

Hegemonic Threats and Great Power Balancing in Europe, 1495-1999

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ABSTRACT

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The balance of power is a venerable concept in international relations theory, but it is plagued by ambiguities about what the concept means and what the theory purports to explain, and the key proposition that states balance against concentrations of power or hegemonic threats is rarely if ever subjected to systematic empirical test. We argue that despite these ambiguities and disagreements, there is one proposition that nearly all balance of power theorists and their critics as well would accept as an accurate reflection of the theory and that provides the basis for a "most-likely" test of the theory: great powers have balanced against extreme concentrations of land-based military power in Europe, concentrations that have created the potential for hegemony over the continent.

We develop and test several hypotheses linking military concentration, capability changes, and alliance responses for the European system from 1495-1999 using the Rasler and Thompson (1994) data on army concentrations and developing a new data base of great power alliances for the last five centuries. We find that European great powers have demonstrated a strong propensity to balance against the leading state when one state acquired a third or more of the total military capabilities in the system. Lower concentrations of power have been less likely to generate balancing coalitions. Not all available great powers have joined these balancing coalitions, however, so that the size of the typical balancing coalition is smaller than balance of power theory might lead us to believe.

While our overall findings provide considerable support for hypotheses about balancing against high concentrations of military power in Europe, we emphasize that this is the "most likely" case for the balancing proposition, given the longstanding great power and Eurocentric biases of the balance of power tradition in international relations theory. Consequently, our findings cannot necessarily be generalized to other systems. We are particularly skeptical as to the validity of our findings for international systems dominated by maritime or global powers.

HEGEMONIC THREATS AND BALANCING IN EUROPE, 1495-1999

The balance of power is one of the oldest and most fundamental concepts in the study of international relations. Hume regarded the balance of power as a scientific law, and Waltz argued that "if there is any distinctively political theory of international politics, balance of power is it." Although there are many variations of balance of power theory, and although there is considerable disagreement about the meaning of its key concepts and even about what the theory purports to explain (Haas, 1953; Claude, 1962), the central proposition of nearly all balance of power theories is that states tend to balance against concentrations of power or hegemonic threats. Indeed, this is one of the most widely-held propositions in the international relations field. Even this proposition is quite ambiguous, however, and it is rarely if ever subjected to systematic empirical test. Our aim in this paper is to formulate a testable version of the balancing proposition and examine its empirical accuracy during the last five centuries.

The ambiguity of the key concepts of balance of power theory and the widespread support for its core proposition may be related -- it is precisely because of the ambiguity of the theory's key concepts that so much behavior has been interpreted as consistent with some version of the balancing proposition. Any given behavior can be interpreted as somebody balancing against some kind of power or some kind of threat by somebody, but scholars fail to specify who balances against whom, in response to concentrations of what kinds of power or what kinds of threats, and in what kinds of systems. Moreover, they treat balancing as a dichotomous category, assume that states either balance or not, fail to acknowledge intermediate forms of balancing, and generally fail to operationalize the concept in ways that might enable us to identify balancing when we see it. This makes it very difficult to construct an empirical test of balancing propositions that might be accepted by both its proponents and its critics.¹

There have been some efforts to refine balance of power theory and the balancing proposition in particular, as well as some attempts, even by realists, to explain why states might not balance. In a modification of the long-held notion that states balance against the strongest power in the system, which remained a key proposition in Waltz's (1979) reformulation of neorealist theory, Walt (1987) argued that states balance instead against the greatest threats to their interests, with threats defined as some combination of perceived intentions, ideology, and distance as well as aggregate capabilities. Scholars also undertook empirical studies of particular cases to see if states balanced against threats (or power) or bandwagoned with them (Labs 1992; Kaufman 1992; Walt 1992). Schroeder (1994) broadened the categories and demonstrated that states often bandwagon, hide, or "transcend" rather than balance (see also Arquilla, 1992), and Schweller (1994) argued that while status quo states balance to preserve their security, revisionist states often bandwagon with the strong in order to secure economic gains and otherwise expand their influence.² Others argued that some behavior often associated with balancing is actually something else: states often fight powerful aggressors not because they balance against these aggressors but rather because they are directly attacked (Schroeder, 1994; Rosecrance and Lo, 1996;

Vasquez, 1997).

These new perspectives on balancing have been supported with historical illustrations from the last four centuries and with more detailed historical case studies. While some of these individual studies are convincing, either in support or criticism of the balancing proposition, they do not constitute, individually or collectively, anything approaching a systematic empirical test of the proposition that states systematically balance against hegemonic threats. Moreover, while these studies have advanced our understanding of some of the issues involved in the analysis of balancing, there are a number of extremely serious conceptual and methodological issues that remain undeveloped or unrecognized and that limit our confidence in the empirical validity of existing studies (Levy, 2002b).

We begin with the premise that the question of whether or not states engage in balancing behavior, or the conditions under which they do so, cannot be resolved unless scholars can find a way to shift the debate from the purely theoretical level, move beyond illuminating but contradictory historical examples, and begin to incorporate more systematic empirical evidence about balancing behavior.³ This requires that we first eliminate many of the conceptual ambiguities surrounding balancing, specify the balancing proposition more precisely so that it yields more easily testable propositions, identify any scope conditions that limit the universality of the proposition, and deal with the analytic problems that create potential threats to valid inference.

Our aim in this study is to provide a preliminary test of the proposition that states balance against concentrations of power or hegemonic threats. We believe that any balancing proposition stated in universal form is almost certainly false or non-falsifiable, and we begin by formulating a more restricted balancing proposition and the scope conditions under which we expect it is most likely to be valid. In particular, any theory of balancing must begin by specifying the geographical scope of the system over which hegemony is threatened and within which balancing takes place, the basis of power in that system, and the identity of the actors that engage in balancing.

Our general proposition, which we believe that most balance of power theorists and most of their critics would agree constitutes an accurate representation of the theory, is that in any autonomous continental system, the leading powers in the system tend to balance against any state that threatens to dominate the system by amassing a disproportionate concentration of military power. We attempt to test this proposition for the modern European system for the last five centuries, based in part on our view of the implicit great power and Eurocentric biases in most formulations of balance of power theory. We consider some of the analytic problems raised by our proposition, construct a research design to deal with those potential threats to valid inference, and describe the operational indicators we use for our key theoretical concepts. We then present our results, discuss their limitations, and suggest possible lines for future research.

We are not claiming that balancing occurs only in Europe, but instead that the modern European system is the only place where all balance of power theorists agree that great powers systematically

balanced against hegemonic threats. Most proponents of balance of power theory argue that balancing occurs in other systems as well, but they disagree on precisely where else it occurs and how regularly. In this sense, our analysis provides a minimal test for the balancing proposition. This is the best case for balance of power theory, and thus constitutes a most-likely test for the balancing proposition. If the proposition does not apply in modern Europe, it is not clear where it would apply, so that negative findings would raise serious questions about the general validity of the proposition. Confirmation of the balancing hypothesis for the European system, while informative about that system, would not allow us to generalize to other systems in the absence of comparative historical research.⁴ One important implication of our argument is that we would not necessarily expect great powers, much less states in general, to balance against leading maritime powers (Levy 2002b).⁵

HYPOTHESES ON BALANCING

Despite the many variations of balance of power theory, its proponents agree on many things. They share the fundamental realist assumptions that the key actors in the system are states (or other territorially-based actors) who wish to survive and who act rationally to maximize power or security under constraints in an anarchic international system, and they accept the key realist proposition that the distribution of power is the primary determinant of international outcomes. They also agree that extreme concentrations of power rarely if ever form in anarchic multi-state systems, and that the avoidance of such concentrations is a primary instrumental security goal of states. Balance of power theorists may disagree about the level of concentration of power or degree of threat that leads to balancing, but they agree that concentrations of power that would put one state in a position to dominate over the rest are sufficient to trigger blocking coalitions. Both balance of power theory and balance of threat theory converge on this proposition, because if a state is strong enough to threaten hegemony it will usually be the greatest single threat to the interests of any other great power.

