) recently released new set of treatment-effects estimators, which is in accordance with my model in terms of addressing potential correlations between the treatment assignment stage and the outcome stage. However, the available functions in Stata are for binary, ordinal, interval, and linear treatment effects. Therefore, the model proposed in this paper is better than the Stata commands when the choice among multiple categorical treatments could be correlated and there is a sample selection process. Furthermore, my Bayesian approach can be another contribution that is absent in Stata
packages, with the benefits of using a Bayesian model described above.

4.2.4 Why Bayesian? Benefits of Using Bayesian Approach Compared to Frequentist Approach

One might wonder why I adopt a Bayesian approach to develop a statistical model, instead of a frequentist approach that is more prevalent in current stage of quantitative analyses in social sciences. In this paper, I argue that building and using a Bayesian model of endogenous choices and sample selection is better than a model based on the frequentist approach. It is not only because of the general advantages of using Bayesian approach (i.e., Bayesian inference is more intuitive than frequentist inference; a frequentist model is a special form of a Bayesian model with diffuse prior; etc.), but also because of the Bayesian models’ ability to handle high-dimensional problems better. Specifically, Gibbs sampling breaks down the high-dimensional estimation issue (that becomes even more difficult with missing data) into a set of low-dimensional estimations, and specifying the priors of the parameters as well as the distributions for the missing variables (Ibrahim et al. 2005, 333) help estimation.

Gibbs sampler, one algorithm of Markov Chain Monte Carlo (MCMC) estimation, characterizes joint probability densities as their component conditional densities (Jackman 2009, forthcoming). The main advantage of the Copula approach is that it allows the flexibility in the specification of the error terms, while most of the conventional endogenous switching models and/or the sample selection models assume that the error terms are normally distributed. However, extending the Copula approach to more than two equations is still nascent and is much more complicated than the two-equations setting. Therefore, I decide to proceed with the Bayesian multiple-equations approach with the assumption that the errors are normally distributed, but investigating the extension of the Copula approach in the future would be worthwhile.

Furthermore, there have been recent developments in Copula approach to illustrate the endogenous treatment issue and/or the sample selection issue (Chiba, Martin, and Stevenson 2015; Braumoeller et al. forthcoming). The main advantage of the Copula approach is that it allows the flexibility in the specification of the error terms, while most of the conventional endogenous switching models and/or the sample selection models assume that the error terms are normally distributed. However, extending the Copula approach to more than two equations is still nascent and is much more complicated than the two-equations setting. Therefore, I decide to proceed with the Bayesian multiple-equations approach with the assumption that the errors are normally distributed, but investigating the extension of the Copula approach in the future would be worthwhile.
For example, instead of drawing a joint posterior density of all of the estimates that we are interested in, one can divide the estimates into several subsets and draw each subset of parameters from the conditional density of the parameters, given all the other estimates and data. For example, this paper is interested in getting estimates of coefficients as well as correlations between equations. In frequentist approach (e.g., maximum likelihood), if there are \( J \) number of categorical endogenous choices, the likelihood function is derived from a joint \( J + 1 \)-dimensional multivariate distribution, resulting in estimating both sets of parameters simultaneously. Then, maximizing the (log) likelihood function is both analytically and numerically very complicated. In particular, the conventional factorization method may not work well with missing data, while expectation-maximization (EM) algorithm may do a better job. On the other hand, Gibbs sampling for this paper allows us to partition the whole set and to draw the coefficient estimates and the correlation estimates for each subset, from an one-dimensional conditional distribution of the multivariate distribution. This reduces the complexity of the estimation process significantly.

Furthermore, because all of the outcome variables of the system of equations in this paper are binary and some of them are missing, using a latent variables approach is reasonable, which is incorporated well with Gibbs sampling. To be more specific, a marginalized data-augmented Gibbs sampler makes estimation easier because directly modeling a likelihood function for a discrete outcome variable is difficult.

Finally, while it is not required for Bayesian estimation, imposing a well-reasoned informative prior can help estimation process by yielding more precise estimates. In addition to the general benefit of imposing informative priors – making inferences from the results is more intuitive and substantively meaningful –, its MCMC sampling does not have to explore every possible parameter space (which is high-dimensional), as guided by the priors. Of course, the precision of the estimates comes at a cost of the robustness of the estimates: imposing a

\[ p(\beta | y, X) \]

**Instead of calculating the posterior density of the estimates given the observed data** \( p(\beta | y, X) \), **I calculate the joint density of the estimates and latent variables given the data, and draw posterior estimates from the density, conditioning on the latent variable and covariates** \( p(\beta | y^*, X) \).
diffuse prior allows the MCMC sampling to explore much wider parameter space and its estimates can be more robust. Depending on the information available to a researcher and her priorities, the choice of a prior in a Bayesian approach is left to the researcher’s discretion, while there is no such choice in a frequentist approach. Similarly, a Bayesian parametric estimation is straightforward with the assumptions about the distributions of missing variables or latent variables. In summary, adopting a Bayesian approach provides substantial benefits to answer this paper’s research question, along with its general robustness compared to a frequentist approach.

4.3 Model Description

4.3.1 Illustration of the Multiple Equation Model

As briefly described above, the model consists of three sets of equations. The first set of equations is to estimate the potential target’s alliance type choices, whose dependent variable is a categorical (not ordinal) variable. The second set, consists of one equation, estimates the potential challenger’s decision to initiate a conflict. The third set, consists of one equation, estimates the target’s decision to resist on initiated conflict. For the second and the third sets of equations, scholars could argue that the (potential) target’s alliance type choices affect the potential challenger’s decision to initiate a conflict and the target’s decision to resist. Furthermore, the target is provided with an opportunity to resist only after the challenger initiates a conflict. Therefore, in analyzing the effects of alliance type choices on these foreign policy decisions, one needs to address both the endogenous treatment issue and the sample selection issue.

Illustrating this situation as a statistical model, if the number of alliance type choices is $J$, then the total number of equations estimated in this model is $(J - 1) + 2$, assigning the $J$th category as the baseline category. In the second and the third sets of equations, the dependent variable in the first equation becomes an independent variable as $(J - 1)$ binary
indicator variables (the endogenous treatment issue). The dependent variable in the third set of equation is only observable when the dependent variable in the second equation is one (the sample selection issue).

For the first set of equations, I adopt a multinomial probit model (the MNP model), which is to address alliance type choices. The MNP model is used to explain the choice among multiple alternatives, when the alternatives are categorized but not ordinal as discussed earlier. The MNP model could be illustrated as a set of \((J - 1)\) equations, where each equation calculates the latent utility of each alternative. For this model, I set the baseline category as the \(J\)th category without loss of generality. All the other \((J - 1)\) number of latent utilities are relative to the utility of the \(J\)th alternative. Given the latent utilities of \(J\) alternatives, \(t_{ij}\) where \(j = 1, 2, \ldots, J\), (the latent utility of the \(J\)th alternative \((t_{iJ})\) is always set to zero), the target state chooses the alliance type that yields the highest utility. Because \(t_{iJ}\) is always zero, the \(J\)th alliance type is chosen when all the other latent utilities \(t_{i1}, t_{i2}, \ldots, t_{i(J-1)}\) are smaller than zero. For the other cases, the \(j\)th alliance type is chosen when \(t_{ij}\) yields the highest utility and is greater than zero. \(T_{ij}\) is a binary variable, which is coded one if the \(j\)th category is chosen, zero otherwise.

For the choice to initiate a conflict and the choice to resist or back down given the conflict, I adopt the two-stage Heckman sample selection model (1979) with binary outcomes. The major difference between this model and the conventional Heckman selection model is that the alliance type choice, which is the dependent variable in the MNP model, is now a treatment variable and the assignment of the treatment is correlated with both the dependent variables in the selection stage and in the outcome stage. Therefore, simply running an existing Heckman selection model with the two equations (the selection stage and the outcome stage) cannot address the nonrandom assignment of treatment and can produce biased estimates of the treatment effects. For the selection stage, \(u_i\) is the latent variable of dispute initiation and \(U_i\) is the binary outcome variable coded zero if \(u_i \leq 0\) and one otherwise. This is a latent variable approach: I assume that there is a general tendency
of dispute initiation between states, which is the latent variable \( u_i \), and we observe dispute initiation \( U_i = 1 \) when a state initiates a dispute. For the outcome stage, \( y_i \) is the latent variable of dispute escalation and \( Y_i \) is the binary outcome variable coded missing if a state becomes the target of a militarized dispute \((u_i \leq 0)\), zero if the state is attacked but does not respond militarily (the conflict is not escalated) \((u_i > 0 \text{ and } y_i \leq 0)\), and one if the state is attacked and responds militarily (the conflict is escalated) \((u_i > 0 \text{ and } y_i > 0)\).

Each dependent variable is a function of its set of independent variables \((Z_i, W_i, \text{ and } X_i)\), the coefficients \((\zeta_j, \gamma, \text{ and } \beta)\), and the correlated error terms \((\varepsilon_{i1}, \varepsilon_{i2}, \ldots, \varepsilon_{iJ}, \varepsilon_{i,J+1})\). The number of exogenous independent variables in the selection stage is \( k \) and the number of exogenous independent variables in the outcome stage is \( l \).

Assuming that the error terms in the equations are distributed normally, the model is given by

\[
\begin{align*}
t_{i1} &= Z_i \zeta_1 + \varepsilon_{i1} \\
t_{i2} &= Z_i \zeta_2 + \varepsilon_{i2} \\
& \vdots \\
t_{i(J-1)} &= Z_i \zeta_{J-1} + \varepsilon_{i(J-1)} (t_{iJ} = 0) \\
u_i &= T_{i1} \gamma_1 + T_{i2} \gamma_2 + \cdots + T_{i(J-1)} \gamma_{J-1} + W_i \gamma_{J:(J-1+k)} + \varepsilon_{iJ} \\
y_i &= T_{i1} \beta_1 + T_{i2} \beta_2 + \cdots + T_{i(J-1)} \beta_{J-1} + X_i \beta_{J:(J-1+l)} + \varepsilon_{i(J+1)}
\end{align*}
\]

\((E_1, E_2, \ldots, E_{J-1}, E_J, E_{J+1}) \sim MVN(0_{J+1}, \Sigma), \Sigma \neq \sigma^2 I_{(J+1) \times (J+1)}\) for any \(\sigma\)

For better identification, having different sets of independent variables in each set of equations is ideal. At the same time, whether having the same set of independent variables cause serious identification issue or not is in question. The concern can be mitigated for discrete outcome variables, which is the case for this paper.
\[
\begin{align*}
T_{ij} &= 1 \quad \text{if} \quad t_{ij} = \arg \max (t_{i1}, \ldots, t_{iJ}) = t_{ij}; \text{ otherwise 0} \\
U_i &= 1 \quad \text{if} \quad u_i > 0; \text{ otherwise 0} \\
Y_i &= \text{NA} \quad \text{if} \quad u_i \leq 0 \\
Y_i &= 0 \quad \text{if} \quad u_i > 0 \text{ and } y_i \leq 0 \\
Y_i &= 1 \quad \text{if} \quad u_i > 0 \text{ and } y_i > 0
\end{align*}
\]

where the subscript \( i \) indicates each observation, \( i = 1, 2, \ldots, N \). The notation \( \Sigma \neq \sigma^2 I_{(J+1) \times (J+1)} \) for any \( \sigma \) indicates that at least one of the off-diagonal elements of the variance-covariance matrix is not zero, suggesting that there is at least one pair of the error terms in the equations are correlated. The model can be used and will yield unbiased estimates when all of the off-diagonal elements of the matrix is zero, but the results would be equivalent to the results obtained from running separate regression models.

### 4.3.2 Bayesian Computation of the Multiple Equation Model

In this model, I propose a sampling algorithm of the model parameters using Markov Chain Monte Carlo (MCMC) and derive posterior densities of the parameters. Because of the complexity of the above data generating process, I transform the multiple equations and reparameterize the variance-covariance matrix.

For the \((J + 1)\) number of multiple equations model, I partition the equations into two parts: the \((J - 1)\) number of equations which estimate the alliance type choices and the other two equations which explain states’ conflict behavior. The model could be rewritten as follows, as the reduced form:
\( T = Z \zeta + V \)

\[
\begin{align*}
    u_i &= T \gamma_{1:(J-1)} + W_i \gamma_{J:(J-1+k)} + \varepsilon_{iJ} \\
y_i &= T \beta_{1:(J-1)} + X_i \beta_{J:(J-1+t)} + \varepsilon_{i(J+1)}
\end{align*}
\]

whose variance-covariance matrix is

\[
\Sigma = \begin{pmatrix}
    \sigma^2_{t_1} & \rho_{t_1 t_2} \sigma_{t_1} \sigma_{t_2} & \cdots & \rho_{t_1 t_{J-1}} \sigma_{t_1} \sigma_{t_{J-1}} & \rho_{t_1 u} \sigma_{t_1} \sigma_u & \rho_{t_1 y} \sigma_{t_1} \sigma_y \\
    \rho_{t_1 t_2} \sigma_{t_1} \sigma_{t_2} & \sigma^2_{t_2} & \cdots & \rho_{t_2 t_{J-1}} \sigma_{t_2} \sigma_{t_{J-1}} & \rho_{t_2 u} \sigma_{t_2} \sigma_u & \rho_{t_2 y} \sigma_{t_2} \sigma_y \\
    \vdots & \vdots & \ddots & \vdots & \vdots & \vdots \\
    \rho_{t_{J-1} t_{J-1}} \sigma_{t_{J-1}} \sigma_{t_{J-1}} & \rho_{t_{J-1} t_{J-2}} \sigma_{t_{J-1}} \sigma_{t_{J-2}} & \cdots & \sigma^2_{t_{J-1}} & \rho_{t_{J-1} u} \sigma_{t_{J-1}} \sigma_u & \rho_{t_{J-1} y} \sigma_{t_{J-1}} \sigma_y \\
    \rho_{t_1 u} \sigma_{t_1} \sigma_u & \rho_{t_2 u} \sigma_{t_2} \sigma_u & \cdots & \rho_{t_{J-1} u} \sigma_{t_{J-1}} \sigma_u & \sigma^2_u & \rho_{u y} \sigma_u \sigma_y \\
    \rho_{t_1 y} \sigma_{t_1} \sigma_y & \rho_{t_2 y} \sigma_{t_2} \sigma_y & \cdots & \rho_{t_{J-1} y} \sigma_{t_{J-1}} \sigma_y & \rho_{u y} \sigma_u \sigma_y & \sigma^2_y
\end{pmatrix}
\]

\[
\begin{pmatrix}
    \Sigma_{11} & \omega'_{1u} & \omega'_{1y} \\
    \omega_{1u} & \sigma^2_u & \rho_{u y} \sigma_u \sigma_y \\
    \omega_{1y} & \rho_{u y} \sigma_u \sigma_y & \sigma^2_y
\end{pmatrix} = \begin{pmatrix}
    \Sigma_{11} & \omega' \\
    \omega & \Sigma_{22}
\end{pmatrix}
\]

where \( \sigma^2_{t_i} \) is the variance of the error terms in the utility function of the \( i^{th} \) alternative, \( t_i \), \( \sigma^2_u \) is the variance of the error terms in the selection equation, and \( \sigma^2_y \) is the variance of the error terms in the outcome equation. Furthermore, \( \rho_{ij} \) is the correlation between the error terms in the equation \( i \) and the equation \( j \). Because \( U_i \) and \( Y_i \) are binary outcomes drawn from their latent variables, \( u_i \) and \( y_i \), it is impossible to estimate \( \sigma^2_u \) and \( \sigma^2_y \). Therefore, we can set them to one and estimate the coefficients given the fixed variance. On the other hand, estimating the variance of the error terms in the \( (J - 1) \) utility functions are important. It is because the multinomial choice is decided by the \( (J - 1) \) equations altogether, and the choice is dependent on the variances of the error terms in the utility functions.
Based on the discussion above, the unconstrained variance-covariance matrix ($\tilde{\Sigma}$) can be reparameterized to the constrained variance-covariance matrix ($\Sigma$), by fixing the first diagonal element and the last two diagonal elements to one. The unconstrained $\tilde{\Sigma}$ can be reconstructed by multiplying the diagonal matrix $V$, whose diagonal elements are $\sigma_{t_1}$ for the MNP part and $\sigma_u$ as well as $\sigma_y$ for the sample selection part. $\Sigma_{11}$ indicates the sub-matrix of the matrix $\Sigma$, which is about the variance and covariance terms of endogenous choices (i.e., the variance-covariance matrix of the first set of equations).