There are two causal paths leading to the absence of hegemony in balance of power theory, two reasons why the balancing mechanism almost always works successfully to avoid hegemony: (1) potential hegemons anticipate that expansionist behavior or military buildups will lead to the formation of a military coalition against them, and consequently refrain from such behavior; or (2) potential hegemons pursue expansionist policies and are defeated in war by a blocking coalition.⁶ The first results in peace, but the second does not, so that the outbreak of war, even major war or frequent wars, cannot necessarily be taken as disconfirming evidence for balance of power theory or the balancing hypothesis. Balancing hypotheses predict balanced outcomes (non-hegemony) and balancing strategies, but not necessarily peace (Levy 2002b).⁷

There is an important exception to this characterization of the balancing proposition to include both balanced outcomes and balancing strategies. Waltz (1979) argues that his neorealist theory predicts only outcomes, not state strategies or foreign policies. He predicts that balances of power always occur, but leaves open the question of *how* they occur. For Waltz, outcomes of balanced

power do not necessarily require deliberate balancing behavior by states. Most balance of power theorists focus on balancing strategies as well as balanced outcomes, however, and it is incumbent on those who do not to specify the alternative causal mechanisms through which balances arise and persist.⁸

While balanced outcomes and balancing strategies are analytically distinct, and while there are in principle different causal paths to balance besides balancing (Elman 2002), our view is that the balancing mechanism is sufficiently plausible that it is at least incumbent upon balance of power theorists who reject it to specify the alternative causal paths leading to balanced outcomes. In addition, a theory which predicts only that balances will form has far less empirical content than a theory that also identifies the causal mechanisms that lead to this outcome. *Ceteris paribus*, theories that explain both balanced outcomes and balancing strategies are superior to theories of balance alone (Levy, 2002b).

The stated propositions that hegemonies do not form and that threats of hegemony lead to balancing behavior are universal in form, and in fact most theories of the balance of power and balancing are stated in such unconditional terms. We have few propositions in international relations that are universally valid, however. Most are delimited by certain scope conditions, and balance of power theory is no exception. The unconditional hypothesis that hegemonic concentrations of power do not form in multistate systems is contradicted by Britain's economic and naval dominance in the 19th century and by American economic and military dominance after the end of the Cold War (Russett, 1985; Brooks and Wohlforth, 2000/01). In continental systems, it is contradicted by the transformation of Chinese multi-state systems into hegemonies under Qin and Han (Hui, forthcoming) and by the emergence of hegemony in ancient Assyria (Wohlforth and Kaufman, 2003). The unconditional propositions that states balance against dominant states is contradicted by the absence of significant balancing in the British and American cases, and also by the countless situations in which small states do not balance against hegemonic neighbors, including Napoleonic France in the early 19th century and, after World War II, both the United States in the Western Hemisphere and the Soviet Union in Eastern Europe.

Balancing propositions are plausible only under more restrictive conditions. It is particularly important, in our view, to specify the system over which hegemony is threatened, the basis of power in that system, and the identity of the states hypothesized to engage in balancing behavior. It makes a difference, for example, whether we are talking about a European system of great powers with large armies, or a world system of global powers defined by their economic power and naval strength (Levy, 1985; Thompson, 1988; Rasler and Thompson, 1994).

In one sense the failure of balance of power theorists to be explicit about these parameters is a significant omission and source of underspecification of their theories. In another sense, however, those parameters are implicit in much of the balance of power theorizing in the West for the last several centuries, as reflected in the work of Morgenthau (1967), Gulick (1955), Claude (1962), Dehio (1962), Waltz (1979), Mearsheimer (2001), and others. Our argument is that, with few exceptions,

there is a strong great power, military, and Eurocentric bias in most of these formulations of balance of power theory and balancing hypotheses.

Most diplomatic historians have followed von Ranke (1833/1973) in treating European history as the history of great power politics, and this great power bias has pervaded most of the balance of power literature in the West, at least until the end of the Cold War (Waltz 1979, 72-73; Claude 1989, 78; Levy, 1983). In addition, most of the balance of power literature is written by Europeans (especially the British) and Americans (whose security outlook was primarily Europe-centered until the late 20th century), and is grounded in the European experience extending back to Westphalia (1648) and to the Italian city-state system of the late fifteenth century (Mattingly, 1964). Among the manifestations of the Eurocentric bias is the concept of a "balancer" or "holder of the balance," which, while generalizable in principle, is nearly always equated with Britain's role in maintaining an equilibrium of power on the European continent; the principles of territorial compensation and partition, which serve the interests of the great powers at the expense of weaker states; and the idea that an open "colonial frontier" was stabilizing because it provided an area into which European great powers could expand their power at the expense of the weak without directly threatening each others' vital interests (Morgenthau 1967, ch.14; Gulick 1955, ch.1; Hoffmann, 1968).⁹

Along with this implicit Eurocentric bias is a primary focus on land-based military power as the primary basis of military strength in the system. The concentrations of power to be feared, and that are hypothesized to precipitate balancing behavior, are those that most directly and immediately threaten the territorial integrity of other states. This threat is posed far more by large armies that can invade and occupy than by the concentration of naval strength or economic wealth by themselves. Thus balance of power theorists talk about balancing coalitions against the Habsburgs under Charles V in the early 16th century, Philip II at the end of the 16th century, and the combined strength of Spain and the Holy Roman Empire in the Thirty Years War; against France under Louis XIV and then Napoleon; and against Germany under Wilhelm and then Hitler (Gulick 1955; Dehio 1962; Claude 1962; Morgenthau 1967; Aron 1973; Kennedy 1987). It is revealing that even Waltz (1997, 914), who speaks in more universal terms about balances in the international system as a whole, illustrates his arguments with examples of balancing against Charles V, Louis XIV, Napoleon, Wilhelm II, and Hitler.¹⁰ There is no mention, in Waltz (1979) or in other balance of power literature, of balancing against Dutch economic primacy in the 17th century or Pax Britannica in the 19th century or Pax Americana in the 20th or 21st centuries. Those phenomena are central to leadership long cycle theory (Thompson, 1988) and to many formulations of power transition theory (Organski and Kugler, 1980; Gilpin, 1981), not to balance of power theory.¹¹ This longstanding focus on land-based military power is now quite explicit in Mearsheimer's (2001) offensive realist version of balance of power theory.

In summary, we have identified two general propositions common to almost all balance of power theories: hegemonies do not form in multi-state systems, and the threat by any state to gain a dominant position will lead other states to balance against it. We have argued, however, that these two propositions are not universally valid but are instead subject to certain scope conditions: they apply to

autonomous continental systems and to the great powers within those systems, where power and hegemony are defined in terms of land-based military power. We know that sustained hegemonies have not formed in the European system during the last five centuries, so we focus on several variations of one key proposition about balancing behavior: high concentrations of military power that might permit any one state to dominate over the European system will lead other great powers to balance against it. We now specify some operational implications of this proposition and construct a research design to test them.

RESEARCH DESIGN

We noted above that theoretical arguments about balancing and bandwagoning have been backed by historical illustrations rather than more systematic empirical analyses. Much of the anecdotal evidence focuses on major wars and on the question of whether states balanced against the strongest and most threatening state or whether they bandwagoned with them.¹² The methodological problem here is not just that analyses are limited to a single case or small number of cases. Analyses of balancing in all past wars would not rectify the problem, because focusing only on wars and assessing the extent of balancing in those wars raises a serious endogeneity problem. It is also necessary to look at the wars that did not occur and ask whether the absence of war might be due to the anticipation of balancing by the potential aggressor. Balancing can have a causal impact even though it is “off the equilibrium path” and unobserved (Levy, 2002b).¹³

The failure to examine periods of nonwar as well as war can lead to potentially serious inferential biases. Presumably, potential aggressors are more likely to initiate war when they anticipate that potential adversaries will not balance, so if we look only at wars we will observe a fair amount of non-balancing and systematically underestimate the causal impact of balancing. This is a form of selecting on the dependent variable. That is particularly inappropriate given the if-then causal logic of the balancing proposition: if there are extreme concentrations of power, balancing will follow. This implies that extreme concentrations of power approximate a sufficient condition for balancing, and this makes it imperative to select cases based on the independent variable -- some measure of the concentration of power or hegemonic threat.

Hypotheses

The general proposition that great powers balance against hegemonic threats leads to a number of testable implications. While these hypotheses apply in principle to any continental system in which land-based military strength is the basis of power in the system, in this paper we limit our hypotheses to the European system, following the logic of most-likely case research design.

Our argument that balancing is not universal, that it is delimited by scope conditions, and that balancing occurs in response to hegemonic threats but not necessarily in response to lesser threats

implies that we would not expect to find strong patterns of balancing against *any* leading state in the system, only against those posing hegemonic threats based on their capabilities. Similarly, we would not expect to find strong patterns of balancing against increases in power by any leading state in the system, but only against those already much stronger than other states. This leads to our first two hypotheses:

H1: There is not a strong pattern of balancing against the strongest state in the system, independently of the magnitude of its advantage.

H2: There is not a strong pattern of balancing against a leading state that is increasing its power, independently of the magnitude of its advantage.

The greater the magnitude of the leading state's capability advantage, the greater the likelihood that other great powers will balance against it, and also the larger the size of counterbalancing coalitions. Hence

H3: The stronger the most powerful state's relative capability position, the more likely it is that other great powers will ally against the lead state.

H4: The stronger the most powerful state's relative capability position, the more likely it is that more than two major powers will resist the threatening state by allying against it.

H5: The combination of a state's very high concentration of military capabilities and a significant increase in those capabilities is particularly likely to generate counterbalancing alliances.

We have conceptualized balancing in terms of alliance formation in response to concentrations of military power. This has come to be known as "external balancing."¹⁴ Another possible response to concentrations of power is the enhancement of one's own military capabilities or the economic foundations of military potential, which is often referred to as "internal balancing" (Waltz, 1979). We regard internal balancing as a separate question that deserves equally separate treatment, although testing such a proposition is unlikely to be easy.¹⁵ One problem is how to distinguish arms buildups as balancing from arms buildups that result from bureaucratic politics and the vested interests of the military or from domestic pressures; or how to distinguish strategies to increase economic productivity and wealth in order to enhance military potential, from similar strategies intended to promote social welfare or private interest, or from the natural growth of the economy.

By restricting balancing to alliance formation in response to concentrations of power and by excluding other possible responses to external threats, we are constructing a rather conservative test of the balancing proposition, because behavior that some would classify as balancing we would not, at least for the purposes of this study.

Measurement

We begin the analysis in 1495, which many have taken as marking the origins of the modern European great power system, and we end the analysis in 1999. The European great powers (adopted from Levy 1983) include the Ottoman Empire (1495-1699), Spain (1495-1808), Austria (1495-1918),¹⁶ France (1495-), England/Britain (1495-), the Netherlands (1609-1713), Sweden (1617-1721), Russia (1721-), Prussia/Germany (1740-),¹⁷ and Italy (1861-1943). There are no major problems with this approach until 1945. We can identify European subsystems both before and after 1945, but the roles played by European actors have not been the same in the two systems. Prior to 1945, Europe, especially western Europe, had been the central region in the world system.¹⁸ By the end of World War II, this regional primacy had come to an end with the rise of the United States and the Soviet Union, and the decline in the relative status of Britain, France, and Germany. Yet the basic political problem that had plagued Europe since 1495 - how to prevent one actor from achieving hegemony over the whole region - persisted.¹⁹ As a consequence, we feel justified in continuing to ask the same question after 1945 as before - whether European states balance against potential hegemonic threats from the leading power on the continent.²⁰

Our unit of analysis is the individual great power and its alliance behavior (alliance/no alliance) in response to concentrations of power, aggregated by half decade.²¹ An alternative approach would be a system-level focus that asked whether or not an alliance occurs in a given half-decade period. One problem with this approach is that it does not control for the variation in the number of great powers from era to era. Presumably, the larger the number of great powers, *ceteris paribus*, the more likely some balancing coalition will form. A second problem is the failure of the system-level orientation to distinguish situations in which there is a single alliance against the dominant state from one in which there are several bilateral alliances. Presumably balancing is stronger in the second scenario than in the first. By focusing on individual great powers and their alliance behavior, we gain a more discriminating measure of balancing behavior.

The identification of the most powerful state, its relative capability position, and increases in strength all require the measurement of military capabilities. We measure hegemonic threats in terms of the degree of concentration of military capabilities. Ideally, it would be useful to have an indicator of all instances in which the leading European state had expansionist ambitions, or, more precisely, when it was perceived by others as posing such a threat. The measurement of these indicators during the last five centuries, for peacetime as well as wartime, would be a formidable task. Since our hypotheses focus on threats of hegemony over the European system, where balance of power theory and balance of threat theory converge, a measure of the degree of concentration of military power in the hands of a single state serves as an adequate measure of hegemonic threats.

The most widely-used indicators of military strength over the last half-millennium, by European political leaders as well as scholars, are population size, wealth, and armies (Morgenthau, 1967). We have longitudinal information on population and armies. Relative wealth has been calculated on occasion

but little in the way of serial information is available once we go back in time beyond the reach of gross national product calculations. Given the predominately agrarian nature of the early modern European economy, estimations of relative wealth before the Industrial Revolution would be fairly difficult.²² More important, states' balance of power calculations were usually driven by rather short-term time horizons, while economic strength mattered more in the long term than the short term, as reflected in the fact that some of the wealthiest states went bankrupt on a number of occasions with only temporary lapses in their military activity (Rasler and Thompson, 1989: 90-97). Thus the absence of indicators of wealth is not a serious limitation in the estimate of capabilities relating to balance of power considerations.²³

Population estimates are readily available for all of the major European states over the last 500 years. The main problem is that there is only limited variation in the relative population sizes of the European great powers, so this indicator cannot possibly tap the fairly substantial variation in relative power over time.²⁴ A related problem is that population indicators cannot account for the great power status of states with relatively small populations, such as the the United Provinces of the Netherlands, which successfully competed for substantial periods of time with states like Spain and France with populations many times larger? Population size mattered, but it was a secondary attribute of relative power.

That leaves us with armies. As states gradually weaned themselves from reliance on mercenaries (Howard, 1976), standing armies expanded in conjunction with increasing emphasis on infantry warfare and the pressures of European international relations (Thompson and Rasler, 1999). Unlike population, army size and its influence varies significantly across states. Smaller, poorer, and less ambitious states tend to have smaller armies than did larger, wealthier, and more ambitious states, and efforts to expand territorial control in Europe were usually accompanied by a buildup of army size. The most prominent European hegemonic aspirants usually created the largest armies of their times in order to fulfill their military and territorial conquest goals.²⁵ Thus relative army size is a useful and valid indicator of the distribution of power within continental Europe during the last five centuries.