\[
\tilde{\Sigma} = V \Sigma V \\
= \begin{pmatrix}
\sigma_{t_1} & 0 & 0 & 0 & 0 \\
0 & \sigma_{t_1} & 0 & 0 & 0 \\
0 & 0 & \cdots & 0 & 0 \\
0 & 0 & 0 & \sigma_u & 0 \\
0 & 0 & 0 & 0 & \sigma_y \\
\end{pmatrix} \times \begin{pmatrix}
1 & \rho_{t_1t_2} \sigma_{t_2} & \cdots & \rho_{t_1u} & \rho_{t_1y} \\
\rho_{t_1t_2} \sigma_{t_2} & \left(\frac{\sigma_{t_2}}{\sigma_{t_1}}\right)^2 & \cdots & \rho_{t_2u} \left(\frac{\sigma_{t_2}}{\sigma_{t_1}}\right) & \rho_{t_2y} \left(\frac{\sigma_{t_2}}{\sigma_{t_1}}\right) \\
\vdots & \vdots & \ddots & \vdots & \vdots \\
\rho_{t_1u} & \rho_{t_2u} \left(\frac{\sigma_{t_2}}{\sigma_{t_1}}\right) & \cdots & 1 & \rho_{uy} \\
\rho_{t_1y} & \rho_{t_2y} \left(\frac{\sigma_{t_2}}{\sigma_{t_1}}\right) & \cdots & \rho_{uy} & 1 \\
\end{pmatrix} \\
= V \begin{pmatrix}
\Sigma_{11} & \omega' \\
\omega & \sigma_y^2 \\
\end{pmatrix} V
\]

where $\omega = \begin{pmatrix} \rho_{t_1y} & \rho_{t_2y} \left(\frac{\sigma_{t_2}}{\sigma_{t_1}}\right) & \cdots & \rho_{uy} \end{pmatrix}$, the $(J + 1)$th row of the constrained variance-covariance matrix $\Sigma$, except the last element ($\sigma_y^2$).
In the imputation step, I draw $t_1, t_2, \ldots, t_{J-1}, u,$ and $y$ separately from a truncated univariate normal distribution, conditioning on all the other latent variables and the parameters in the constrained variance-covariance matrix ($\Sigma$). First, when $Y_i$ is not missing, I draw the latent variables of the MNP part from the following distribution:

The conditional means and the variances of the latent variables of the MNP part are as follows ("TN" indicates a truncated normal distribution):

$$t_{ij}|t_{i(-j)}, T_{ij}, u_i, y_i, \zeta, \gamma, \beta, \Sigma \sim \text{TN}(\mu_{t_{ij}}, \tau_{t_{ij}}^2, \{\max(t_{i(-j)}), 0\}, \infty) \quad \text{if } T_{ij} = 1$$

$$\sim \text{TN}(\mu_{t_{ij}}, \tau_{t_{ij}}^2, \{\min(t_{i(-j)}), 0\}) \quad \text{if } T_{ij} \neq 1$$

$$\mu_{t_{ij}} = Z_i \zeta_j + F' \begin{pmatrix} t_{i(-j)} - Z_i \zeta_{-j} \\ u_i - T \gamma_{1:(J-1)} - W_i \gamma_{j:(J-1+k)} \\ y_i - T \beta_{1:(J-1)} + X_i \beta_{j:(J-1+l)} \end{pmatrix}, \quad \text{where } F = -\phi^{jj} \varphi_{j(-j)}$$

$$\tau_{t_{ij}} = 1/\phi^{jj}$$

where $\phi^{jj}$ describes the $(j, j)$th component of $\Sigma^{-1}$ and $\varphi_{j(-j)}$ describes all the elements in the $j$th row of $\Sigma^{-1}$ except the $(j, j)$th element. The conditional means and variances of the $(J - 1)$ latent variables of the MNP part are computed by using the R function \texttt{condMom}, which is in part of the R package \texttt{bayesm} (Rossi, Allenby and McCulloch 2005; Rossi 2015).

The conditional means and the variances of the latent variables of the sample selection part are:

$$u_i|t_{ij}, T_{ij}, y_i, \zeta, \gamma, \beta, \Sigma \sim \text{TN}(\mu_u, \tau_u^2, \{0, \infty\})$$
\[\mu_u = T\gamma_{1:(J-1)} + W_i\gamma_{J:(J-1+k)} + F' \begin{pmatrix} t_{ij} - Z_i\zeta_j \\ y_i - T\beta_{1:J-1} + X_i\beta_{J:(J-1+l)} \end{pmatrix}, \quad \text{where } F = -\phi^{JJ} \varphi_{J(-J)}\]

\[\tau_u = 1/\phi^{JJ}\]

\[y_i|t_{ij}, T_{ij}, u_i, \zeta, \gamma, \beta, \Sigma \sim TN(\mu_y, \tau_y^2, \{0, \infty\}) \quad \text{if } Y_i = 1\]
\[\sim TN(\mu_y, \tau_y^2, \{-\infty, 0\}) \quad \text{if } Y_i = 0\]

\[\mu_y = T\beta_{1:(J-1)} + X_i\beta_{J:(J-1+l)} + F' \begin{pmatrix} t_{ij} - Z_i\zeta_j \\ u_i - T\gamma_{1:(J-1)} - W_i\gamma_{J:(J-1+k)} \end{pmatrix}, \quad \text{where } F = -\phi^{(J+1)(J+1)} \varphi_{(J+1)(-(J+1))}\]

\[\tau_y = 1/\phi^{(J+1)(J+1)}\]

where \(\phi^{JJ}\) describes the \((J, J)\)th component of \(\Sigma^{-1}\) and \(\varphi_{J(-J)}\) describes all the elements in the \(J\)th row of \(\Sigma^{-1}\) except the \((J, J)\)th element. Note that \(\Sigma\) is a \((J+1) \times (J+1)\) matrix. In other words, every step in this process is to calculate the mean and the variance of each latent variable, conditioning on all the other latent variables.

Second, if \(Y_i\) is missing, I draw the latent variables of the MNP part from the following distribution:

\[t_{ij}|t_{i(-j)}, T_{ij}, u_i, y_i, \zeta, \gamma, \beta, \Sigma_{11} \sim TN(\mu_{t_{ij}}, \tau_{t_{ij}}^2, \{\max(t_{i(-j)}, 0), \infty\}) \quad \text{if } T_{ij} = 1\]
\[\sim TN(\mu_{t_{ij}}, \tau_{t_{ij}}^2, \{-\infty, \max(t_{i(-j)}, 0)\}) \quad \text{if } T_{ij} \neq 1\]
\[ \mu_{ij} = Z_i \zeta_j + F'' \begin{pmatrix} t_{i(-j)} - Z_i \zeta_{-j} \\ u_i - T_j y_i \gamma_{1:(J-1)} - W_i \gamma_{1:(J-1+k)} \\ y_i - T_j x_i \beta_{1:(J-1)} + X_i \beta_{1:(J-1+l)} \end{pmatrix}, \quad \text{where } F'' = t(-\phi_{11}^{jj} \varphi_{11j(-j)}) \]

\[ \tau_{ij} = 1/\phi_{11}^{jj} \]

where \( \phi_{11}^{jj} \) describes the \((j, j)\)th component of \( \Sigma_{11}^{-1} \) and \( \varphi_{11j(-j)} \) describes all the elements in the \( j \)th row of \( \Sigma_{11}^{-1} \) except the \((j, j)\)th element.

The conditional means and the variances of the latent variables for the sample selection part are:

\[ u_i | t_{ij}, T_{ij}, y_i, \zeta, \gamma, \beta, \Sigma_{11} \sim TN(\mu_u, \tau_u^2, \{-\infty, 0\}) \]

\[ \mu_u = T_j y_i \gamma_{1:(J-1)} + W_i \gamma_{1:(J-1+k)} + F'' \begin{pmatrix} t_{ij} - Z_i \zeta_j \\ y_i - T_j x_i \beta_{1:(J-1)} + X_i \beta_{1:(J-1+l)} \end{pmatrix}, \quad \text{where } F'' = t(-\phi_{11}^{jj} \varphi_{11j(-j)}) \]

\[ \tau_u = 1/\phi_{11}^{jj} \]

\[ y_i | t_{ij}, T_{ij}, u_i, \zeta, \gamma, \beta, \Sigma \sim N(\mu_y, \tau_y^2) \]

\[ \mu_y = T_j y_i \beta_{1:(J-1)} + X_i \beta_{1:(J-1+l)} + F' \begin{pmatrix} t_{ij} - Z_i \zeta_j \\ u_i - T_j y_i \gamma_{1:(J-1)} - W_i \gamma_{1:(J-1+k)} \end{pmatrix}, \quad \text{where } F = -\phi^{(J+1)(J+1)} \varphi_{(J+1)(-(J+1))} \]

\[ \tau_y = 1/\phi^{(J+1)(J+1)} \]
Based on the parameter expansion method used in Imai and van Dyk (2005) and Ding (2014), the conditional distributions of $\sigma_{t_1}$, $\sigma_u$, and $\sigma_y$ given $\Sigma$ are:

$$
\sigma_{t_1}^2 | \Sigma \sim \sigma_0^2 \text{trace}(S_{(1:J-1)\times(1:J-1)}\Sigma_{(1:J-1)\times(1:J-1)}^{-1})/\chi^2_v(J+1)
$$

$$
\sigma_u^2 | \Sigma \sim \sigma_0^2 (S_{J\times J} \Sigma_{J\times J}^{-1})/\chi^2_v(J+1)
$$

$$
\sigma_y^2 | \Sigma \sim \sigma_0^2 (S_{(J+1)\times (J+1)} \Sigma_{(J+1)\times (J+1)}^{-1})/\chi^2_v(J+1)
$$

where $S$ is the prior scale of $\Sigma$ and $\sigma_0^2$ is a positive constant that satisfies $\tilde{S} = \sigma_0^2 S$. $\Sigma_{(1:J-1)\times(1:J-1)}^{-1}$ is the square submatrix of $\Sigma^{-1}$, from the first row/column to the $(J-1)$th row/column. $\nu$ is the prior of the degrees of freedom for $\Sigma$ and $(J+1)$ is the dimension of $\Sigma$.

After sampling $\sigma_{t_1}$, $\sigma_u$, and $\sigma_y$, I multiply the sampled variances to the residuals of the $(J+1)$ equations:
\[ \text{res.} t_{i1} = t_{i1} - Z_i \zeta_1 \]
\[ \text{res.} t_{i2} = t_{i2} - Z_i \zeta_2 \]
\[ \vdots \]
\[ \text{res.} t_{i(J-1)} = t_{i(J-1)} - Z_i \zeta_{(J-1)} \]
\[ \text{res.} u_i = u_i - T\gamma_{1:(J-1)} + W_i \gamma_{J:(J-1+k)} \]
\[ \text{res.} y_i = y_i - T\beta_{1:(J-1)} + X_i \beta_{J:(J-1+l)} \]

\[
\tilde{W} = \begin{pmatrix}
\text{res.} t_1 \\
\text{res.} t_2 \\
\vdots \\
\text{res.} t_{(J-1)} \\
\text{res.} u \\
\text{res.} y
\end{pmatrix}
= \begin{pmatrix}
\sigma_{t_1} & 0 & 0 & 0 & 0 & 0 \\
0 & \sigma_{t_1} & 0 & 0 & 0 & 0 \\
0 & 0 & \ddots & 0 & 0 & 0 \\
0 & 0 & 0 & \sigma_{t_1} & 0 & 0 \\
0 & 0 & 0 & 0 & \sigma_u & 0 \\
0 & 0 & 0 & 0 & 0 & \sigma_y
\end{pmatrix}
\begin{pmatrix}
\text{res.} t_1 \\
\text{res.} t_2 \\
\vdots \\
\text{res.} t_{(J-1)} \\
\text{res.} u \\
\text{res.} y
\end{pmatrix}
\]

and draw the unconstrained variance-covariance matrix \( \tilde{\Sigma} \) from an inverse-Wishart distribution.

\[
\tilde{\Sigma} \propto IW \left( v + N, \tilde{S} + \sum_{i=1}^{N} \tilde{W}_i \tilde{W}_i^T \right)
\]

Furthermore, I set the values of the variances \( \sigma_{t_1}, \sigma_u, \) and \( \sigma_y \) as the squared root of the first and the last two diagonal elements of \( \tilde{\Sigma} \).
\[ \sigma_{t_1} = \sqrt{\tilde{\Sigma}_{1 \times 1}} \]

\[ \sigma_u = \sqrt{\tilde{\Sigma}_{J \times J}} \]

\[ \sigma_y = \sqrt{\tilde{\Sigma}_{(J+1) \times (J+1)}} \]

Using the new values of \( \sigma_{t_1}, \sigma_u, \) and \( \sigma_y \), I reparameterize \( \Sigma \) from \( \tilde{\Sigma} \).

\[ \Sigma = V^{-1}\tilde{\Sigma}V^{-1} \]

\[ = \begin{pmatrix}
\frac{1}{\sigma_{t_1}} & 0 & 0 & 0 & 0 \\
0 & \frac{1}{\sigma_{t_1}} & 0 & 0 & 0 \\
0 & 0 & \ddots & 0 & 0 \\
0 & 0 & 0 & \frac{1}{\sigma_u} & 0 \\
0 & 0 & 0 & 0 & \frac{1}{\sigma_y}
\end{pmatrix} \ast \tilde{\Sigma} \ast
\begin{pmatrix}
\frac{1}{\sigma_{t_1}} & 0 & 0 & 0 & 0 \\
0 & \frac{1}{\sigma_{t_1}} & 0 & 0 & 0 \\
0 & 0 & \ddots & 0 & 0 \\
0 & 0 & 0 & \frac{1}{\sigma_u} & 0 \\
0 & 0 & 0 & 0 & \frac{1}{\sigma_y}
\end{pmatrix} \]

Finally, following the advice of Imai and van Dyk (2005), I transform the latent variables with the new values of \( \sigma_{t_1}, \sigma_u, \) and \( \sigma_y \). This is to make sure that the posterior distribution is stationary.
First, I draw the coefficients $\Lambda = (\beta, \gamma, \zeta)$ given the latent variables and the variance-covariance matrix. Instead of the conventional way of drawing betas ($\mu_{\beta} = \Sigma_{\beta}(X'\Sigma^{-1}Y + \Sigma_{0}^{-1}\mu_{0})$ and $\Sigma_{\beta} = (X'\Sigma^{-1}X + \Sigma_{0}^{-1})^{-1}$), I transform all the $J + 1$ number of latent variables by premultiplying the Cholesky decomposition of $\Sigma^{-1}$ (the upper triangular matrix). The method of transformation offered by McCulloch and Rossi (1994) makes the error terms in the entire system of equations be distributed as $N(0^{J+1}, I^{J+1})$. For $\Sigma^{-1} = CC'$,
\[
C' \begin{pmatrix} t_{i1} \\ t_{i2} \\ \vdots \\ t_{i(J-1)} \\ u_i \\ y_i \end{pmatrix} = C' \begin{pmatrix} Z_i & 0 & 0 & 0 & 0 & 0 \\ 0 & Z_i & 0 & 0 & 0 & 0 \\ 0 & 0 & \ddots & 0 & 0 & 0 \\ 0 & 0 & 0 & Z_i & 0 & 0 \\ 0 & 0 & 0 & 0 & (T, W) & 0 \\ 0 & 0 & 0 & 0 & 0 & (T, X) \end{pmatrix} \begin{pmatrix} \zeta_1 \\ \zeta_2 \\ \vdots \\ \zeta_{J-1} \\ \gamma \\ \beta \end{pmatrix} + C' \begin{pmatrix} v_{i1} \\ v_{i2} \\ \vdots \\ v_{i(J-1)} \\ e_{iu} \\ e_{iy} \end{pmatrix}
\]

\[P* = Q* + R*, \text{ where } R \sim N(0^{J+1}, I^{J+1}).\]

Then, the conditional posterior distribution of the beta coefficients is:
\[
\Lambda | P*, \Sigma \sim N(\mu_\Lambda, \Sigma_\Lambda), \text{ where } \mu_\Lambda = \Sigma_\Lambda (Q*' P + A\bar{\Lambda}) \text{ and } \Sigma_\Lambda = (Q*' Q* + A)^{-1}. \text{ The prior of } \Lambda \text{ is given by } N(\bar{\Lambda}, A^{-1}).
\]

I repeat the MCMC process to obtain additional draws from the posterior.