Army size data, available in the form of 5-year averages, are taken from Rasler and Thompson (1994), with two modifications. First, we updated the data to include the last half-decade of the 20th century, using the same sources that were relied upon in the original data collection effort. Second, we have estimated an Ottoman army series for the 16th and 17th centuries that was missing from the Rasler-Thompson data and added it to the European pool.

Estimating Ottoman army size is problematic in several respects. For one thing, the literature offers little numerical information. Murphey (1999) provides a limited number of data points over 200 years, from which we extrapolated our series assuming, rather heroically, that there were no major changes in between points. The early modern Ottoman army was also quite heterogenous. It had a standing core that could be expanded quite impressively in terms of numbers by calling up cavalry levies from landed estates. However, these cavalry were less than fully disciplined and could only be used for a few months at a time when their services were not needed for harvesting. A third problem is that eastern Europe was only one of the theaters in which Ottoman troops fought. The Persian frontier and

the general Fertile Crescent area imposed its demands on Ottoman troop strength. For these reasons, we have limited our estimation to numbers referring to the Ottoman standing core army and have made no effort to count the potential cavalry auxiliaries that would come and go as circumstances dictated. Although this coding rule is conservative, it provides a more appropriate bias for estimating Ottoman army size in the European context than one that tapped the maximum size of the Ottoman army throughout Europe, southwest Asia, and North Africa.²⁶

In Figure 1 we graph the capability shares of each of the leading European great powers during the last five centuries. It is clear that for the most part the leading land power has been either France, Spain (or the United Habsburgs in the 1519-1556 period), Germany, or Russia during the past 500 years. For the purposes of this analysis, the leading land power in any five year period is the state with the largest army share of the European great power group. We see that for much of the time since the 1490s, one state had a rather significant capability advantage over its closest rival.

/Figure 1 about here/

One problem, implied by Figure 1, is that Russian army sizes were always relatively large, larger than their actual influence. The main reasons for this is that Russian armies, not unlike the earlier Ottomans, were the most distant from west European battlefields. As the Russian empire expanded throughout Eurasia, its need for garrison troops also expanded. In the 19th and early 20th centuries, the Russian army even expanded beyond the ability of the Russian state to provide it with arms and food. The Russian armies that fought for the most part in central Europe were usually no larger than those of their European rivals. They were also very slow to mobilize. Since it is the European system that is our concern here, and European leaders generally discounted Russian army strength (Fuller, 1998), we need some weighting scheme to discount Russian army size to reflect more accurately its relative influence.²⁷

The problem is that any discount scheme would have to be quite large at times and certainly not constant over the three centuries of Russia's involvement in the modern European great power system. Our solution is to use Russian army sizes for computing European totals but not to accept the strong Russian army share as an indicator of "most powerful" status prior to 1945. One consequence is that we often accept the state with the second highest relative share as the army leader in the 18th, 19th, and early 20th centuries. Another is that some of the leads maintained by the number two power will not be fully comparable with earlier, pre-Russian leads. An example is the French score in the 1800s decade. Without Russia in the mix, the French share would have been 0.516. With Russia, France's lead is recorded as a 0.38 share (still above our threshold).

While this element of noncomparability is regrettable, we have decided to accept it as part of the compromises necessary to measure the distribution of capabilities and to operationalize the balancing question. The alternative is to designate Russia as the leading European land power for the past 200 years - a position which simply lacks any face validity. Since the Russian "distortion" problem tends to

flatten leads achieved after the early 18th century, it should weaken support for our balancing propositions. While we would prefer not to bias the analysis in either direction, a bias that works against our propositions is preferable to one that favors our propositions.²⁸ Table 1 summarizes the identities of the states accorded leading European land power status according to our rules.

/Table 1 about here/

The leading power does not necessarily pose a serious threat to the other great powers in the system, especially if the magnitude of its superiority is a modest one. The early pattern in table 1 is one of Spanish and French clusters of leading land power status (associated with Charles V, Phillip II, Louis XIV, and Napoleon) which does tap into their periods of continental predominance. In between the “mounds” of predominance, however, other states appear as leading powers in part because the previous leader has been exhausted and/or demobilized, either temporarily or permanently. Between Napoleon and 1945, a different pattern - one of an absence of clear numerical predominance - prevailed. We need some measure of leading status that works across both early and later patterns to discriminate between a moderate leading status and one that is more likely to trigger perceptions of hegemonic threat and counter-balancing behavior.

One possible threshold is a 50 % share of the capabilities in the system,²⁹ which is a nice focal point but not very useful for our purposes. First, only rarely does a great power reach this level of relative military power. If we discount the strength of the large Russian armies of the 20th century, for reasons outlined above, the only great power to surpass a 50% capability share is the Habsburgs from about 1560-1640. This is obviously too narrow a period for a test of balancing propositions. Second, we would expect that even concentrations of power short of the 50% threshold should generate perceptions of potential threats of hegemony and generate balancing behavior.

We have concluded that a 33% capability provides a more reasonable indicator of a position of dominance sufficient to generate perceptions of hegemonic threat. Admittedly, any number below 50% would be somewhat arbitrary, but we want a single threshold that will be useful in the context of the fluctuating number of great powers in the European system over time, given the awkwardness of using different thresholds for different numbers of great powers. Only in a three actor system would 33 percent be unremarkable, but there have always been more than three great powers in the system, given our operationalization of the concept. The number of great powers has dropped as low as four, but only in a few years in the mid-1500s and mid-1940s, and even then a 33% threshold constitutes a capability share nearly 50% higher than the average for the other great powers in the system. In a large pool of 6 or 7 powers, the possession of a third of the land capability pool could represent a quite formidable concentration of power. Thus, we think a 33% threshold serves our threshold purposes reasonably well.

Some other operational caveats are in order. We are not necessarily claiming that army size would be a good predictor of relative power outside the European region, particularly if there are

significant asymmetries in technology. The army size indicator emphasizes the quantitative dimension while ignoring the qualitative dimension. Big armies can be poorly trained, armed, and/or managed. Small armies can possess more elan, superior tactics, and longer pikes or faster-loading rifles. While lacking access to standardized information on the relative quality of European armies over 500 years is an excuse for sidestepping this important dimension, it also stands as a basic limitation on the interpretation of army size that should be kept in mind in subsequent analyses. Still, army size does capture nicely Stalin's ultra-realist question about how many troops could the Vatican muster. Army size is one of the few indicators in international politics that possesses a roughly similar meaning over the past half-millennium. It also seems particularly appropriate for a focus on regional politics and territorial expansion - both of which require military manpower in the 16th century as much as in the 20th century. In addition, technological asymmetries that might weaken the utility of the army size indicator are minimized by our focus on the European great power system, which has been characterized by relatively rapid technological diffusion.

We have defined our dependent variable in terms of the extent of coalitional balancing in response to concentrations of power. Our focus is military alliances based on formal agreements that require one state to intervene militarily in support of another in the event that one is attacked. Given our interest in balancing against the dominant state, we limit our attention to those "targeted alliances" (Walker, 2001) that identify the dominant state in the system as the specific threat.

We also limit our attention to formal, written alliance treaties. While not all formal alliance treaties are honored, and while some unwritten agreements reflect a serious intent to invoke the alliance in the event of attack, the former are much better than the latter as general indicators of likely commitment over a large number of cases, in part because a formal, written commitment involves a "costly signal" of a state's willingness to honor the treaty (Morrow 2000).³⁰ Recent evidence suggests that states live up to their alliance commitments 75% of the time in the last two centuries (Leeds et al., 2000), with selection effects explaining why we do not always observe this (Smith, 1995).

By focusing on written alliance treaties, we undoubtedly exclude some cases of alignments that were never formalized but that contributed to joint responses against an aggressor. In excluding such cases, our formalization requirement constitutes a conservative bias in our test of balancing. Another way in which we err on the conservative side is that we exclude military interventions that do not involve a formal, written treaty. For example, we do not include the United States as a member of the balancing coalition in World War I (because it was not a European actor and) because its military role was not accompanied by any formal treaty. We could not include the United States because it was "obviously" balancing against Germany³¹ unless we constructed an indicator of state intentions to balance and applied it systematically to all wars over the last five centuries. Note, however, that in cases of intervention in ongoing wars the bias concerns only the size of a wartime balancing coalition, not whether balancing occurs.

To construct an inventory of formal alliances for the last five centuries we utilized four previously

constructed data sets (Small and Singer 1969; Levy 1981; Correlates of War Alliance List 1993; Gibler 1999) and 29 general or specific diplomatic histories.³² The multiple-source focus is necessary given the absence of agreement across these sources as to the specifics of various alliances or even their existence. Each source has a story to tell and a theme to emphasize, and some selectivity is to be expected. Thus, there is a need to consult multiple sources (and older ones at that) until one begins to feel that further sources are unlikely to yield any new information.³³ Finally, there was considerable ambiguity regarding the precise duration of alliances in early modern Europe. Unless it was clear that an alliance persisted, we assumed that many of the earlier cases were rather short-lived, lasting only for the year in which they were actually created or to the end of the war that often followed their creation.

These procedures led to the identification of 223 alliances, though this number is sensitive to exactly how one counts subsequent joiners and renewals, which we handled on a case-by-case basis. Not all of these alliances are directly relevant for this study, however, because our hypotheses call for a focus on alliances that are explicitly targeted against the leading military power.³⁴ This leaves us with 84 relevant targeted alliances.

While there is undoubtedly some measurement error in the data, we want to emphasize that we have defined balancing narrowly in terms of alliances and narrower still in terms of written alliance treaties specifying as a target the system's most powerful state, so that most of the biases in the data work against the confirmation of balancing hypotheses for the European system.

DATA ANALYSIS

We begin by considering two general balancing propositions from the literature that we expect to receive weak support at best: that great powers tend to balance against the strongest state in the system, irrespective of the magnitude of its superiority, or that great powers tend to balance against the strongest state in the system if it is significantly increasing its military strength (defined here as a 10% increase from the last period). We are skeptical of these propositions, and we formulated hypotheses 1 and 2 to predict no strong patterns of balancing under these two sets of conditions.

Information relevant to balancing against the lead state, irrespective of the magnitude of its advantage, can be found in the marginal frequencies to the right of table 2 or of table 3.³⁵ Great power alliances have formed against the lead state in 202 cases out of a total of 451 possible opportunities, or about 45% of the time, in contrast to 249 instances of no alliance response (55%). This is strong support for the expectation from hypothesis 1 of no strong patterns of balancing against just any lead state.

/Table 2 about here/

Our expectation from hypothesis 2 is that there should be no strong patterns of balancing against

a lead state that is significantly increasing its capabilities (by 10% since the last period).³⁶ Table 2 compares alliance formation and non-formation associated with lead state capability changes of less than 10% with changes greater than 10%. We find that while an alliance response against the leading power is somewhat more likely when capabilities are increasing, the observed relationship ($p=.18$) falls short of statistical significance. This absence of strong support for balancing is consistent with our second hypothesis.

As expected, therefore, we find no evidence of any tendency for great powers to balance against the strongest state in the system irrespective of the magnitude of that state's superiority. This null finding, which contradicts much of the traditional balance of power literature, provides a useful baseline for analyzing our primary hypotheses, which emphasize balancing against the leading state in the system if and only if it poses a hegemonic threat based on a high concentration or disproportionate share of land-based military power in the system.

Hypothesis 3 suggests that the stronger the lead state's relative capability position, the more likely that other great powers will balance against it by forming a military alliance. Table 3 contrasts alliance responses to leading states possessing a third or more of the military capabilities in the system with responses to leading states possessing less than a third of the capabilities in the system. We find a strong and statistically significant ($p=.000$) tendency for states with stronger relative positions to provoke an alliance reaction, compared to states with weaker relative positions. This finding represents clear support for this variant of the proposition that great powers tend to balance against hegemonic threats.

This finding is all the more significant if judged in the context of great power alliance formation in general, not just targeted alliance formation against the most powerful state in the system. This draws on information that is not presented in Table 3. If we switch our dependent variable from targeted alliances/non-alliances against the lead state to great power alliances against any great power, we find no statistically significant relationship between power concentration at the 33 percent or higher level and alliance making in general. European great powers have been just as likely to ally in periods in which one state has over a third of the capabilities in the system (68%) as in periods without such a dominant actor (70%). This suggests that our analysis in table 3 is indeed capturing balancing against a leading state with hegemonic potential rather than other kinds of alliance formation, such as those driven by traditional bilateral rivalries.

/Table 3 about here/

Hypothesis 4 taps another dimension of balancing behavior – the size of the balancing coalition, defined here in terms of the number of great powers in the alliance. Most balance of power theories imply that hegemonic threats deriving from high concentrations of power should not only generate a counter-balancing coalition, but a coalition involving several great powers rather than just two. Multilateral coalitions are also significant for another reason. The more states joining an alliance, the more plausible it is to infer that they were driven by considerations of balancing for the collective good

of avoiding hegemony as opposed to more limited parochial goals. Table 4 summarizes the relevant information on the prediction of hypothesis 4 that wider coalitions are more likely to be formed against leading states with a high capability share than with weaker and presumably less threatening leading powers.

/Table 4 about here/

There are two ways to evaluate table 4 but both ways suggest the same outcome. If one includes the cells for no alliance response, 57.5 percent of responses to dominant leaders (with 33% capability share or more) involved coalitions of three or more actors, in comparison to 21.3 percent for the responses to non-dominant leaders. Focusing only on the cases in which actors actually responded (dropping the zero cell), the ratio between situations in which a dominant leader was present as opposed to absent is still 79:39, or 2:1. Thus high concentrations of power not only encourage alliance responses, they also stimulate the wider coalitions that are often equated with balance of power coalitions.

Balance of power theory implies that high concentrations of power should be perceived as even more threatening if those concentrations are increasing, if the dominant power is increasing in strength. Table 5 pairs the 33 percent or greater position with the 10 percent increase in capabilities, and shows substantial and statistically significant ($p=.000$) support for the hypothesis that situations in which very strong states are becoming stronger are more likely to provoke balancing behavior than are other situations. Still, we should note that most of the effect derives from the level of capabilities rather than their rate of increase. A 33% capability share leads to a counterbalancing coalition 60% of the time (see table 3), and the combination of a 33% capability share and a 10% increase leads to a counterbalancing coalition 65% of the time.

/Table 5 about here/

These results taken as a whole suggest that high concentrations of military power tend to systematically generate balancing behavior, and that situations in which such powerful states are becoming still stronger are somewhat even more likely to trigger balancing coalitions. While table 4 supports these conclusions in terms of the size of the coalitions involved, we hasten to add that the propensity to balance is obviously far from automatic. The number of the great powers in the European system has ranged from four to seven, while the average size of a balancing coalition, given that a coalition forms (excluding the 0-row in the middle column), is 3.34. Thus while a balancing coalition usually forms in response to a high concentration of capabilities, only some but not all great powers join that coalition, while others stand aside and some even bandwagon with the dominant state.³⁷ Balancing against concentrations of power is a probabilistic tendency, not an “iron law” of behavior in international politics.