4.4 Simulation

4.4.1 Description of the Simulation

I conduct a Monte Carlo simulation to assess the performance of the model. In short, the simulation shows that the proposed model provides unbiased estimates for the coefficients in the system of equations, both for the effects of the endogenous treatment variable and the effects of exogenous treatment variables. Furthermore, the estimates of the effects of
exogenous treatment variables have lower values of variance than the existing models, which could be another reason to encourage using this model.

First, I assume a situation where there are three endogenous categorical choices ($J = 3$). Therefore, in this simulation, there are four equations to estimate ($= J - 1 + 2$). I draw the main quantities of interest, the beta coefficients, from a uniform distribution ranging from -3 to 3. Second, I draw the variance-covariance matrix of the four sets of error terms from a four-dimensional multivariate normal distribution. The mean vector of the distribution consists of four zeros, and the variance of each set of error terms is drawn from a uniform distribution ranging from 1 to three, and the pairwise correlations are drawn from a uniform distribution ranging from -0.6 to 0.6. These “true” values in the data generating process changes for each simulation. Third, for generating each data set, each independent variable is drawn from independent standard normal distribution. Furthermore, the “observed” variance-covariance matrix of the error terms as well as the error terms are drawn repeatedly. Fourth, using the aforementioned variables, I create the dependent variables for the four equations. The sample size of each data set is $N = 3,000$ and I repeat the simulation 2,500 times.

After drawing the error terms, I simulate the data set with the following data generating process:

The intercepts for the equations are set to zeros, simply for convenience. In addition, I changed the distribution from a standard normal distribution to cover wider range of beta coefficients.

When I compare the estimates to the true values, I standardize the true values so that I can compare the results directly. I divide the true values of coefficients by the standard deviation of the error terms. It is because all the outcome variables ($T_{ij}$, $U_i$, and $Y_i$) are binary variables, a model could not correctly uncover the variance. All we can estimate is the “standardized” coefficient estimates, setting the variance of the error terms to one. For the multinomial probit part, I set the variance of the error terms in the first equation to one and calculate the variance of the error terms in the second equation relative to the first one. For example, if $\sigma_1^2 = 2$, I compare the estimates of the first equation to $\frac{\zeta_{21}}{\sqrt{2}}$, $\frac{\zeta_{22}}{\sqrt{2}}$, $\frac{\zeta_{23}}{\sqrt{2}}$, and $\frac{\zeta_{24}}{\sqrt{2}}$. For $\zeta_{21}$, $\zeta_{22}$, $\zeta_{23}$, and $\zeta_{24}$, whose variance is defined relative to $\sigma_1^2$, I compare the estimates of the second equation to $\frac{\zeta_{21}}{\sigma_1}$, $\frac{\zeta_{22}}{\sigma_1}$, $\frac{\zeta_{23}}{\sigma_1}$, and $\frac{\zeta_{24}}{\sigma_1}$. For the third and the fourth equations, the coefficients are divided by the standard deviation of the error terms in each equation ($\frac{1}{\sigma_3}$ and $\frac{1}{\sigma_4}$).


\[ t_{i1} = \zeta_{10} + \zeta_{11}Z_{i1} + \zeta_{12}Z_{i2} + \zeta_{13}Z_{i3} + \zeta_{14}Z_{i4} + \varepsilon_{i1} \]
\[ t_{i2} = \zeta_{20} + \zeta_{21}Z_{i1} + \zeta_{22}Z_{i2} + \zeta_{23}Z_{i3} + \zeta_{24}Z_{i4} + \varepsilon_{i2} \]
\[ t_{i3} = 0 \]
\[ u_i = \gamma_0 + \gamma_1T_{i1} + \gamma_2T_{i2} + \gamma_3W_{i1} + \gamma_4W_{i2} + \gamma_5W_{i3} + \gamma_6W_{i4} + \gamma_7W_{i5} + \varepsilon_{i3} \]
\[ y_i = \beta_0 + \beta_1T_{i1} + \beta_2T_{i2} + \beta_3X_{i1} + \beta_4X_{i2} + \beta_5X_{i3} + \varepsilon_{i4} \]

\[ (E_1, E_2, E_3, E_4) \sim MVN \left( \begin{pmatrix} 0 \\ 0 \\ 0 \\ 0 \end{pmatrix}, \begin{pmatrix} \sigma_{t1}^2 & \rho_{t1t2}\sigma_{t1}\sigma_{t2} & \rho_{t1u}\sigma_{t1}\sigma_{u} & \rho_{t1y}\sigma_{t1}\sigma_{y} \\ \rho_{t1t2}\sigma_{t1}\sigma_{t2} & \sigma_{t2}^2 & \rho_{t2u}\sigma_{t2}\sigma_{u} & \rho_{t2y}\sigma_{t2}\sigma_{y} \\ \rho_{t1u}\sigma_{t1}\sigma_{u} & \rho_{t2u}\sigma_{t2}\sigma_{u} & \sigma_{u}^2 & \rho_{uy}\sigma_{u}\sigma_{y} \\ \rho_{t1y}\sigma_{t1}\sigma_{y} & \rho_{t2y}\sigma_{t2}\sigma_{y} & \rho_{uy}\sigma_{u}\sigma_{y} & \sigma_{y}^2 \end{pmatrix} \right) \]

\[ \begin{cases} 
T_{ij} = 1 & \text{if } t_{ij} = \text{argmax}(t_{i1}, \ldots, t_{iJ}) = t_{ij}; \text{ otherwise 0} \\
U_i = 1 & \text{if } u_i > 0; \text{ otherwise 0} \\
Y_i = NA & \text{if } u_i \leq 0 \\
Y_i = 0 & \text{if } u_i > 0 \text{ and } y_i \leq 0 \\
Y_i = 1 & \text{if } u_i > 0 \text{ and } y_i > 0 
\end{cases} \]

I run five statistical models and compare their coefficient estimates across models. First, I run my proposed model with 8,000 iterations and 2,000 burn-in period. I use a multivariate normal distribution for the priors of coefficients \((\Lambda = (\beta, \gamma, \zeta))\), where \(MVN(0^{24}, 100 * I^{24})\). The prior distribution of the error terms is another multivariate normal distribution, \(MVN(0^4, I^4)\). This also suggests that I set the prior scale \(S = \tilde{S} = I_4\) and \(\sigma_0^2 = 1\) for the inverse-Wishart distribution for \(\tilde{\Sigma}\). I set \(\nu = (J + 1) + 1 = J + 2 = 5\) so that the priors for the correlations are marginally distributed for \([-1, 1]\) (Barnard, McCulloch, and Meng 2000). While the proposed model can simultaneously estimate all of the coefficients, the other four models can estimate different subsets of the coefficients. Second, to obtain the coefficient estimates for the first two equations, explaining endogenous choices, I utilize the

In practice, I use the precision matrix of 0.01 * \(I^{24}\) in the simulation.

Imposing a diffuse prior for the distribution of error terms may not be very helpful, because three variance terms out of four will be rescaled to one in the estimation process. Again, this is because these are latent variables and we cannot figure out the true variance of the latent variables; we only observe discrete outcomes based on the latent variables. Furthermore, the prior distribution suggests that neither the endogenous issue nor the nonrandom sample selection issue exists. In other words, the estimation starts with a conservative perspective, in case where the scholars do not firmly believe that the two issues prevail.
R package MNP (Imai and van Dyk 2005) using their original reparameterization approach, not using the trace restriction method (Burgette and Nordheim 2012). Because I adopt the former approach, I believe it makes more sense to use the former for direct comparison. Third, I run the \texttt{glm()} function in \texttt{R} with a probit link to get the coefficient estimates in the third equation, explaining the outcome of the selection equation. Fourth, I run another \texttt{glm()} to get the estimates in the fourth equation. Note that these two separate probit models do not address endogeneity. Fifth, I also run a Bayesian sample selection model with binary outcomes to obtain another set of estimates for the third and fourth equations. This model addresses nonrandom sample selection, but does not address endogeneity. I utilize Ding (2014)’s Bayesian selection-probit model (approximated to normal from Student-t distribution by setting the degrees of freedom $\nu = 10^{10}$). My expectation is that the Bayesian multinomial probit (MNP) model shows similar performance to that of the proposed model (because there is no endogeneity/selection issue in the first stage); However, the estimates of the proposed model, especially for the endogenous treatment effects, should be less biased than those of the two probit models and the Bayesian sample selection model. It is because the latter three models do not address the endogeneity issue.

### 4.4.2 Simulation Results

I adopt four measures to compare the simulation results. First, I calculate the average absolute difference between the true value and the corresponding estimate. I can also run the MNP switching model (Burgette and Nordheim 2010) to get the coefficient estimates for the first three stages. I expect their model and my model to perform similarly regarding bias. At the same time, because the model’s structure is different from the proposed model (i.e., estimate different number of equations) and the reparameterization methods is different, I am not sure whether direct comparison of the two models is possible. Furthermore, the MNP switching model cannot address nonrandom sample selection. Therefore, the model does not yield estimates for the fourth equation.

In my earlier version, I fixed the values in the correlation matrix and calculated the average difference. However, because the values in the correlation matrix are not fixed in this manuscript, I calculate the absolute difference between the true value and the corresponding estimate. As explained in my earlier version, the sign of bias (either downward or upward) depends on the sign of the coefficients as well as the sign of the correlation. Therefore, calculating the average of these differences will cancel out the ones in different signs,
Second, I calculate the root mean square error (RMSE: \( \sqrt{E\left((\theta - \hat{\theta})^2\right)} \)), which shows how far the estimate is from the true value in an absolute sense. Third, I calculate the percentage of the 95% confidence intervals (or credible intervals: hereafter CIs) of each estimate containing the true value. Because I use a diffuse prior for the coefficient estimates, both credible intervals and confidence intervals should provide similar results and be comparable. Fourth, I calculate the widths of the CIs to check the efficiency of the models. While this is not the main criterion of model choice, my model is motivated from the concern about getting biased coefficient estimates. At the same time, if two models yield similar results regarding their biasedness, I can choose a more efficient model.

Table 6 shows the average absolute differences between estimates obtained from each model and the corresponding true values. The first three columns show the results of separate single-equation models for each of the three stages. The first column is the results of a MNP model (Imai and van Dyk 2005), simply analyzing the categorical outcome variable. The second column shows the results of a single-equation probit model, estimating the effect of both endogenous and exogenous treatment variables on the dependent variable of the selection stage \( U_i \). In addition, the third column shows the results of another single-equation probit model, estimating the effect of both endogenous and exogenous treatments on the dependent variable of the outcome stage, \( Y_i \), while not addressing the missingness of \( Y_i \). Furthermore, the fourth column shows the results of a Bayesian sample selection model (Ding 2014), which addresses the sample selection issue but not the endogenous treatment issue. Finally, the fifth column shows the results of the model proposed in this paper, which addresses both the endogenous treatment issue and the sample selection issue. The asterisks indicate the effect of the endogenous treatment choices, which I am particularly interested in.

resulting in zero. The RMSE below served as another measure to validate the model in the earlier version separately from the average difference. However, given that I have changed the first measure, both the first and the second measures capture almost the same characteristics.

According to the DGP, the treatment variable is endogenous, but the probit regression does not account for the endogeneity issue.
For Table 6, smaller number indicates better performance. First, as expected, there is not much difference between the estimates of a Bayesian MNP model and those of the proposed model regarding bias, though the proposed model’s average bias is slightly smaller than those of the Bayesian MNP model in most cases. However, for the endogenous treatment variables in the second and the third stage ($\gamma_0, \gamma_1, \gamma_2, \beta_0, \beta_1$, and $\beta_2$), the proposed model yields much smaller bias compared to the other models. For example, if we run a single probit model or the Bayesian two-stage selection model for the second stage, the estimates of $\gamma_0$ are biased either upward or downward by approximately 0.15-0.25, while the bias of the proposed model is only 0.08-0.12. In general, the amount of bias we expect from other models is twice larger than that we would expect from the proposed model.

For the exogenous treatment variables, all of the models’ estimates are unbiased. Still, the proposed model often performs better than the others by a small margin, especially for the second and the third stages. I believe it is because of the smaller variance in the estimates from the proposed model. Because the proposed model not only performs better for yielding smaller biases for endogenous variables but also performs better for most of the exogenous variables, I argue that the proposed model performs better than the others in terms of yielding more accurate estimates.

Table 7 shows the RMSEs of estimates obtained from each model. In summary, Table 7 shows similar results to the results shown in Table 6. The numbers in bold texts indicate the smallest RMSE for each coefficient estimate. The smaller the RMSE is, the closer the estimate is to the true value. As can be seen from Table 7, the proposed model produces the estimates that give the smallest RMSE for all of the coefficients. In particular, regarding estimating the endogenous treatment effects (the coefficients with asterisks), the proposed model is better at producing less biased estimates overall. For example, the RMSE for $\gamma_1$, another endogenous treatment effect, is much bigger in the two-stage model (0.304) than in the proposed model (0.156). According to the simulation results, the RMSEs for endogenous treatment effects range from 0.111 to 0.201 in the proposed model, while those of the two-stage model range from 0.205 to 0.331. For predicting exogenous treatment
effects (the coefficients without asterisks), the proposed model is slightly better than the other models. Therefore, there is no particular drawback of using the proposed model in terms of bias. From Table 6 and Table 7, I conclude that the proposed model considerably reduces biases for endogenous treatment effects and also slightly reduces biases for exogenous treatment effects.

Because I let the values of both the coefficients and the covariance matrix vary across simulations, positive and negative biases cancel out. At the same time, we can specify the conditions under which biases will be upward (the estimated values are greater than the true values) or downward (the estimated values are smaller than the true values). Figure 9 illustrates a simple example of a case where the bias is upward. Revisiting the main motivation of this dissertation, we can think of an example for such cases and check the simulation results. For example, I have argued in the previous chapter that the level of subjective threat assessment can affect both a decision to sign defense pacts and the probability of being attacked positively, leaving a positive correlation between the two choices even after controlling for the observables ($\rho_{t1u} > 0$). In such cases, even when a defense pact affects the probability of being affected negatively ($\gamma_1 < 0$), a naive model can conflate these two different signs of associations when estimating $\gamma_1$.

To visually illustrate the example, I select the simulated cases where they meet the two conditions mentioned above ($\rho_{t1u} > 0$ and $\gamma_1 < 0$) and plot the differences between the estimated values and the true values ($\hat{\beta} - \beta$). In such cases, the bias will be “upward,” meaning that naive models will yield larger estimates for the endogenous treatment effect. Figure 9 shows the distribution of such differences across models. The lines of each boxplot indicate 5th and 95th quantiles of the differences. Both ends of each box indicate 25th and 75th quantiles, and the vertical line in the middle of the box indicates the median value. As expected, the proposed model yields the most unbiased estimates compared to the other two. As expected, both the estimates of the two-stage sample selection model and those of the probit model are greater than the true values in general. Going back to my earlier
example of defense pacts, the negative effect of having defense pacts on the probability of
being attacked may not be very apparent if one uses either a two-stage model or the probit
model. In fact, it is more likely to find a positive association between having a defense pact
and the probability of being attacked, while the proposed model is capable of reducing bias
and detecting the negative effect of having a defense pact. Similarly, if an unobservable
confounder increases the chance of signing a defense pact but decreases the chance of being
attacked ($\rho_{t,u} < 0$), the estimates of both the two-stage sample selection equation and the
probit model are “downward-biased” ($\hat{\beta} - \beta < 0$).