SUMMARY AND CONCLUSION

We have argued that despite the ambiguities in balance of power theory, there is one core proposition that nearly all balance of power theorists would accept -- the threat of domination by the leading land power in continental systems induces other great powers to form counter-balancing alliances against the dominant state. We have tested this proposition for the European system of the past 500 years. For our independent variable, we have operationalized land-based military strength in terms of army size, and hegemonic threat in terms of the possession by any single state of 33% or more of the capabilities in the system. Our dependent variable is balancing behavior (not peace/war), defined as the formation/non-formation of a formal military alliances directed against the leading army power, aggregated by five-year period. Our research design follows a "most likely case" logic -- if we find no systematic evidence of balancing in the European international system, we suspect that we are unlikely to find systematic tendencies toward balancing in other systems. Within that design, we argue that our test is a conservative one because we have constructed our operational indicators in a way that defines balancing rather narrowly, in a way should work against the balancing hypothesis.

Before testing our primary proposition, we examined the argument, common to many but not all balance of power theorists, that states balance against the most powerful state in the system. This argument is almost certainly false as stated, given the many small states who prefer to hide or bandwagon with the strongest state rather than balance against it. A more plausible reformulation of this proposition, based on our argument about the implicit scope conditions associated with balance of power theory, is the following: great powers tend to balance against the most powerful land-based military power in the European system.

We were skeptical even of this proposition. Our argument is that it is hegemonic threats, not lesser threats, that tend to generate balancing coalitions, and that such threats generally arise only from states that possess a significant advantage in military capabilities relative to its competitors. Such state generally pose the greatest threats to other great powers, so that balance of power theory and balance of threat theory converge in their predictions. If the leading state has only a modest advantage in military strength, we predict no systematic tendencies toward balancing. Thus our first hypothesis predicts no strong patterns of balancing against the strongest state in the system irrespective of the magnitude of its relative strength. By similar logic, hypothesis two predicts no systematic tendencies toward balancing toward a lead state whose relative power is increasing, irrespective of the magnitude of its relative strength.

Our findings provide fairly strong support for our first two hypotheses. Great power alliances form against the most powerful state in the system only about 45% of the time. Non-balancing is a more common response than is balancing, which supports hypothesis 1. If we look at response to significant increases in military capabilities of the most powerful state, independently of the magnitude of its relative power, we find that while there is some tendency toward great power alliance formation in response to capability increases by the leading power, the observed relationship is a modest one that falls short of

statistical significance.

As expected, therefore, we find no evidence of any strong tendency for great powers to balance against the most powerful state in the system irrespective of the magnitude of that state's superiority. This null finding, which contradicts much of the traditional balance of power literature, provides a useful baseline for analyzing our primary hypotheses, which emphasize balancing against the leading state in the system if and only if it poses a hegemonic threat based on a high concentration of land-based military power, which we operationalize as 33% or more of the capability shares in the system.

This basic idea is captured by hypothesis 3, which gains strong support from the data. Once the leading state comes to control a third of the capabilities in the system, a balancing coalition of other great powers has formed 60 percent of the time during the past five centuries. We have included tests for two other variations of the balancing hypothesis, one based on the size of the balancing coalition and one on the response to a combination of a high concentration of capabilities and an increase in those capabilities by the leading state. With respect to the first we find that high concentrations of power are more likely than lower concentrations of power to be followed by larger balancing coalitions involving three or more great powers, which confirms hypothesis 4. We also find that when high concentrations of power (33%) are combined with *increasing* concentrations of power (with a 10% threshold), a great power alliance follows nearly two-thirds of the time, confirming hypothesis 5.

These findings raise a number of questions for further research, some regarding the mechanisms driving the European system which we have investigated and some regarding the extent to which our findings can be generalized to other systems. Let us begin with the former. One of the most basic questions emerges from our finding that while great powers have a tendency to balance by forming alliances against any state possessing a disproportionate share of the military capabilities in the system, they do not always balance, and the size of balancing coalitions often falls considerably short of the number of potential great power balancers in the system. Under what conditions do great powers balance, and under what conditions do they stand aside? Can variations in behavior be explained in terms of the geography of the system, the location of key rivalries in the system, or by potential balancers' involvement in other wars at the time (the Ottoman wars against the Persians, for example)?³⁸ Can all these phenomena be subsumed under collective-action problems, or is something else going on?

Another question concerns the timing of balancing. At what point do great powers balance against concentrations of power and hegemonic threats? In this study we have not distinguished between the formation of balancing coalitions in peacetime and the formation of coalitions in wartime. Do great powers respond to concentrations of power by forming counter-balancing coalitions in peacetime, or does it usually take the outbreak of war to emphasize the seriousness of the threat, overcome wishful thinking that downplayed hostile intentions, and overcome collective action problems and incentives to free ride on others' balancing? We know that balancing occurs without war, and in fact the formation of a counter-balancing alliance and the anticipation of its terms being invoked by

expansionist behavior may have a causal impact on the absence of war. NATO provides a clear example of balancing in peacetime, and arguably a case of the causal impact of balancing on the absence of war. When balancing is associated with war, however, does balancing tend to occur prior to war or after the outbreak of war, and what are the causal mechanisms driving these patterns? Stated differently, do states balance in anticipation of a war that they perceive is likely or that they hope to deter, or do they balance in response to war?

One of the more basic questions to arise from this study, and one that requires much further research, is the extent to which our findings can be generalized to other systems. We have emphasized that our formulation of the balancing proposition in terms of great power balancing against hegemonic threats in Europe represents a most-likely case for the balancing proposition. This creates an asymmetry in the logic of inference from our negative and positive findings. We have found, first of all, little support for the common (but not universally-accepted) proposition that great powers tend to balance against the strongest military power in the system. If this proposition does not apply in Europe, which the literature suggests it is most likely to apply, then we can have little confidence that the proposition holds elsewhere. While further research is still warranted and in fact desirable, and while we certainly expect that many instances of balancing against the strongest state in the system will be uncovered, we do not expect to find systematic patterns of balancing against the strongest states in the system unless those states amass a much greater proportion of the military capabilities in the system.

The logic of inference for our more positive findings is different. Whereas disconfirming evidence from most-likely tests provides some confidence that the hypotheses in question cannot be generalized to other systems, the same cannot be said for the findings that confirm our other hypotheses. The empirical support for our propositions about balancing against hegemonic threats or high concentrations of power, while valid for the historically important case of Europe, give us far less leverage for generalizing to other systems. These findings increase our confidence that our hypotheses are valid in other autonomous continental systems, but only modestly. This makes additional empirical research on other systems absolutely imperative.

Having said this, it is true that the logic of our theoretical argument does have certain implications as to where, outside of Europe, balancing should be most likely and where it should be least likely, and these considerations can help guide future research. We have argued that balancing coalitions in response to concentrations of power should be most likely to form in autonomous continental systems, and least likely to form in response to concentrations of power in maritime systems or any global system in which power is based on a combination of economic strength and military power of global reach. We include the "autonomous" qualifier to continental systems because the existence of stronger great powers outside of those systems can affect the security of states in the system and provide alternatives to standard balancing strategies. The critical assumption of anarchy underlying theories of balance and balancing may not necessarily be satisfied in such systems. While balance of power/threat theorists sometimes apply the balancing proposition to regional systems (Walt, 1987), the fact that key assumptions of the theory, particularly anarchy, are not satisfied in such systems leads us to be very

cautious.

We have examined balancing strategies rather than balanced outcomes because our focus is on Europe and because the former but not the latter have varied over the last five centuries of the modern European system. The multi-state system in Europe has been fairly stable in that a European hegemony has not formed, but the same is not true of all other multi-state systems, where system-wide empires have occasionally emerged. One is the emergence of a hegemonic empire under the Qin Dynasty by 221BC after the collapse of the Chinese multi-state system, and another is the subsequent emergence of the Han dynasty over China. Still another case is the transformation of Assyria from a decentralized system to a centralized empire in the 8th century BC (Wohlforth and Kaufman, 2003).

While scholars are just beginning to examine these non-European systems through the analytic lenses of international relations theory, we need much more work on such systems if we are to fully understand the conditions under which balances form and break down, and the role of balancing strategies in the process. We also need to understand exactly how frequently hegemonies emerge in multi-state systems and to identify the conditions under which this occurs and the causal mechanisms that drive the process. Such an inquiry, which requires careful comparative historical research, will lead back to the important question of whether the European international system is fairly typical of the dynamics of international systems or whether it is an anomaly. The answer to this question will have an important bearing on the broader relevance of our theories of international relations, which have been disproportionately influenced by the European experience. We have argued that balance of power theory contains implicit Eurocentric biases. Perhaps the most significant bias is the implicit assumption that hegemonies rarely if ever arise. This is an empirical question that needs much further exploration.

Figure 1: European Dominance Patterns

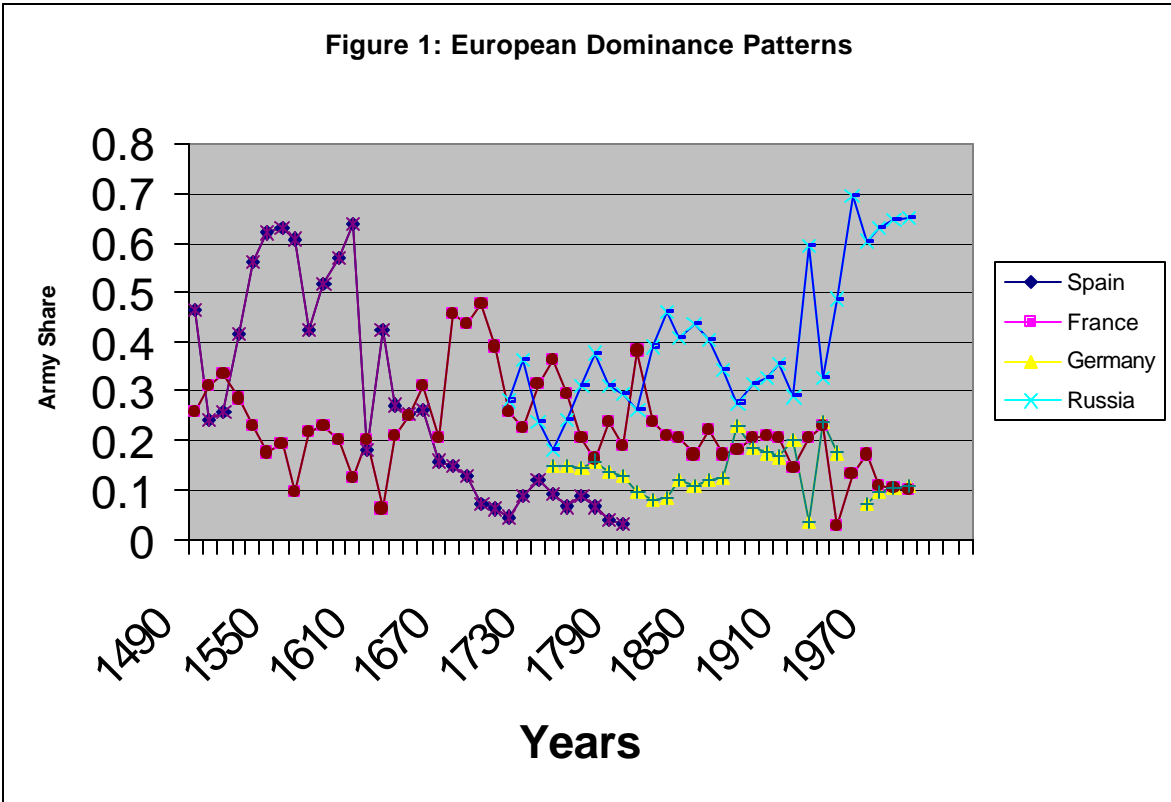


Table 1 Leading European Land Power, 1495-1999

Years	Leading land power	Years	Leading land power
1495-1499	France	1790-1814	France
1500-1504	England	1815-1819	Austria
1505-1514	France	1820-1829	France
1515-1524	Ottoman Empire	1830-1834	Austria
1525-1609	Habsburg/Spain	1835-1839	France
1610-1619	Ottoman Empire	1840-1849	Austria
1620-1644	Spain	1850-1864	France
1645-1659	France	1865-1869	Austria
1660-1664	Austria	1870-1874	Germany
1665-1714	France	1875-1909	France
1715-1729	Austria	1910-1914	Germany
1730-1764	France	1915-1934	France
1765-1774	Austria	1935-1944	Germany
1775-1779	France	1945-1999	Russia
1780-1789	Austria		

**Table 2: Capability Change and Alliance Response
Against the Leading Power**

1495- 1999

	capability change of less than 10%	capability change of 10% or more
no alliance response	151 (. 570)	98 (.527)
alliance response	114 (.430)	88 (. 473)
N = 451	265 (1.000)	186 (1.000)

Chi. sq. = 1.79; p = .181

Table 3: Positional Dominance (33% or greater) and Alliance Response Against the Leading Power

1495- 1999

	weaker relative position (less than 33%)	stronger relative position (33% or greater)
no alliance response	184 (.637)	65 (.401)
alliance response	105 (.363)	97 (.599)
N = 451	289 (1.000)	162 (1.000)

Chi. Sq. = 28.27; p = .000

Table 4: Dominance and Coalition Size

Number of major powers allying against the lead power	Dominant leader (with 33 % share or greater) is present	Dominant leader (with 33% share or greater) is absent
0	11 (.275)	28 (.459)
2	6 (.150)	20 (.328)
3	8 (.200)	10 (.164)
4	14 (.350)	2 (.033)
5	1 (.025)	1 (.016)
Total	40 (1.000)	61 (1.000)

Table 5: Positional Dominance (33% or greater)/Increasing Capabilities and Alliance Response Against the Leading Power

1495- 1999

	weaker relative position (less than 33%) and or less than 10% capability increase	stronger relative position (33% or greater) and 10% or greater capability increase
no alliance response	227 (.585)	22 (.349)
alliance response	161 (.415)	41 (.651)
N = 451	388 (1.000)	63 (1.000)