Table 8 shows the percentage of the 95% CIs containing the true value across estimates.
Again, the numbers in bold indicate the highest percentage of containing the true value for
each estimate. Theoretically, the percentages should be approximately 0.95 for all of the cells.
However, it is not always the case, especially for the endogenous treatment effects. As can
be seen from Table 8, the proposed model outperforms the two single probit models and the
Bayesian sample selection model regarding estimating the effects of endogenous treatment
variables. For example, the CIs of the proposed model cover the true value of $\beta_0$ by 93.4%,
while those of the probit model cover the true value by 61.6% and those of the Bayesian
sample selection model cover the true value by 70.1%. The coverage rates of the proposed
model are quite consistent, though none of them in the second and third stages is greater
than 0.95. At the same time, approximately half of the 95% CIs in the first stage have more
than 95% coverage rate of true values. Furthermore, the proposed model is as good as the
other models in terms of estimating the effects of exogenous independent variables. The
Bayesian MNP model is better at covering the true values for their effects on the first choice,
but it cannot provide estimates for the latter two stages, including the endogenous treatment
effects. Therefore, I believe that the proposed model is better at recovering the entire data
generating process. This also suggests that there is no particular tradeoff or costs associated
with using the proposed model.\[\]

To be more specific, the variance of the estimates regarding the first choice is restricted to one, while the
Table 9 shows the average widths of the CIs across estimates. In general, the narrower the CI width is, the more precise the estimate is (therefore, the better the model is). If the CI coverage rate is substantively the same, then one can argue that a narrower CI is preferred to a wider CI because it provides more precise solutions. However, one should be cautious about applying the precision argument here because the narrower CIs do not correctly contain the true value as can seen in Table 8. There is always a tradeoff between the precision (=variance) and the accuracy (=bias) regarding model choice, but considering that the main motivation of this paper and the proposed model is to address the concerns about biased estimates, variance reduction is not the priority in this paper. Rather, the narrower CIs could be interpreted as a model misspecification.

Regarding endogenous treatment effects, either the proposed model or the two-stage sample selection model provides the narrowest CIs. Interestingly, the single probit models do not yield the narrowest confidence intervals nor do better at containing the true value as seen in Table 8. On the other hand, for estimating the effects of exogenous independent variables, all of the models have at least one of the most efficient estimates. In particular, the single problem model in the second column and the two-stage sample selection model are better at having narrower CIs than the proposed model in the last two stages. This is interesting especially for the two-stage sample selection model, because I originally expected that the proposed model will be better than the others given that it is in line with the seemingly unrelated regressions (SUR) approach. However, the CI widths of the proposed model is not too far from the others (including those of the probit models), and shows more stable widths in general. In other words, there is greater variability in the other models regarding the CI widths (probit models ranging from 0.22 to 0.57; the two-stage model
ranging from 0.23 to 0.57). In sum, the proposed model sometimes provides wider CIs than the others but this is to correctly address the endogeneity issue.

In summary, in testing endogenous treatment effects, the proposed model outperforms the existing models, because its estimates are closer to the true value and its CIs contain the true values by the correct percentages. Furthermore, even in testing the effects of control variables, the proposed model almost always outperforms the existing models by providing more precise estimates (smaller distance between the estimates and the true values). In other words, regarding the RMSEs and the CI coverage rates, the proposed model is as unbiased as the other existing models. At the same time, it may not be necessarily “more efficient” than the others regarding providing narrower CIs. In conclusion, the simulation results suggest that when scholars are concerned about endogeneity and sample selection issues, utilizing the proposed model is a better choice – it yields more accurate estimates for endogenous treatment variables and does not inhibit our ability to examine the effects of exogenous treatment variables. Even more, its estimates are often more efficient than those of the existing models. This model is applicable to many scholarly works that aim to test the effects of any non-random treatment variables, when researchers could not perfectly specify the set of control variables and/or there is a sample selection problem. Given the simulation results, I apply this proposed model to estimate the effects of alliance type choices on interstate dispute initiation and dispute escalation.

4.5 Application: how do defense pacts and consultation pacts affect interstate disputes?

4.5.1 The Effects of Defense Pacts on MID Initiation and Escalation

By making a commitment to communicate, cooperate, and coordinate with the allies,
the members of an military alliance can reveal their foreign policy preferences to others. However, the commitment to cooperate militarily has to be costly in order to be credible (Morrow 1994; Smith 1995, 1998; Fearon 1997; Yuen 2009). If the commitment does not impose any additional burden to the members, states who are not sincerely interested in cooperating with others could also precommit to military cooperation. States may still bluff, meaning that they can precommit to higher levels of military cooperation than their optimal level of cooperation and fail in fulfilling the obligation when the alliance is invoked, but this could be a rare case under special circumstances (Morrow 1994). In practice, we can easily observe that signing a formal military agreement incurs various types of costs. First, there are ex ante costs of forming and maintaining alliances, such as costs for alliance negotiation, costs for establishing formal organizations, and costs for peacetime military coordination. Second, there are also ex post costs of alliances, such as costs of assisting the allies if the alliances are invoked or reputational costs for abrogating alliance obligations. Finally, forming alliances also involves several future risks: the risks for external shocks, the risks of losing the domestic support, and the risk of their allies’ alliance abrogation or termination.

States can promise different alliance obligations, and among the obligations, defense pacts are about committing oneself to provide active military assistance when its ally’s sovereignty or territorial integrity is attacked by other states (Leeds et al. 2002, 9). In accordance with the signaling argument above, defense pacts incur ex post alliance costs to credibly reveal that the signatories are interested in protecting their allies’ territory and sovereignty. The ex post reputational costs that the signatories would pay if they fail to fulfill the commitment are also quite clear, because both international and domestic audiences can easily detect the failure to intervene when the alliance is invoked. Therefore, because of the reputational costs imposed on the signatories and the enhanced joint fighting ability through peacetime military coordination among allies, the option to intervene becomes more attractive to the signatories of the defense pacts, compared to the non-signatories. In conclusion, with the higher chance of third-party interventions (and thus decreasing chance of winning in a mul-
tilateral militarized disputes), the potential challengers are less likely to attack the members of defense pacts than those who do not have any defense pact.

_deterrent effect of Defense Pacts:_ A state is less likely to become a target of militarized interstate disputes if there is at least one ally who precommits to the state’s defense, compared to states who do not have any relevant defensive ally.

Many empirical studies that examine the deterrent effect of defense pacts on interstate disputes have found that having defense pacts is associated with lower chance of becoming a target of militarized interstate disputes (Leeds 2003, Johnson and Leeds 2011, Wright and Rider 2014, Johnson, Leeds, and Wu 2015, Leeds and Johnson 2017). At the same time, there are other empirical works that find the opposite relationship between the two, mainly motivated by the steps to war argument (Senese and Vasquez 2008, Kenwick, Vasquez, and Powers 2015; Leeds and Johnson 2017; Kenwick and Vasquez 2017). At the same time, there have been few studies that address the endogeneity of defense pacts. Wu (2017) adopts a binary endogenous model to examine the effects of defense pacts on MID initiation, and finds that after accounting for the fact that states who are at higher risks of becoming the targets of MIDs are more likely to form defense pacts, having at least one defensive ally significantly reduces the potential target’s chance of becoming the target of MIDs. In other words, it suggests that one source of the mixed findings about the deterrent effect of defense pacts can be the exogenous treatment assumption in the previous empirical studies. This paper serves as an additional empirical test that addresses the endogeneity issue of defense pacts, while extending the previous work by allowing to test the deterrent effect of multiple alliance choices simultaneously and to compare those of defense pacts to those of consultation pacts.

In addition to the deterrent effect of defense pacts, scholars have suspected whether defense pacts will also impact on the targeted ally’s decisions to escalate the disputes. If we simply focus on the dispute escalation stage, knowing that the defender (the signatory of a defense pact)’s commitment makes it more likely to intervene when conflicts escalate, the targeted ally is more likely to resist than other targeted states without the defense pact,
ceteris paribus. As explained earlier, the potential targets with defense pacts are more likely to resist because they know that their allies will pay ex post reputational costs if the allies fail to intervene and the allies expect higher chances of winning in multilateral wars.

At the same time, if we account for the endogeneity issue of the prior decisions, the relationship between defense pacts and the probability of escalation becomes more complicated. Recent formal models on dynamics within alliance partners and conflict mediation (Yuen 2009, Fang, Johnson, and Leeds 2014) argue that promising a defense pact may not necessarily increase the target’s likelihood of resistance. Yuen (2009)’s argument suggests a selection effect of dispute initiation, meaning that if the potential challenger observes the defender’s public and costly commitment to defend the potential target, it will adjust the amount of demand and will make a smaller demand. Therefore, even though the target is likely to resist more if it has defensive allies, what we observe in the real world is smaller demands made by potential challengers. Consequently, the targeted ally would not necessarily resist more given that the defense pact has reduced the amount of demands in earlier stages.

On the other hand, Fang, Johnson, and Leeds (2014) focus on the defender’s role as a(n) adviser/mediator in cases of conflict. The defender can make policy recommendations to the targeted ally, and depending on how the targeted ally values the alliance relationship with the defender, the targeted ally will follow the defender’s recommendation. The defender can sometimes advise the targeted ally to resist or not to resist, so we cannot conclude that having defensive allies will always increase the target’s likelihood of resistance. In sum, there is no consensus in theoretical arguments regarding the effects of defense pacts on dispute escalation. Identifying the conditions under which defensive allies recommend resisting than conceding could help us to investigate the relationship between having a defense pact and dispute escalation further, but I do not do so in this article. Because the relationship can be different depending on the conditions and on the samples I can get (the two different recommendations may cancel out each other, one can be more dominant than the other, etc.), I do not have expectation about how having a defense pact is related to dispute
escalation. Therefore, instead of providing a clear hypothesis about the escalation effect of defense pacts, I explore how having a defense pact is associated with dispute escalation in my data set in the following section using the three-stage estimator.

*Escalation Effect of Defense Pacts: When a state becomes a target of militarized disputes, the effect of having at least one defensive ally on the targeted state’s overall likelihood to resist can be different depending on other conditions. Therefore, I do not provide an expectation for the escalation effect of defense pacts.*

Existing empirical studies that examine the escalation effect have adopted a two-stage selection model to illustrate the escalation process, meaning that only the targeted states are provided with an opportunity to resist. While several studies find slightly negative or no relationship between having a defensive ally and the target’s likelihood to resist (Johnson and Leeds 2011, Fang, Johnson, and Leeds 2014, Wu 2016), a study with pooled alliance types (not distinguishing defense pacts from other alliance choices) find the opposite relationship (Senese and Vasquez 2008). Still, there has been no study that examines the escalation effect of defense pacts while addressing both the endogeneity issue of alliance choices and the sample selection issue. In the following section, this paper adopts the three-stage model described above to re-examine the relationship while taking both issues into account.

### 4.5.2 The Effects of Consultation Pacts on MID Initiation and Escalation

While defense pacts can be a useful foreign policy tool to reveal the defender’s interests in protecting its allies, there are other types of military alliances that send different signals. One of the other alliance obligations is a commitment to consultation, which does not promise active military assistance but promise coordination and cooperation in cases of conflict. Although both consultation pacts and defense pacts imply that the signatory is interested in coordinating and cooperating its security policies with its members, the absence of a defense pact reveals that the signatory is not fully committed to defend the potential target allies. Because consultation pacts also promise cooperation when conflicts occur but their
level of commitment is weaker than that of defense pacts, scholars have suspected whether consultation pacts can be alternatives to defense pacts regarding their effects on interstate disputes. In general, defense pacts incur greater ex post reputational costs than consultation pacts because 1) the violation of defensive commitment is easier to be detected by domestic and international audiences and 2) the (potential) target would value defense pacts more than consultation pacts, thus violating defensive commitment can hurt the alliance relationship more than violating consultation commitment.

Can precommitting to consultation be an alternative to precommitting to defense, and vice versa? Based on the fact that a defense pact and a consultation pact (without a defensive commitment) incur different amounts of reputational costs, we can extend the existing theoretical arguments about the effects of defense pacts on interstate disputes to examine the effects of consultation pacts on interstate disputes. First, we can think that a consultation pact will have similar effects on interstate dispute initiation and escalation but only to a lesser degree, because a consultation pact incurs smaller amount of costs than a defense pact. In this case, a consultation pact will have some deterrent effect compared to the cases without any alliance, but the effect will be smaller than that of a defense pact. More interestingly, if one thinks that a defense pact will encourage the targeted signatory’s resist when attacked (because its allies are more likely to come to an aid), a consultation pact will not encourage the targeted ally as much as the defense pact does. It may still encourage the target’s resist to some extend compared to no-alliance cases, but the effect will be relatively small.

Some scholars have focused on this smaller escalation effect of consultation pacts compared to that of defense pacts, and have argued that a consultation pact can be an alternative to a defense pact and vice versa. In particular, Snyder (1984) argues that states may prefer forming consultation pacts to forming defense pacts when they are concerned about the increasing chance of the targeted ally’s resistance due to promising a defense pact. In other words, because defense pacts may make the targeted allies more likely to resist, thus resulting
in more dispute escalations, states can promise a lower-level alliance commitment instead. In this context, at the cost of decreasing deterrent effect, the defenders can suppress the probability of the targeted allies’ resistance. To simply put, both the deterrent effect and the escalation effect could be proportional to the amount of ex post reputational costs of alliance choices, and there is a trade-off between the deterrent effect and the escalation effect.

On the other hand, the signaling argument by Fearon (1997) derives different sets of hypotheses about the deterrent effect and the escalation effect of consultation pacts. Fearon argues that if the defender state wants to reveal its interests in defending its allies, it should have formed a defense pact, not a consultation pact. Because forming a consultation pact is a partial (half-hearted) signal, the potential challenger would think that the defender is not committed to defend its allies. It is because if the defender is fully committed to defense, it would have formed a defense pact, not a consultation pact. Therefore, seeing a consultation pact, the potential challenger sees few changes in its probability of winning and will not deterred. Similarly, the targeted ally would not be “emboldened” – that is to say, does not become more likely to resist given the potential challenger’s demand – by consultation pacts, because it understands that the defender is not fully committed to intervene when conflicts occur. Therefore, according to this signaling argument, a half-hearted commitment is not different from no commitment regarding influencing other states’ behavior. In summary, according to this argument, a consultation pact has no impact on both dispute initiation and dispute escalation. The first chapter of this dissertation tests these arguments by adding the indicator of consultation pacts in the two-stage model of Johnson and Leeds (2011), and finds empirical supports for no relationship or even negative relationship between having consultation pacts and MID initiation as well as MID escalation. However, the study does not address the endogeneity issue of military alliance choices. To explore the possibility that consultation pacts can be alternatives to (or weaker versions of) defense pacts, I examine the deterrence effect and the escalation effect of consultation pacts, compared to those of defense pacts.
deterrent effect of Consultation Pacts: A state is less likely to become a target of militarized interstate disputes if there is at least one ally who precommits to consultation in the absence of a defense pact, compared to states who do not have any relevant ally. At the same time, the state is more likely to become a target of MIDs with the consultation obligation, compared to states who have at least one defensive ally.

Escalation Effect of Consultation Pacts: When a state becomes a target of militarized disputes, the effect of having at least one ally who precommits to consultation in the absence of a defense pact can be different depending on other conditions. Therefore, I do not provide an expectation for the escalation effect of consultation pacts.

4.5.3 Research Design

As mentioned earlier, this paper conducts a quantitative analysis in examining the deterrent effect and the escalation effect of defense as well as consultation pacts. The unit of analysis is a potential target state-year. One might be concerned about this monadic setting because many previous studies that examine the deterrent effect and the escalation effect have adopted a directed dyad-year settings. I understand that using directed dyadic level data sets allow us to capture the effects of both the potential challenger’s and the potential target’s characteristics on dispute initiation and escalation. However, the directed dyadic setting can be problematic for endogenous treatment models because the alliance choices can be highly clustered and often duplicated in a directed dyadic level data set. In other words, in the directed dyadic data set, one alliance choice of a potential target may be the results of only a couple of influential potential challengers, but the same alliance choice becomes applicable to all directed dyads of potential challengers and the potential target. Some alliance texts specify the list of applicable potential challengers, but many others do not provide the list explicitly. Therefore, the effects of state characteristics on alliance choices may be inflated (e.g., the level of threat could be low for most directed dyads that involve the potential target, but the potential target’s alliance choice could be applicable to all dyads) in
a directed dyad-year level data set. Also, the effects of alliance choices on dispute initiation and escalation can be underestimated (e.g., while the potential target’s alliance choices are applicable to all dyads, there are few cases where they are relevant to dispute initiation and escalation). Therefore, to avoid the issues of clustered alliance choices, I use a potential target state-year level data set. I use the states in the COW state system membership and the time period of this analysis is from 1816 to 2000, due to the data availability.

As described above, the equations in the first stage estimates the potential target’s alliance choices in a given year. This paper mainly compares the effects of defense pacts to those of consultation pacts on interstate disputes, so I build a mutually exclusive and exhaustive categorical variable of alliance choices. First, I code the variable one if the potential target state has at least one defensive ally in a given year. If the same alliance agreement promises both defense and consultation, I code the alliance as defense pacts, assuming that defense pacts incur higher reputational costs than consultation pacts. Second, I code the variable two if the potential target state has at least one ally who promises consultation when conflicts occur, but neither defense nor offense/neutrality. This is to separate the possible impacts of the other alliance choices from those of consultation pacts. Third, I code the variable three if the potential target state has at least one ally who promises offense or neutrality but not defense (it could promise consultation). This third category is to separate the effects of the first two alliance types (defensive and consultation commitments) from the effects of other alliance choices and the effect of not having any alliance. The decision to treat an alliance that has both consultation and offense/neutrality as a third category is not to dilute the effect of a consultation pact with the effect of an offense/neutrality pact. Because I do not have a clear idea about what the effect of a consultation pact would be as well as what the effect of an offense/neutrality pact on interstate disputes, I treat the alliances that have both commitments not as a consultation pact so that I can make a more conservative case for comparison. On the other hand, the effect of a defense pact, especially its deterrence effect, is more theoretically well-established and is expected to be larger than
that of a consultation pact (based on the idea that a consultation pact could be a “weaker” version of a defense pact), I code the alliance that has both defensive and offensive/neutrality commitments as a defense pact, placing it in the first category.

Finally, I code the variable four if there is no ally who promises any of the four alliance obligations to the potential target state. Nonaggression pacts have been considered to be different from other alliance obligations because 1) they have been considered as conflict management devices among the signatories, not necessarily influencing the third-parties’ decisions regarding conflicts (Mattes and Vonnahme 2010, Lupu and Poast 2016). Therefore, I combine that category with no-alliance cases. Figure 8 summarizes the coding rule. For coding the variable, I use the ATOP data set (Leeds et al. 2002). In coding conditional, unconditional, secret, and public alliances, I adopt the coding rules of Johnson and Leeds (2011) and Johnson, Leeds, and Wu (2015). The variable is first coded at the triadic level (potential alliance partner-potential target-potential challenger-year), collapsed to directed dyadic level (potential target-potential challenger-year) to match the following MIDs variable, and finally collapsed to the potential target-year level.

After the estimation, the alliance choices become the main independent variables in the second and the third equation. The outcome variable in the second equation is a binary variable, coded one if there is at least one relevant MID targeting the state in a given year. Because of the monadic setting, I code all MIDs as relevant for unconditional alliances or
cases without any alliance. For conditional alliances, I only code the MIDs initiated by relevant potential challengers. If I code all MIDs for the potential target-year observations, the MIDs initiated by other states who are not relevant to the alliances (i.e., not supposed to be deterred by the alliances) are included in the data set. This does not seem to be a good decision because it would add irrelevant conflict cases in the dependent variable, those the alliances should not affect. This could result in making false accusations of deterrence failure and restraint failure. One theoretical argument that justifies all MIDs could be the two-good theory (Palmer and Morgan 2006), arguing that even conditional alliances can affect the state’s other foreign policy decisions by redistributing its available resources. At the same time, the dispute initiation variable in this paper is the third state’s attacking the alliance member, not the alliance member’s attacking other states. Therefore, for this analysis, I think it makes more sense not to include the MIDs that are not subjects of the alliances for my main results. At the same time, I run the same model with all MIDs and show the results as a robustness check in the following section. Furthermore, the outcome variable in the third equation is also a binary variable, whether the targeted state responded militarily (coded as one) to the initiated MIDs in a given year. This variable is missing if there is no MID initiated in a given year. For coding the responses, I utilize the Maoz’ dyadic MIDs (2005; Ghosn, Palmer, and Bremer 2004) used in Johnson and Leeds (2011), collapsed to the potential target-year level.

Also, there are control variables that are included in all of the three sets of equations. First, I control for the potential target’s national capabilities, using the COW CINC (Composite Index of National Capability) scores (Singer et al. 1972; Singer 1987). The state capabilities can affect the state’s choice of alliance types because states with different capabilities may have different policy goals, and they can have different levels of bargaining.

Although I use a state-year level data set for my estimation, I start building the data set at the directed dyad-year level, especially to properly code the conditional and secret alliances and the relevant MIDs. After coding the variables, I collapse the directed dyad-year level data set to a potential target-year level data set. This coding scheme can (at least partially) attenuate the concerns about simply using a state-year level data set.
power in negotiating the alliance choices. At the same time, the state capabilities also affect the state’s likelihood of becoming a target of MIDs and the state’s decision to escalate the disputes. Second, I also control for the overall level of threat the potential target state faces (Bennett 1997; Signorino and Ritter 1999; Leeds and Savun 2007; Chiba, Johnson, and Leeds 2015; Johnson 2015). As the potential target state faces higher levels of overall threat, the state is more likely to seek alliance partners. At the same time, the overall level of threat is a good predictor of the state’s likelihood of being a target of MIDs and its decision to resist.

Third, I control for other predictors of interstate disputes at the state level, such as the number of borders that the potential target has (EUGene software: Bennett and Stam 2000), a binary indicator of whether the potential target makes any territorial claims or other states are making any territorial claims against the potential target (ICOW data set, the provisional version 1.01 of the territorial claims data set: Hensel et al. 2008; Frederick, Hensel, and Macaulay 2014). Fourth, I code whether the potential target has an ongoing war using the COW war data set, and the Cold War period to capture any differences at the international system level. Finally, because democracies may respond differently to interstate disputes (Reiter and Stam 2002; Bueno de Mesquita et al. 2003) and domestic political institutions can affect alliance choices (Mattes 2012; Chiba, Johnson, and Leeds 2015), I control for the potential target’s regime type. It is coded one if the potential target is a democracy, using the threshold of six or higher on the Polity2 variable (Polity IV: Marshall, Jaggers, and Gurr 2010).

Regarding the identification of the model, the list of independent variables for the first two stages (alliance choice stage and dispute initiation stage) is the same. In other words, I do not have an identification variable for the two stages, thus relying on the distributional assumptions about the error terms (that the two sets of error terms are normally distributed). I understand that one can be concerned about this issue. Because there is no identification variable, the estimates I get can be less reliable compared to the ones that have identification variables. At the same time, as discussed in the previous chapter, the
distributional assumption is quite robust and may attenuate the issue by the latent variable approach. Furthermore, to differentiate the third stage (dispute escalation stage) from the first two, I exclude the regime type variable. Omitting the variable from the equation is in accordance with the Johnson and Leeds (2011)’s decision. Johnson and Leeds (2011) use the challenger’ defensive alliance membership, distance, S-score, and joint democracy in their directed dyad-year level data set. However, I use a monadic potential target state-year level data set, in which these variables cannot be defined.

At the same time, motivated by the fact that the fourth variable is to address democratic peace in general, I include the democracy variable mentioned above in the dispute initiation stage but not in the dispute escalation stage. There have been studies that relate democratic regimes to dispute initiation, mainly through the argument that their governments are more susceptible to the costs of acting against their precommitments, being belligerent (Fearon 1994; Levy et al. 2015; Kertzer and Brutger 2015), and losing a war the government initiates (thus try harder: Bueno de Mesquita et al. 2005). However, it is less clear whether a democratic government is more likely to resist when attacked or not. It is because the mechanisms mentioned above is about attributing the responsibility of “initiating” a foreign policy to the government, while the attribution of responsibility for “responding” militarily is not apparent. Democracies may be more likely to back down in a prolonged conflict because their policy is more likely to be affected as wartime casualties increase, but it does not mean that they would be less likely to resist in the first place. Rather, if a democracy sees a good chance of winning a war, it may resist and fight harder to end the war quickly. Another potential mechanism is that democracies are more likely to intervene in favor of the other attacked democracies, but neither its mechanisms nor relevant results are clear (Reed 2000; Reiter and Stam 2002).

I run the Bayesian multiple equation model explained above, with 50,000 iterations and 20,000 burn-ins. For the priors of beta coefficients, I use a multivariate normal distribution whose mean vector consists of zeros and whose precision matrix is $100 \cdot I^{50}$. For the priors
of the variance-covariance matrix of the error terms in the system of equations, I use a 5-dimensional identity matrix. In other words, I use a diffuse prior for the coefficient estimates because I do not want to restrict the posterior in any sense without having good information about the priors. In addition, by adopting (a version of) identity matrices for the priors of the variance-covariance matrices of coefficient and correlation estimates, I do not impose any informative assumptions about correlations between coefficients or those between foreign policy decisions (i.e., correlation between a defense pact and dispute initiation). I have used Microsoft R Open version 3.4.3 for the estimation.

4.5.4 Key Results

1.5.4.1. Description of Main Results Table 10 shows the summary results of the Bayesian multiple equation model on the effects of alliance choices on MID initiation and escalation. Regarding the effect of a defense pact, having a defense pact is almost always associated with lower chances of being a target of MIDs, compared to the cases where the potential target does not have any relevant alliance. The median of the posterior density is -0.484 and its 95% credible interval is [-0.80, -0.09]. In other words, given the diffuse prior (without informative priors), more than 95% of the most probable values for the effect of a defense pact on the chances of being attacked is negative. The empirical results provides some support for the deterrent effect of a defense pact. Furthermore, having a defense pact is also negatively associated with the targeted ally’s decision to resist in most cases, but not as certain as the deterrent effect of a defense pact.

Regarding the effect of a consultation pact as well as the effect of an offense pact and/or a neutrality pact, their posterior distributions are very dispersed. To be more specific, the

The R libraries that are used in building and/or running my estimator are: mvtnorm, QRM, VGAM, corpcor, tmvtnorm, bayesm, truncnorm, and clusterGeneration. The hardware specifications are: Intel(R) Xeon(R) CPU E5-2620 0 (2.0GHz: 2 processors), 48 GB of RAM, with a 64-bit Windows 10 Pro OS.
posterior median values for the effects of both alliance choices are negative (-0.449 and -0.249). At the same time, the variances of the two densities are much larger than that of the posterior density for the effect of a defense pact. In general, it is likely that all of the three alliance choices (a defense pact, a consultation pact, and an offense/ neutrality pact) decrease the chance of being a target of MIDs. However, if one asks how certain the effects might be, the effects of the latter two alliance choices are not as certain as the deterrent effect of a defense pact. We can draw similar conclusions for the escalation effect of the two alliance choices. Similar to a defense pact, a consultation pact is negatively associated with the targeted ally’s dispute escalation, while having an offense pact or a neutrality pact is associated with increasing chances of dispute escalation. Still, the posterior densities of all of the alliance choices are quite dispersed, suggesting that there are considerable odds that the sign of the effects can be opposite.

Although I am most interested in examining the effects of different alliance choices on MID initiation and escalation, I can also examine how control variables affect alliance choices, MID initiation, and MID escalation. It is worth reiterating that I use the same set of control variables for predicting alliance choices and for predicting MID initiation. Therefore, I understand that this model could suffer from the identification issue, but at the same time, it does not necessarily mean that the estimates are unreliable as explained above. First, a country’s national capability is almost certainly positively associated with all of the alliance choices as well as MID initiation. Because the unit of analysis is state-year and it does not incorporate any information regarding the state’s alliance partners, it is particularly difficult for me to interpret the results at the alliance formation stage. The results do not translate to the argument that stronger states are more likely to make alliance commitments to other states, because the alliance choices variable is about “having at least one ally who promises such commitment to the state.” Of course, many alliances are symmetric alliances and that might have been reflected in this result, but it is more accurate to say that this suggests that stronger states are more likely to have allies who are committed to defend, consult with, and
coordinate to exert power beyond its sovereignty with them or remain neutral. Regarding MID initiation, the finding may comport with the well-known previous finding that stronger states are more likely to get involved in interstate conflicts, but is also different in the sense that it suggests that stronger states are more likely to be “targets” of interstate disputes.

Second, a state’s (observable) level of external threat is mostly positively associated with all of the foreign policy decisions except the decision to sign a consultation pact. This conforms to the theoretical expectation above. The effect of external threat on the probability of escalating conflicts may not be very straightforward, and the positive association is less clear for the decision. I suspect that the relationship depends on who initiated the conflict: If the major threat state is the initiator, there is good amount of chance for the attacked state to reciprocate militarily. On the other hand, if a third-party state initiates a conflict, the state could be more reluctant to escalate the conflict, facing another major potential challenger. Also, the posterior density regarding its effect on the decision to sign a consultation pact is symmetric around zero. This suggests that the level of external threat that the signatories of a consultation pact face is no different from that states who do not have any alliance face. This can be interpreted into two ways: 1) states who face relatively low level of external threat are more likely to seek consultation pacts, compared to the ones who seek defense pacts or offense/neutrality pacts; 2) level of external threat might not be the main motivation for states to sign a consultation pact, and there are other motivations. Both possibilities have been suggested in the IR literature, but I cannot tell which one is the case only from reading these results.

Third, contiguity is mostly associated positively with all of the outcome variables as expected. Furthermore, being a target of at least one territorial claim is generally associated positively with signing a defense pact, signing an offense/neutrality pact, and the probability of being a target of MID and the probability of responding militarily when attacked. This suggests that the target states of territorial claims are more likely to seek alliances (compared to not having any alliance), except for a consultation pact. Again, this brings up the possi-
bility that 1) states think that consultation pacts do not prevent potential challengers from making demands and/or 2) consultation pacts are signed for the reasons other than territory and sovereignty. On the other hand, having at least one claim regarding territory is more likely to dissuade states from signing alliances, interestingly even for an offense pact/neutrality pact compared to not having any alliance. At the same time, both of these variables seem to increase the probability of “being involved” in conflicts, either by being a target of MIDs or by escalating the conflict when targeted.

Fourth, having an ongoing war is more likely to be associated with increasing chance of seeking a defense pact or an offense/neutrality pact. Furthermore, as expected, it is more likely to increase the chance of being a target of MIDs but decrease the chance of responding to the initiated MIDs militarily. On the other hand, different from the other alliance choices, state who are in the middle of a war is less likely to form a consultation pact compared to not having any alliance. In other words, states who have an ongoing war would rather not have any alliance than having a consultation pact. This reiterates the argument that consultation pacts might be formed for other reasons. Furthermore, the Cold War period increases the chances of making all of the decisions: states are more likely to sign a defense pact, a consultation pact, and an offense/neutrality pact, and are more likely to be MID targets as well as more likely to reciprocate militarily.

Finally, a democratic state is more likely to form any of the alliances, and is more likely to be a target of MIDs. The interpretation of this finding, especially for the deterrent effect, is not very intuitive. As aforementioned above, there is no clear theoretical expectation regarding the effect of being democratic on the probability of being attacked. In accordance with the expectation, I did not find any clear relationship between the two in the previous chapter. This is one of a couple of results that are different from the ones in Chapter 3 (the other is the reversed effect sign for the effect of making at least one territorial claim), and needs to be investigated further in the future.

In sum, I find that most of the effects of control variables on alliance choices, dispute
initiation, and dispute escalation conforms with our theoretical expectation. At the same time, their effects on signing consultation pacts are often quite different from those on signing either defense pacts or offense/neutrality pacts. This difference suggests that the decision to form a consultation pact may be driven by fundamentally different factors than the ones that drive signing a defense pact and/or an offense/neutrality pact. If a consultation pact is simply a “weaker” version of a defense pact, I would expect the coefficients regarding the formation of consultation pacts to be smaller than those regarding the formation of defense pacts but are in smaller degree. On the other hand, the associations are often in much smaller degrees (often very close to zero) and some of the associations are in opposite direction compared to those regarding signing a defense pact.

Furthermore, Table 11 shows the estimates of correlations among the three decisions – potential target’s decision to choose alliance types, its likelihood of being a target of MIDs (i.e., potential challengers’ decisions to attack the potential target state), and the targeted state’s decision to escalate the conflict given the initiated MID(s). As shown in Table 11, there is a positive correlation between the state’s decision to form any alliance (in particular, defense pacts with more certainty) and the state’s likelihood of being a target of MIDs. This indicates that the unobservable confounders that facilitate forming a defense pact also increases the state’s chances of being a target of MIDs. This conclusion is in accordance with the second chapter that also addresses the endogeneity issue of the alliance choices. In other words, after accounting for the fact that the baseline probability of being a target of MIDs is higher for those who sign defense pacts, having a defense pact decreases the chance of being a target of MIDs. This finding is an important contribution to the IR literature, because according to the finding, defense pacts seem to have deterrent effects, but the effects could be watered down (and be negligible) if we ignore that states who are at higher risks of being MID targets are more likely to sign defense pacts. Therefore, adopting the endogenous treatment model can be useful if a scholar is concerned about the non-randomness of alliance choices.
Also, we find almost the same positive correlation between the state’s decision to form offense/neutrality pacts and the state’s likelihood of being a target of MIDs. It also suggests that states who are at higher risks of being targeted are more likely to seek offense pacts – possibly because the states are expecting war in the near future – and/or neutrality pacts, not to getting involved in interstate conflicts. Therefore, I am more confident to argue that given the findings, states who choose to form a defense pact, an offense pact, and/or a neutrality pact are at higher risks of being targets of MIDs, compared to the states who do not have any alliance.

Interestingly, after accounting for the positive correlations between alliance choices and the MID initiation, the alliance choices are associated with decreasing chances of MID escalation, but the estimated correlations are not statistically significant at conventional levels. What does this mean substantively? For example, we can think of candidates of unobservable confounders. One candidate of unobservable confounders is the political leader’s subjective assessment of threat, which is inherently very difficult to measure. If we fail to control for the subjective threat level, that variable will facilitate forming defense pacts (to enhance her country’s security), given that the probability of being a target of MIDs is higher. This example explains the positive correlations between having defensive ally and the probability of being targets of MIDs. On the other hand, this could also explain the negative correlation between having defensive ally and the probability of conflict escalation. Given that the leader sees high probability of being attacked (and thus seeking defense pacts), the leader could be reluctant to escalate conflicts when attacked by a state, concerned about the prospect of having multiple interstate conflicts. Another example to explain the negative correlations between forming alliances and dispute escalation is that aggressive states (who are revisionists or are willing to change the status quo) are less attractive as alliance partners to other states. In this case, states who are more likely to respond militarily when attacked (thus are more war-prone) are less likely to find alliance partners, because its potential alliance partners are concerned about getting involved in military conflicts. At the same time, whether this
example is applicable to offense/neutrality pact cases is in question, because states who are likely to change the status quo can be good candidates for the alliance partners of an offense pact. At the same time, it is worth pointing out that offense pacts are rare in the ATOP data set, and several empirical studies have shown that having a neutrality pact decreases the chances of disputes among signatories (Mattes and Vonnahme 2010; Lupu and Poast 2016), which dispute escalation consists of.

Finally, I run the same model with another dependent variable, all MIDs instead of relevant MIDs, and reach substantively the same conclusion. Table 13 shows the coefficient estimates using the alternative dependent variable and Table 14 shows the correlation estimates using the alternative dependent variable. Regarding the effects of alliance choices, the posterior median of the effect of a consultation pact is positive in Table 13 (was negative in Table 10), but given that the posterior density is very diffuse, I do not think it affects our understanding about the relationship considerably.

One might wonder whether utilizing this multiple-equations model to examine the effects of alliance choices on interstate disputes is crucial for our inference. I argue that applying this model to examine the effects of alliance choices on interstate disputes is important because the proposed model provides results that are different from the results that existing statistical models provide and because it provides additional information, such as the correlations between choices described above. Table 12 compares the key results of the proposed model to those of the existing models, such as two separate probit models (one predicting dispute initiation, the other predicting dispute escalation) and Heckman sample selection model for binary outcomes. Neither probit nor the two-stage sample selection model addresses the endogeneity issue. The two-stage categorical endogeneity model by Burgette and Nordheim (2012) does not provide easily comparable coefficients, thus not presented in this table, but I plan to add the comparison in the future.

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As explained earlier, their model is an extension of the von Stein (2005)’s approach, separately estimating the outcome equations for every value of the endogenous treatment. Therefore, one can only figure out how different treatment values affect the outcome differently by calculating the substantive effects. Calculating
In short, addressing the issues of endogeneity and non-random sample selection provides us with the empirical findings that we could not have known if we utilize other regression models. Both the separate probit models and the Heckman selection model are not able to capture the deterrent effect of a defense pact, which is captured in the proposed model. It is because the proposed model is able to set different baseline probability of entering each alliance choice by estimating the correlations between alliance choices and interstate dispute decisions, conditioning on the control variables. What is also interesting is that the negative and statistically significant associations found in the models that ignore endogeneity between having a defense pact and conflict escalation when attacked loses its significance when I utilize the proposed model. The negative association is in accordance with the previous empirical finding (Johnson and Leeds 2011: their unit of analysis is directed dyad-year), but if we account for the fact that states who are more likely to resist have difficulty in forming alliances, alliance choices (in particular, defense pacts) may not have much impact (or have mixed effects) on the targeted ally’s decision to escalate MIDs.

1.5.4.2. Calculation of Substantive Effects Figure 10 shows the distribution of differences in predicted probabilities of being a target of MIDs conditioning on each endogenous alliance choice, compared to the no relevant alliance category\footnote{The density plot shows the overall distribution of the differences. The percentages show how many of the estimated substantive effects are negative. For example, when we compare having a defense pact to not having any relevant alliance, 98.1% of the estimated effects are negative, suggesting that having a defense pact is almost always associated with smaller chances of being a target of MIDs, compared to no relevant alliance. The boxplot below the density plot shows the 25th, the 50th, and the 75th quantiles of each distribution. The numbers below each boxplot show the substantive effects using their model and comparing those to those of the proposed model could be worth trying.} The density plot shows the overall distribution of the differences. The percentages show how many of the estimated substantive effects are negative. For example, when we compare having a defense pact to not having any relevant alliance, 98.1% of the estimated effects are negative, suggesting that having a defense pact is almost always associated with smaller chances of being a target of MIDs, compared to no relevant alliance. The boxplot below the density plot shows the 25th, the 50th, and the 75th quantiles of each distribution. The numbers below each boxplot show the substantive effects using their model and comparing those to those of the proposed model could be worth trying.

Figure 12 shows the posterior distribution of coefficients, which cannot be directly translated to a substantive effect. Therefore, I do not discuss the posterior distribution in detail.
the 2.5th, the 50th, and the 97.5th quartiles of the distribution, applicable to hypothesis testing.

Calculating a substantive effect for an endogenous treatment model is difficult because we have to condition on the endogenous alliance choice. In general, the previous literature (Timpone 2002; Schultz and Stein 2005) has suggested that one can hold the previous stage the same, and then make a change of interest in the following stage. Following the suggestion, I first assume the situation where a potential target has a defense pact (fixing the alliance choice stage). Second, given that the state has a defense pact, in the second stage (MID initiation), I calculate the corresponding probabilities for two hypothetical cases: 1) being a target of MIDs when the state keeps having the defense pact and 2) being a target of MIDs when the state no longer has the defense pact (again, I assume that the state initially formed a defense pact in the alliance choice stage; in this second stage, I compare having a defense pact to not having any relevant alliance). Then, I take the difference between the two predicted probabilities. I repeat this for every pairwise comparison between each alliance choice and the no-alliance category. The formula for calculating the substantive effect of having a defense pact is below, which can be expanded by Bayes’ rule.

\[
\begin{align*}
&= Pr(\text{dispute with the defense pact} \mid \text{initially having a defense pact}) - Pr(\text{dispute without the defense pact} \mid \text{initially having a defense pact}) \\
&= \frac{Pr(\text{dispute with the defense pact} \& \text{initially having a defense pact})}{Pr(\text{initially having a defense pact})} - \frac{Pr(\text{dispute without the defense pact} \& \text{initially having a defense pact})}{Pr(\text{initially having a defense pact})}
\end{align*}
\]

In general, even in a high-dimensional multivariate normal distribution, calculating a marginal distribution over a subset of random variables is not that difficult: the marginal distribution of a multivariate normal is simply the normal distribution with the relevant
mean vector and variance-covariance matrix. However, in this multinomial probit setting, I cannot simply take the parameters for having a defense pact (and dispute initiation). This is because the alliance choices are mutually exclusive: choosing one type of alliance automatically means that the state does not choose any other alliance category. Therefore, the conditional probability becomes more complicated. For example, we have to calculate the probability of dispute initiation with the defense pact, given that the state initially decides to form a defense pact, decides not to form a consultation pact, decides not to form an offense/neutrality pact, and decides not to have no relevant ally.

Because deriving the formula for the above situation analytically is difficult, I simulate samples using the estimates for each iteration of the Markov chain, calculate relative frequencies for each situation (e.g., dispute with a defense pact, dispute with a consultation pact, dispute with an offense/neutrality pact, etc.), and use the ratio of occurrence as predicted probabilities. To be more specific, 1) for each iteration of the Markov chain, I take the coefficient estimates ($\beta$s) and the variance-covariance matrix estimates of the error terms. 2) Using the coefficient estimates and setting up a universal typical case for any alliance choice (median of the control variables for the entire data set), I calculate linearly predicted values ($X\beta$) of alliance choices, dispute initiation, and dispute escalation. 3) Then, I use the linearly predicted values as the mean vector of a 5-dimensional multivariate normal distribution and the estimates of the variance-covariance matrix as the variance-covariance matrix of the multivariate normal distribution and draw 5,000 samples from the distribution. The samples are the estimates of the latent variables under the model. 4) Furthermore, I classify the latent variables as discrete choices (e.g., if $X\beta_{\text{dispute}} > 0$, I classify it as a dispute case) and create a contingency table of the all possible cases and calculate the relative frequencies of each case. 5) Finally, I take the ratio of occurrence as predicted probabilities and calculate the difference in predicted probabilities according to the formula above. 6) I repeat this process for every iteration of the Markov chain except the burn-ins (30,000 iterations in this paper) for each alliance choice. By running this simulation, I obtain 30,000 simulated differ-
ences in predicted probabilities, comparing each alliance choice to the no-alliance category. In general, all of the three alliance choices (either a defense pact, a consultation pact, or an offense/neutrality pact) are associated with lower probabilities of being a targets of MIDs, compared to not having any relevant alliance. In particular, it is almost certain (there are few uncertainties) that signing a defense pact (moving from the no-alliance category to the defense pact category) decreases chance of being attacked.

Furthermore, even if we consider that this is a monadic state-year level data set (thus MIDs are not rare as they are in a directed dyadic level data set), the magnitude of the difference is substantial: the median of the estimated substantive effects is -0.099, suggesting that having a defense pact decreases the probability of being targets of MIDs by approximately 10% on average. Also, What is also interesting is that the median of the three estimated effects are quite similar, raising a question to the idea that having a consultation pact would have a smaller deterrent effect than having a defense pact. In other words, on average, it could be the case where a consultation pact can exert almost the same amount of influence on deterring potential challengers as a defense pact does, though there are much greater variations in the size of the effects. As mentioned in the earlier section, I do not discuss whether the large variation suggests that a consultation pact has no effect or a consultation pact can have different signs of effects (both positive and negative). Investigating the conditions under which a consultation pact can have a positive or negative impact, or have no effect at all is left for future research. Going back to the substantive effects of consultation pacts, one thing to note is that the estimated effects are very disperse. Approximately 15% of the estimated substantive effects for having a consultation pact is positive, suggesting that there are non-ignorable chances of a consultation pact encouraging a MID initiation by potential challengers. Regarding the offense/neutrality category, most of the substantive effects are negative, but still 13% of the estimated substantive effects can be positive. The distribution is more concentrated than that of a consultation pact.

Figure 11 shows the distribution of differences in predicted probabilities of responding
militarily, conditioning on the endogenous treatment choices and the fact that there is at least one dispute initiated. Again, each density plot shows the overall distribution of the simulated differences, while the percentages show how many of the simulated differences are positive. The boxplot and the numbers below each boxplot convey the same summary statistics as the ones in Figure 10. In accordance with the calculation in Figure 10, I extend the Schultz and Stein (2005)’s way of calculating the substantive effect to a three-stage model. In other words, I hold both the endogenous alliance choice stage and the dispute initiation stage the same, and compare two hypothetical situations of dispute escalation. For example, for the defense pact category, I first assume that all states have at least one defense pact. Given that all states have at least one defense pact, I examine the probability of being attacked under the same defense pact. Then, narrowing down the sample to the cases where there is at least one militarized dispute (initiated by other states) with the defense pact, I compare 1) the probability of resisting with the defense pact to 2) the probability of resisting when we take out the defense pact only for the conflict escalation stage. As described above, I apply the same simulation approach to calculate the predicted probabilities.

\[
\Pr(\text{resist with the defense pact} \mid \text{dispute with the defense pact \\& initially having a defense pact}) - \\
\Pr(\text{resist without the defense pact} \mid \text{dispute with the defense pact \\& initially having a defense pact})
\]

\[
= \frac{\Pr(\text{resist with the defense pact \\& dispute with the defense pact \\& initially having a defense pact})}{\Pr(\text{dispute with the defense pact \\& initially having a defense pact})} - \\
\frac{\Pr(\text{resist without the defense pact \\& dispute with the defense pact \\& initially having a defense pact})}{\Pr(\text{dispute with the defense pact \\& initially having a defense pact})}
\]

The results in Figure 11 show do not suggest clear relationship between having any of the alliance choices on the targeted ally’s probability of responding militarily. In general, Figure 13 shows three posterior densities of coefficients (one for each alliance choice), compared to the no-alliance baseline category. Again, for the same reason discussed above, I do not discuss the distribution of the coefficients in detail.
the differences are slightly more likely to be negative for a defense pact and a consultation pact, but there are substantial chances for the differences to be positive. The results are not particularly surprising, considering that there is no consensus about the effects of a defense pact on the targeted ally’s decisions to escalate conflicts, while there has been not many theoretical arguments developed about the other two. The first density being almost symmetric around zero may suggest that after controlling for the endogenous alliance choices and dispute initiation, a defense pact may 1) not have any impact on dispute escalation or 2) have a conflicting effect. For the first case, it could indicate that the escalation effect of defense pacts can be attenuated by tuning the defensive alliance text, not necessarily switching to other alliance choices (e.g., a consultation pact). Regarding the second case, depending on the other conditions that are not identified in this chapter, the effect of a defense pact on dispute escalation can be either positive or negative. Regarding the escalation effects of consultation pacts, the effects are close enough to zero and there is a huge variation. This finding does not contradict existing theories, but from this evidence only, we cannot conclude whether consultation pacts have any impact on conflict escalation or the conflicting effects of consultation pacts are averaged to zero. Therefore, it will be interesting to investigate the conditions under which allies make different recommendations (especially for defense pacts) and whether a consultation pact can work as a leverage or venue to influence the allies’ foreign policy decisions.

Furthermore, one might wonder what we can learn if we compare the effect of one alliance type to that of the other. Figure 14 shows the substantive effects of an alliance choice, compared to the other types of alliances. The figures are created in the same way as described earlier, simply subtracting the effect of one alliance type from the other alliance type’s. For example, the first distribution in Figure 14 shows the difference between the deterrent effect of a defense pact and the deterrent effect of a consultation pact. As we can expect from

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Note that Figure 10 and Figure 11 show the substantive effect of each alliance type on MIDs, compared to non-alliance category.
Figure 10, it is difficult to conclude that having a defense pact shows considerable advantage in deterring potential challengers than having a consultation pact. The difference between the two sets of effects are centered around zero, while having a longer tail on the left-hand side of the distribution. The longer tail on the negative differences suggests that there might be a small set of cases where having a defense pact can be particularly better at deterring potential challengers than having a consultation pact, while in most of the cases they yield similar degrees of deterrent effects. The asymmetry of the distribution mainly comes from the large variance in the deterrent effect of a consultation pact, which this dissertation does not provide an answer to but can be investigated in future studies.

On the other hand, having a defense pact is mostly associated with lower chances of being a target of MIDs, compared to having an offense/neutrality pact. 92.7% of the predicted differences in the deterrent effects are negative. Again, it could be because a defense pact is specifically designed to deter potential challengers, while external deterrence may not be the main motivation of signing an offense pact or a neutrality pact. Finally, the differences between the deterrent effect of a consultation pact and the deterrent effect of an offense/neutrality pact are very diffused. Compared to an offense/neutrality pact, having a consultation pact is more likely to reduce the probability of being attacked (67.8% of the differences are negative), but the variability of the differences is quite large.

Similarly, Figure 15 shows the pairwise comparisons of the escalation effects of alliance types. As expected from Figure 11 and Figure 13, there are considerable chances for the differences in predicted probabilities to be either positive or negative. Again, the large variations in the first and third distributions are mainly due to the large variance in the effect of a consultation pact. On the other hand, it is more certain that having a defense pact is likely to be associated with decreasing chance of responding militarily compared to having an offense/neutrality pact. Revisiting Figure 11 and Figure 13, it is because the escalation effect of an offense/neutrality pact is skewed to the right (i.e., the majority of the predicted probabilities of MID escalation with an offense/neutrality pact compared to
non-alliance case are positive).

4.6 Conclusion

This chapter examines how different military alliance choices affect the initiation and the escalation of militarized interstate disputes. The main contribution of this chapter compared to those of the previous ones is twofold. First, I test how defense pacts and consultation pacts affect states’ likelihood of being targets of MIDs and the targets' likelihood of resisting when challenged. Furthermore, I address two methodological challenges described above (both issues of categorical endogenous treatment and sample selection) on top of making the first contribution. Similar to the conclusions in Chapter 3, I find that states who are at higher risks of being targeted are more likely to seek any type of alliances in general – defense pacts, offense pacts, and/or neutrality pacts are the alliance choices examined in this chapter. Therefore, we should acknowledge that those who sign these types of military alliances are likely to have higher baseline probability of being involved in interstate conflicts. Another caveat that conforms to the conclusion of Chapter 3 is that after accounting for the higher baseline probability of conflicts, having a defense pact is associated with decreasing probability of being a target of MIDs. At the same time, having a defense pact is not associated with increasing or decreasing probabilities of responding to the initiated MIDs militarily.

On the other hand, I am less certain about the effects of a consultation pact on interstate disputes. Given the finding, it is not clear whether a consultation pact can have comparable effects (with different degree) to those of a defense pact. It is mainly due to the lack of development in theoretical arguments regarding its effects. Of course, the Fearon’s formal model about signaling and resolve (1997) suggests that having a consultation pact – which can be framed as a half-hearted commitment – is not associated with MID initiation and escalation. At the same time, the finding of this chapter can be interpreted as a mixed finding
or a non-finding. Understanding that the effects of consultation pacts can be very divergent or even none, identifying the conditions under which states sign a consultation pact can be very helpful. There have been several studies that approach the issue from a domestic politics perspective (Mattes 2012a) and from a screening mechanism (Chiba, Johnson, and Leeds 2015), but they do not necessarily assume how a consultation pact itself can be useful to the alliance members. Deriving better expectations about the relationship between a consultation pact and interstate disputes directly from a well-established theoretical argument can be the next step for future research.

Finally, the Bayesian multiple equation model developed in this project can be applicable to test the effect of international institutions on states’ foreign policy in other issue areas. Because both endogenous treatment and sample selection issues can be addressed as omitted variable bias issue (the same structure), one can take this model and directly apply it or make some small changes to illustrate the situation better. Although this is not a panacea for the two methodological issues, the model is more comprehensive and generalizable than other models that are often used in the discipline. Compared to the other chapters, this chapter complements the methodological issues that exist in Chapter 2 and Chapter 3 by utilizing the Bayesian multiple equation model. Chapter 2 does not address either the endogeneity issue and Chapter 3 does not address the issue of multiple alliance choices and does not examine the effect of defensive alliances on dispute escalation. It is encouraging that the main findings of these three chapters are quite consistent, especially for the effect of different alliance choices on interstate disputes. At the same time, there are many questions remaining regarding the varying levels of uncertainty about the effects across alliance choices both theoretically and
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Table 4.1: Average Absolute Difference between True Value and an Estimator ($E(|\beta - \hat{\beta}|)$)

Note: the numbers in bold indicate the smallest bias for each parameter, indicating most unbiased estimates.
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<td></td>
<td>0.087</td>
</tr>
</tbody>
</table>

Table 4.2 : Root Mean Square Error of Estimates
Note: the numbers in bold indicate the smallest RMSE for each parameter, indicating most unbiased estimates.
<table>
<thead>
<tr>
<th>Coefficients</th>
<th>Bayesian MNP</th>
<th>Probit ((U))</th>
<th>Probit ((Y))</th>
<th>Bayesian selection</th>
<th>Proposed</th>
</tr>
</thead>
<tbody>
<tr>
<td>(\zeta_{10})</td>
<td>0.920</td>
<td></td>
<td></td>
<td></td>
<td>0.924</td>
</tr>
<tr>
<td>(\zeta_{11})</td>
<td>0.928</td>
<td></td>
<td></td>
<td></td>
<td>0.920</td>
</tr>
<tr>
<td>(\zeta_{12})</td>
<td>0.926</td>
<td></td>
<td></td>
<td></td>
<td>0.915</td>
</tr>
<tr>
<td>(\zeta_{13})</td>
<td>0.921</td>
<td></td>
<td></td>
<td></td>
<td>0.914</td>
</tr>
<tr>
<td>(\zeta_{14})</td>
<td>0.932</td>
<td></td>
<td></td>
<td></td>
<td>0.927</td>
</tr>
<tr>
<td>(\zeta_{20})</td>
<td>0.883</td>
<td></td>
<td></td>
<td></td>
<td>0.904</td>
</tr>
<tr>
<td>(\zeta_{21})</td>
<td>0.978</td>
<td></td>
<td></td>
<td></td>
<td>0.969</td>
</tr>
<tr>
<td>(\zeta_{22})</td>
<td>0.980</td>
<td></td>
<td></td>
<td></td>
<td>0.967</td>
</tr>
<tr>
<td>(\zeta_{23})</td>
<td>0.973</td>
<td></td>
<td></td>
<td></td>
<td>0.962</td>
</tr>
<tr>
<td>(\zeta_{24})</td>
<td>0.976</td>
<td></td>
<td></td>
<td></td>
<td>0.965</td>
</tr>
<tr>
<td>(\gamma_0^*)</td>
<td>0.570</td>
<td>0.571</td>
<td></td>
<td></td>
<td>0.938</td>
</tr>
<tr>
<td>(\gamma_1^*)</td>
<td>0.507</td>
<td>0.506</td>
<td></td>
<td></td>
<td>0.938</td>
</tr>
<tr>
<td>(\gamma_2^*)</td>
<td>0.489</td>
<td>0.484</td>
<td></td>
<td></td>
<td>0.928</td>
</tr>
<tr>
<td>(\gamma_3)</td>
<td>0.940</td>
<td>0.920</td>
<td></td>
<td></td>
<td>0.942</td>
</tr>
<tr>
<td>(\gamma_4)</td>
<td>0.942</td>
<td>0.922</td>
<td></td>
<td></td>
<td>0.940</td>
</tr>
<tr>
<td>(\gamma_5)</td>
<td>0.944</td>
<td>0.921</td>
<td></td>
<td></td>
<td>0.944</td>
</tr>
<tr>
<td>(\gamma_6)</td>
<td>0.946</td>
<td>0.924</td>
<td></td>
<td></td>
<td>0.940</td>
</tr>
<tr>
<td>(\gamma_7)</td>
<td>0.945</td>
<td>0.920</td>
<td></td>
<td></td>
<td>0.944</td>
</tr>
<tr>
<td>(\beta_0^*)</td>
<td>0.616</td>
<td>0.701</td>
<td></td>
<td></td>
<td>0.934</td>
</tr>
<tr>
<td>(\beta_1^*)</td>
<td>0.605</td>
<td>0.606</td>
<td></td>
<td></td>
<td>0.929</td>
</tr>
<tr>
<td>(\beta_2^*)</td>
<td>0.620</td>
<td>0.601</td>
<td></td>
<td></td>
<td>0.932</td>
</tr>
<tr>
<td>(\beta_3)</td>
<td>0.948</td>
<td>0.932</td>
<td></td>
<td></td>
<td>0.937</td>
</tr>
<tr>
<td>(\beta_4)</td>
<td>0.944</td>
<td>0.921</td>
<td></td>
<td></td>
<td>0.932</td>
</tr>
<tr>
<td>(\beta_5)</td>
<td>0.950</td>
<td>0.931</td>
<td></td>
<td></td>
<td>0.939</td>
</tr>
</tbody>
</table>

Table 4.3: The Percentage of 95% CIs Containing the True Value
Note: the numbers in bold indicate the best coverage for each parameter.
Table 4.4: Average CI Widths
Note: the shaded boxes indicate the narrowest CI for each parameter. If the estimates are equally unbiased, we can prefer narrower CIs. At the same time, the estimates of the probit models and the two-stage Bayesian selection model are more biased than those of the proposed model, so I argue that the proposed model can be better in terms of achieving unbiasedness.

<table>
<thead>
<tr>
<th>Coefficients</th>
<th>Bayesian MNP</th>
<th>Probit (U)</th>
<th>Probit (Y)</th>
<th>Bayesian selection</th>
<th>Proposed</th>
</tr>
</thead>
<tbody>
<tr>
<td>( \zeta_{10} )</td>
<td>0.362</td>
<td></td>
<td></td>
<td></td>
<td>0.496</td>
</tr>
<tr>
<td>( \zeta_{11} )</td>
<td>0.364</td>
<td></td>
<td></td>
<td></td>
<td>0.708</td>
</tr>
<tr>
<td>( \zeta_{12} )</td>
<td>0.312</td>
<td></td>
<td></td>
<td></td>
<td>0.706</td>
</tr>
<tr>
<td>( \zeta_{13} )</td>
<td>0.608</td>
<td></td>
<td></td>
<td></td>
<td>0.291</td>
</tr>
<tr>
<td>( \zeta_{14} )</td>
<td>0.309</td>
<td></td>
<td></td>
<td></td>
<td>0.290</td>
</tr>
<tr>
<td>( \zeta_{20} )</td>
<td>0.606</td>
<td></td>
<td></td>
<td></td>
<td>0.293</td>
</tr>
<tr>
<td>( \zeta_{21} )</td>
<td>0.312</td>
<td></td>
<td></td>
<td></td>
<td>0.386</td>
</tr>
<tr>
<td>( \zeta_{22} )</td>
<td>0.606</td>
<td></td>
<td></td>
<td></td>
<td>0.556</td>
</tr>
<tr>
<td>( \zeta_{23} )</td>
<td>0.313</td>
<td></td>
<td></td>
<td></td>
<td>0.554</td>
</tr>
<tr>
<td>( \zeta_{24} )</td>
<td>0.616</td>
<td></td>
<td></td>
<td></td>
<td>0.221</td>
</tr>
<tr>
<td>( \gamma_{0}^* )</td>
<td>0.305</td>
<td>0.432</td>
<td></td>
<td></td>
<td>0.222</td>
</tr>
<tr>
<td>( \gamma_{1}^* )</td>
<td>0.435</td>
<td>0.561</td>
<td></td>
<td></td>
<td>0.224</td>
</tr>
<tr>
<td>( \gamma_{2}^* )</td>
<td>0.431</td>
<td>0.565</td>
<td></td>
<td></td>
<td>0.223</td>
</tr>
<tr>
<td>( \gamma_{3} )</td>
<td>0.228</td>
<td>0.292</td>
<td></td>
<td></td>
<td>0.221</td>
</tr>
<tr>
<td>( \gamma_{4} )</td>
<td>0.229</td>
<td>0.291</td>
<td>0.348</td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \gamma_{5} )</td>
<td>0.231</td>
<td>0.295</td>
<td>0.287</td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \gamma_{6} )</td>
<td>0.230</td>
<td>0.312</td>
<td>0.285</td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \gamma_{7} )</td>
<td>0.228</td>
<td>0.437</td>
<td>0.287</td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \beta_{0}^* )</td>
<td>0.388</td>
<td>0.440</td>
<td>0.287</td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \beta_{1}^* )</td>
<td>0.568</td>
<td>0.226</td>
<td>0.332</td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \beta_{2}^* )</td>
<td>0.572</td>
<td>0.228</td>
<td>0.385</td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \beta_{3} )</td>
<td>0.304</td>
<td>0.228</td>
<td>0.381</td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \beta_{4} )</td>
<td>0.303</td>
<td>0.227</td>
<td>0.384</td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \beta_{5} )</td>
<td>0.306</td>
<td>0.227</td>
<td>0.384</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
methodologically, which I will examine in subsequent future studies.

Figure 4.2: Example of Upward-Bias Cases in Simulation
Table 4.5: Determinants of Alliance Choices and the Effects of Alliance Choices on Relevant MIDs (N = 11,793)

<table>
<thead>
<tr>
<th></th>
<th>Defense pact</th>
<th>Consultation pact</th>
<th>Offense/Neutrality pact</th>
<th>MID initiation</th>
<th>MID escalation</th>
</tr>
</thead>
<tbody>
<tr>
<td>Defense pact</td>
<td>-0.484</td>
<td></td>
<td></td>
<td>-0.086</td>
<td></td>
</tr>
<tr>
<td>Consultation pact</td>
<td>-0.449</td>
<td></td>
<td></td>
<td>-0.193</td>
<td></td>
</tr>
<tr>
<td>Offense/Neutrality pact</td>
<td>-0.249</td>
<td></td>
<td></td>
<td>0.203</td>
<td></td>
</tr>
<tr>
<td>National capabilities</td>
<td>[0.34, 1.93]</td>
<td>[1.41, 3.71]</td>
<td>[1.08, 2.35]</td>
<td>2.542</td>
<td>0.431</td>
</tr>
<tr>
<td>Level of external threat</td>
<td>[0.99, 1.30]</td>
<td>[-0.39, 0.28]</td>
<td>[0.58, 0.96]</td>
<td>[1.87, 3.22]</td>
<td>[-0.94, 1.74]</td>
</tr>
<tr>
<td>Contiguity</td>
<td>0.145</td>
<td>0.048</td>
<td>0.123</td>
<td>0.060</td>
<td>0.016</td>
</tr>
<tr>
<td>Democracy</td>
<td>[0.13, 0.16]</td>
<td>[0.02, 0.08]</td>
<td>[0.09, 0.14]</td>
<td>[0.04, 0.08]</td>
<td>[-0.02, 0.05]</td>
</tr>
<tr>
<td>Territorial claim target</td>
<td>[-0.05, 0.07]</td>
<td>[-0.09, 0.03]</td>
<td>[0.09, 0.20]</td>
<td>[0.20, 0.32]</td>
<td>[0.03, 0.31]</td>
</tr>
<tr>
<td>Territorial claim challenger</td>
<td>[-0.16, 0.06]</td>
<td>[-0.16, -0.01]</td>
<td>[-0.13, -0.04]</td>
<td>[0.33, 0.46]</td>
<td>[0.03, 0.36]</td>
</tr>
<tr>
<td>Ongoing war</td>
<td>0.091</td>
<td>-0.133</td>
<td>0.069</td>
<td>0.252</td>
<td>-0.151</td>
</tr>
<tr>
<td>Cold War</td>
<td>0.705</td>
<td>0.364</td>
<td>0.387</td>
<td>0.333</td>
<td>0.290</td>
</tr>
<tr>
<td>Constant</td>
<td>-1.236</td>
<td>-1.027</td>
<td>-1.054</td>
<td>-1.780</td>
<td>-0.630</td>
</tr>
</tbody>
</table>

Note: The coefficient is the median value of the 30,000 chains, while the values inside the bracket indicate 2.5 and 97.5 percentiles of the posterior densities.

Table 4.6: Correlation Estimates Among Alliance Choices and Relevant MIDs

<table>
<thead>
<tr>
<th></th>
<th>Defense pacts</th>
<th>Consultation pacts</th>
<th>Offense/Neutrality</th>
</tr>
</thead>
<tbody>
<tr>
<td>Being targets of MIDs</td>
<td>0.241</td>
<td>0.220</td>
<td>0.248</td>
</tr>
<tr>
<td></td>
<td>[0.01, 0.42]</td>
<td>[-0.06, 0.46]</td>
<td>[0.003, 0.44]</td>
</tr>
<tr>
<td>Resisting when challenged</td>
<td>-0.108</td>
<td>-0.006</td>
<td>-0.136</td>
</tr>
<tr>
<td></td>
<td>[-0.53, 0.31]</td>
<td>[-0.49, 0.59]</td>
<td>[-0.55, 0.30]</td>
</tr>
</tbody>
</table>

Note: The coefficient is the median value of the 30,000 iterations, while the values inside the bracket indicate 2.5 and 97.5 percentiles of the posterior densities.
Table 4.7: Comparing the Main Results to Those of the Existing Models

<table>
<thead>
<tr>
<th></th>
<th>Separate Probit</th>
<th>Heckman Selection</th>
<th>Proposed Model</th>
</tr>
</thead>
<tbody>
<tr>
<td>deterrent effect of</td>
<td>-0.043</td>
<td>-0.043</td>
<td>-0.484</td>
</tr>
<tr>
<td>Defense Pacts</td>
<td>(0.035)</td>
<td>(0.035)</td>
<td>[-0.83, -0.05]</td>
</tr>
<tr>
<td>deterrent effect of</td>
<td>-0.048</td>
<td>-0.048</td>
<td>-0.449</td>
</tr>
<tr>
<td>Consultation Pacts</td>
<td>(0.095)</td>
<td>(0.095)</td>
<td>[-0.98, 0.25]</td>
</tr>
<tr>
<td>deterrent effect of</td>
<td>0.104</td>
<td>0.104</td>
<td>-0.249</td>
</tr>
<tr>
<td>Offense/Neutrality</td>
<td>(0.055)</td>
<td>(0.055)</td>
<td>[-0.57, 0.15]</td>
</tr>
<tr>
<td>Escalation Effect of</td>
<td>-0.262</td>
<td>-0.256</td>
<td>-0.086</td>
</tr>
<tr>
<td>Defense Pacts</td>
<td>(0.073)</td>
<td>(0.074)</td>
<td>[-0.83, 0.75]</td>
</tr>
<tr>
<td>Escalation Effect of</td>
<td>-0.186</td>
<td>-0.193</td>
<td>-0.193</td>
</tr>
<tr>
<td>Consultation Pacts</td>
<td>(0.201)</td>
<td>(0.194)</td>
<td>[-1.49, 0.94]</td>
</tr>
<tr>
<td>Escalation Effect of</td>
<td>-0.071</td>
<td>-0.040</td>
<td>0.203</td>
</tr>
<tr>
<td>Offense/Neutrality</td>
<td>(0.110)</td>
<td>(0.115)</td>
<td>[-0.54, 0.81]</td>
</tr>
</tbody>
</table>

Note: The coefficient of the proposed model is the median value of the 30,000 iterations, while the values inside the bracket indicate 2.5 and 97.5 percentiles of the posterior densities. The parentheses in the first two columns show the standard errors of the estimates.

Figure 4.3: Substantive Effect of Each Alliance Type on MID Deterrence Compared to Non-alliance Category
Table 4.8: Determinants of Alliance Choices and the Effects of Alliance Choices on All MIDs ($N = 11,793$)

<table>
<thead>
<tr>
<th></th>
<th>Defense pact</th>
<th>Consultation pact</th>
<th>Offense/Neutrality</th>
<th>MID initiation</th>
<th>MID escalation</th>
</tr>
</thead>
<tbody>
<tr>
<td>Defense pact</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.238</td>
</tr>
<tr>
<td>Consultation pact</td>
<td>-0.420</td>
<td></td>
<td></td>
<td></td>
<td>0.038</td>
</tr>
<tr>
<td>Offense/Neutrality</td>
<td>-0.206</td>
<td></td>
<td></td>
<td></td>
<td>0.366</td>
</tr>
<tr>
<td>National</td>
<td>1.528</td>
<td>2.223</td>
<td>1.972</td>
<td>2.447</td>
<td>0.316</td>
</tr>
<tr>
<td>capabilities</td>
<td>[0.82, 2.21]</td>
<td>[1.53, 2.90]</td>
<td>[1.37, 2.56]</td>
<td>[1.77, 3.11]</td>
<td>[-0.98, 1.60]</td>
</tr>
<tr>
<td>Level of external threat</td>
<td>1.102</td>
<td>0.330</td>
<td>0.806</td>
<td>0.458</td>
<td>0.385</td>
</tr>
<tr>
<td>Contiguity</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Democracy</td>
<td>0.226</td>
<td>0.104</td>
<td>0.124</td>
<td>0.192</td>
<td></td>
</tr>
<tr>
<td>Territorial claim target</td>
<td>-0.110</td>
<td>-0.082</td>
<td>-0.088</td>
<td>0.384</td>
<td>0.215</td>
</tr>
<tr>
<td>Territorial claim challenger</td>
<td>[-0.16, -0.06]</td>
<td>[-0.14, -0.03]</td>
<td>[-0.13, -0.04]</td>
<td>[-0.32, 0.45]</td>
<td>[0.06, 0.37]</td>
</tr>
<tr>
<td>Ongoing war</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Cold War</td>
<td>0.702</td>
<td>0.430</td>
<td>0.439</td>
<td>0.289</td>
<td>0.189</td>
</tr>
<tr>
<td>Constant</td>
<td>-1.198</td>
<td>-0.888</td>
<td>-1.056</td>
<td>-1.751</td>
<td>-0.654</td>
</tr>
</tbody>
</table>

Note: The coefficient is the median value of the 30,000 chains, while the values inside the bracket indicate 2.5 and 97.5 percentiles of the posterior densities.

Table 4.9: Correlation Estimates Among Alliance Choices and All MIDs

<table>
<thead>
<tr>
<th></th>
<th>Defense pacts</th>
<th>Consultation pacts</th>
<th>Offense/Neutrality</th>
</tr>
</thead>
<tbody>
<tr>
<td>Being targets of MIDs</td>
<td>0.268</td>
<td>0.273</td>
<td>0.266</td>
</tr>
<tr>
<td></td>
<td>[0.07, 0.45]</td>
<td>[0.01, 0.45]</td>
<td>[0.06, 0.45]</td>
</tr>
<tr>
<td>Resisting when challenged</td>
<td>-0.253</td>
<td>-0.201</td>
<td>-0.273</td>
</tr>
<tr>
<td></td>
<td>[-0.57, 0.18]</td>
<td>[-0.55, 0.23]</td>
<td>[-0.59, 0.17]</td>
</tr>
</tbody>
</table>

Note: The coefficient is the median value of the 30,000 iterations, while the values inside the bracket indicate 2.5 and 97.5 percentiles of the posterior densities.
Figure 4.4: Substantive Effect of Each Alliance Type on MID Escalation Compared to Non-alliance Category
Figure 4.5: Posterior Densities of Coefficients: MID Deterrence

Figure 4.6: Posterior Densities of Coefficients: MID Escalation
Figure 4.7: Substantive Effects of Alliance Choices on MID Initiation, Pairwise Comparisons of Alliance Choices
Figure 4.8: Substantive Effects of Alliance Choices on MID Escalation, Pairwise Comparisons of Alliance Choices
Chapter 5

Conclusion

This dissertation was originally motivated by a puzzle involving Fearon’s seminal piece on signaling models (Fearon 1997). According to his formal model, making a partial or half-hearted commitment does not provide any benefit to the signatories; rather, making a partial commitment could be an adverse signal to potential challengers (if a state is sincerely committed to protecting the other, the state would have provided a full commitment). On the other hand, in the alliance literature, consultation pacts – often considered as partial commitments – are prevalent in the real world. Therefore, the original main research question of this project was “why or under what conditions do states form consultation pacts?” Despite its prevalence, consultation pacts have been hardly studied in the previous literature, partly due to the skepticism about the effects of a consultation pact (i.e., a treaty being a “piece of paper” and a consultation pact being an even weaker type of treaty).

At the same time, how can we know whether consultation pacts affect states’ foreign policy decisions if no one has tested their effects? Therefore, I thought that before moving to examining the factors of forming a consultation pact, I should examine whether a consultation pact has any impact on foreign policy decisions first. Then, I encountered the two methodological challenges (endogenous treatment and sample selection bias) while I was working on Chapter 2. Addressing the methodological challenges was important to me because the non-finding about the deterrence effect of a consultation pact in Chapter 2 could be attributed to either (or both) of the followings: 1) there was no such puzzle but I observed the discrepancy between theoretical expectations and the empirics simply because I was not adequately addressing the methodological concerns (i.e., revise my research design), or 2) the puzzle persists even after accounting for the fact that states do not form alliances randomly.
and make subsequent decisions deliberately (i.e., develop a theoretical argument to fill in the gap). With the advanced methods training I was able to have at Rice, I decided to tackle the methodological issues, and the effort ultimately became my entire dissertation project, leaving the question about the factors of forming a consultation pact unanswered.

As described earlier, each main chapter approaches the different sets of the overall research agenda, while Chapter 4 can clearly address more issues than either Chapter 2 or Chapter 3. What have I learned from these different (but evolving) approaches? One consistent finding throughout the three main chapters is the deterrent effect of a defense pact. If a state has a relevant defensive ally, the state is less likely to become a target of MIDs. This finding holds even after addressing the fact that military alliances are not formed randomly. In fact, because the unobservable confounders that increase the prospect of being attacked also encourage signing a defense pact, the deterrence effect of a defense pact could be greater than we have found in previous research. This also suggests that the debates on the deterrent effect of a defense pact could be due to these unobservable confounders. I believe this is an important contribution to the literature: Going back to the above paragraph, I show that addressing the methodological concerns does provide us with a different empirical finding. Furthermore, in Chapter 3 and Chapter 4, I find that states who see higher prospect of being attacked are more likely to sign a defense pact (as well as other types of alliances: Chapter 4). This result conforms to the theoretical arguments in the IR literature, but the arguments have not been empirically examined until very recently. Along with the other recent works that address the endogeneity issues, this paper contributes to the literature by revisiting our understanding about the effects of alliance choices.

On the other hand, I have found mixed (or unclear) evidence regarding the effects of the other types of alliances. The mixed finding can be partly attributed to the relatively unestablished theoretical argument (i.e., it is not very clear how a potential target’s offense/neutrality pact will impact potential challenger’s decision to initiate a conflict) or the limitations in the data set (consultation pacts are coded conservatively – only the alliances
without defense, offense, or neutrality obligation are coded as consultation pacts). Furthermore, there are other limitations in this research, such as the absence of an identification variable for the first stage and the unit of analysis being a monadic state-year. At the same time, this study still adds another result to the literature, addressing the methodological issues that have not been rigorously examined in the previous research. Given that I have attenuated the methodological concerns and have found that the effects of a consultation pact are quite uncertain, there is a good chance that we still have the puzzle in hand – why do states form consultation pacts, while neither theoretical arguments nor my empirical findings explain the reasons for them to exist?

Finally, this dissertation project can have a couple of policy implications regarding alliance policies. In recent years, public as well as key policymakers’ attitudes toward defensive military alliances, particularly NATO, have become more negative in the U.S. One of the concerns is about the effectiveness of alliances: Do NATO and other alliances deter the potential challengers of the U.S. and its allies? How can we examine whether these alliances are working as designed? Would admitting a new member to NATO reduce its chance of being involved in interstate conflicts? What are the consequences of “watering down” the current defensive commitment of these alliances? Although this dissertation may not provide a direct answer to each specific case, I believe it has several implications drawn from the comprehensive examination of cases in history.

First, acknowledging that the deterrent effect of defensive alliances is the most consistent finding in this dissertation, I argue that a member of defensive alliances has lower chance of being attacked than the other non-member countries, all else being equal. Furthermore, this dissertation points out one of the reasons why it has been difficult for scholars to examine the effectiveness of military alliance choices: many of the previous studies might have overlooked (or have not incorporated the issue rigorously into their research) the fact that military alliance choices are closely tied to the prospect of interstate disputes. As demonstrated in the empirical analyses in Chapter 3 and 4 as well as in the simulations in Chapter 4, the
deterrent effect of defensive alliances was detectable only when we explicitly address the endogeneity in the research design.

Therefore, although not perfect, one empirical strategy to examine the effectiveness of alliance choices is to understand this potential sources of bias and develop an empirical strategy to address the issue. In other words, fully successful military alliances will never have an opportunity to be put into test (except offensive alliances), because they are capable of deterring every potential challenger. To simply put, we cannot distinguish a defensive alliance that works perfectly when facing potential challengers from another defensive alliance that is not functioning at all and has no potential challengers. Since the establishment of NATO, people have raised questions about its effect on preventing interstate disputes targeting its member states. My dissertation points out that the fact that it has been invoked only once throughout its history does not suggest that the alliance has not been working. Rather, it could be the case where NATO has been preventing many potential challengers from initiating disputes with its member states.

Second, along with the endogenous treatment, another source of bias addressed in this dissertation is non-random sample selection. In the main empirical results in Chapter 4, I do not find evidence of the conventional idea that the deterrent effect of a defensive alliance is offset by the protege’s willingness to escalate disputes when attacked. Rather, it is more likely for the proteges not to escalate disputes when attacked, and the results are consistent across alliance choices. Policymakers who are concerned about the traditional idea of entrapment-entanglement tradeoff can instead carefully design and limit the scopes of alliance obligations if necessary. In other words, there is no clear empirical evidence that forming or signing a military alliance should be hindered by such concerns.

Third, my results regarding different effects of alliance choices on interstate dispute initiation and escalation suggest that “watering down” defensive alliance commitments or making a public statement that harms the credibility of alliance commitments adds more uncertainty regarding the effect of the alliance commitment or weakens the effect. In particular, com-
paring the effect of an defensive alliance to that of a consultation pact, the estimated effect of a consultation pact varies more than that of a defense pact. Therefore, reducing the level of defensive commitment (i.e., not declaring that allies will aid when attacked) makes policymakers’ assessment of foreign policy more difficult. Furthermore, regardless of the level of an alliance commitment itself, making an alliance less credible (i.e., declaring that allies may not fulfill what they have promised) can undermine the overall effect of the alliance commitment. Although not empirically examined in this dissertation, the entire dissertation is built upon the idea that alliances are institutions that work as signals by incurring costs, and thus convey the allies’ foreign policy preferences to others credibly. If an alliance is not credible, regardless of its type of commitment, would have no impact on any foreign policy outcome. However, we find that it is not the case, and different alliance choices have varying degrees and signs of effects.

Finally, I would like to note that my methodological approach is one of many ways to address the potential sources of bias when examining the effects of international institutions. A case study that chooses cases carefully to control for the prospect of dispute, not only matching the cases based on the observables, can be another solution. Compared to the qualitative approach, a useful aspect of my approach is that it enables us to conduct quantitative studies with many observations, and works as a fool-proof tool as long as it is properly identified (which is another limitation of the empirical part of this dissertation). In the absence of endogeneity or sample selection, scholars will get the same results as the ones they would get with other parsimonious models. On the other hand, if either or both of these issues exist(s), the model proposed in this dissertation will be able to incorporate them. Therefore, I believe the model has a great potential to be utilized for other empirical research examining the effects of international institutions. In sum, this project contributes to the IR literature by re-examining the effects of defensive alliances and expanding the scope of studies by comparing the effects of alliances by their commitments. Furthermore, it also contributes to the literature in political methodology by introducing a novel statistical esti-
mator that is motivated by a particular research question and is better at properly addressing the potential biases regarding the issue. It has also raised several follow-up questions and research agendas in both subfields, which could be examined further in the future.
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