Chi. Sq. = 12.19; p = .000

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NOTES

1. A few examples would be helpful. Many weak states allied with Napoleonic France and then with Nazi Germany. Does this falsify the balancing hypothesis, and suggest that states often bandwagon rather than balance. Or does it reflect a more limited balancing proposition that applies to great powers and not to weaker states? In the period prior to World War I, Britain failed to make any commitment to intervene against Germany in the event of war, and some (Fischer, 1967) claim that this played a critical role in the outbreak of war by undermining deterrence. Does Britain's behavior contradict the balancing hypothesis, or does Britain's subsequent intervention in the war provide support for the hypothesis? Similarly, does British and French behavior in the 1930s – appeasement followed by military intervention – falsify or confirm the balancing hypothesis? Finally, the great powers did not balance against Great Britain, unquestionably the leading power in the world in the 19th century, or against the United States after World War II or even after its rise to hegemonic status after the collapse of the Soviet Union. Does the absence of balancing in these cases falsify the balancing hypothesis, or does it reflect the conditional balancing proposition that great powers balance against potential continental land-based hegemonies but not necessarily against global hegemonies or maritime powers (Levy 2002b)?
2. David (1991) suggest that political leaders often seek external alliances for political support against internal enemies. Barnett and Levy (1991) emphasize the economic motivations underlying alliance formation, particularly for weaker states, but argue that the resources extracted from allies are sought primarily for domestic purposes. Barnett (1996) argued that states often balance against threats to national identity rather than against more narrowly-defined security threats.
3. Vasquez (2002) makes a similar argument. Vasquez (1997) also argues that attempts to refine the balancing proposition have taken the form of ad hoc attempts to save the proposition without adding theoretical and empirical content, and consequently that realist research on balancing constitutes a degenerating research program from the perspective of Lakatos' (1970) methodology of scientific research programs (Vasquez 1997). For responses to Vasquez see the essays by Waltz, Christensen and Snyder, Elman and Elman, Schweller, and Walt in the December 1997 *APSR* symposium, reprinted in Vasquez and Elman (2002).
4. This conception of the analytical limitations of the balance of power literature and the balancing proposition in particular, including its implicit great power and Eurocentric biases, and the argument that an empirical analysis of balancing in Europe follows the logic of a most-likely case design, is developed in Levy (2002b).
5. Thus great powers did not balance against Britain at the peak of its global leadership in economic and naval power, and we would not expect them to balance today in response to American global dominance. The absence of balancing against global powers follows directly from leadership long cycle theory (Thompson, 1988; Rasler and Thompson, 1994) and from many applications of power transition theory (Organski and Kugler, 1980).

6. In a variation of the first path, a blocking coalition forms not in response to blatantly expansionist behavior, but rather in response to the concentration of capabilities alone, and the alliance successfully deters any expansionist behavior and war that would likely follow. One salient example here is the formation of the NATO alliance against the Soviet Union after World War II. This case raises the standard question of how to validate the success of general deterrence. In this case, did the existence of NATO deter Soviet expansion, or did the Soviets have no plans to expand to begin with?
7. The dyadic-level “power parity” hypothesis predicts that equality between two states leads to peace, but the power parity hypothesis is analytically distinct from systemic-level balance of power theory. While rejecting any strong bivariate relationship between the distribution of power and the likelihood of war, some have theorized about the conditions under which certain distributions of power lead to war while other conditions lead to peace (Buono de Mesquita and Lalman 1992; Wagner 1994; Kadera 2001).
8. Waltz (1979, 1997) is not always consistent on this matter, and often speaks about balancing as well as balances (Levy, 2002b; Elman, 1996).
9. For further discussion of the great power, Eurocentric, and military orientation of most balance of power theories see Sheehan (1996) and Levy (2002b).
10. Waltz refers to Charles I, the Spanish king who became Charles V when he was elected Holy Roman Emperor in 1519.
11. The failure to integrate European and global perspectives is an important limitation in the literature. For an exception see Rasler and Thompson (1994).
12. See the essays in the 1997 *APSR* symposium on balancing, in Vasquez and Elman (2002).
13. This is the first path to balanced outcomes noted above: the anticipation of balancing leads to general deterrence and the absence of a bid for dominance. While the study of unobserved balancing, like the study of general deterrence success, involves the rather serious problem of identifying the population of cases to which we want to generalize and from which we want to sample, we can identify a few cases for illustrative purposes and possibly for more intensive examination. One example is Russia’s failure to play a more assertive role in the Turkish crises in the late 1820s and 1830s because of the fear of provoking a counter-balancing coalition (Rendall 2000, 2002). Another is the Soviet Union’s restraint during the Cold War (Gaddis, 1998).
14. Other possible forms of external balancing might include territorial partitions and compensations (Gulick, 1955).
15. However, see Thompson and Rasler (1999) on this question. At the very least, there was a tendency for each successive hegemonic aspirant to ratchet up the competitive level of standing armed forces.

16. Spain and Austria were joined under the rule of Charles V from 1519-1556, and we treat the "United Habsburgs" as a single great power during that period.

17. Russia includes the Soviet Union from 1917-1991. Prussia/Germany includes the Federal Republic of Germany from 1945-1989. There are enormous conceptual problems involved in specifying a European system after 1945, when that system ceased to be the dominant subsystem in a larger global system. While Britain, France, and West Germany may not have been global powers throughout most of the post-1945 period, they were the leading powers in the European system (along with the Soviet Union and the United States). We include West Germany because the future of Germany was the central issue in the Cold War (Trachtenberg, 1999), within the West as well as between West and East, and because the West German army was the foundation of NATO's conventional defense in Europe.

18. In Kaplan's (1957) terms, the pre-1945 system was "subsystem dominant."

19. This is reflected not only in the NATO alliance of the leading European states against the Soviet Union as the strongest land-based military power on the continent, but also in the widely-repeated comment attributed to Lord Ismay – that the aim of NATO was "to keep the Russians out, the Americans in, and the Germans down."

20. While the United States was critical in many respects to the NATO coalition that formed after 1945, alliances among European states did precede the formation of NATO. Nor do we feel any need to add the United States as a European state after 1945. The United States became a major actor in Europe but was never a European state. To treat it otherwise would also force us to abandon our regional focus. Finally, we recognize that pre-1939 European great powers were no longer great powers after 1945, but we employ the pre-1939 elite designations (Russia, Britain, France, Germany, and Italy) to identify the European elite of the post-1945 era.

21. The half-decadal temporal unit of analysis is arbitrary. The army data are available in five year averages, thereby eliminating year-by-year analyses. A ten year interval might allow for some lag between the perceived need to balance and actually creating an alliance, but it also encompasses a rather long period of time during which wars can begin and end and alliance partners can switch sides. The shortest time period possible seems preferable in terms of evading the potential for distortion associated with assigning a representative score to a temporal unit in which markedly different types of behavior can be observed.

22. The wealthiest European state in the 16th century owed its sinews of war capability more to American silver than to its indigenous wool cash crop. On the other hand, the agrarian wealth of France was especially difficult to mobilize for state purposes (Rasler and Thompson, 1989: 94-97).

23. A few small states (e.g., the Netherlands) were able to develop their trading sectors sufficiently to compete with and defeat the large agrarian land powers, at least for a time.

24. In Western Europe, for example, France was the consistent leader in population size through the mid-19th century until it was edged aside by a unified Germany after 1871. Although this shift in status had reverberations in international relations, French population size is not a good predictor of the extent, success, or predominance of French foreign policy prior to 1871. There are simply too many years in which French population remained large while its foreign policy activity was quite limited due to various considerations such as internal turmoil or state insolvency.

25. An important exception is twentieth century Germany.

26. In some years, cavalry auxiliaries could double the size of the Ottoman army for short periods of time (Murphey, 1999).

27. See Wohlforth (1987) on problems in estimating Russian capabilities prior to World War I.

28. We also experimented with a consistent 50 percent discount of Russian army size but found that it did not alter any of our findings.

29. Many formal models imply that a capability share exceeding 50% essentially constitutes hegemony. Niou, Ordeshook, and Rose (1989:76), for example, state that "If any single country controls a majority of the resources available to all countries in the system, then that country will eliminate all other and expropriate all the available resources."

30. The causal relationship between alliance behavior and subsequent warfare is complex and contested (Wagner, 1986; 1994; Smith 1995; Maoz, 2000; Morrow, 2000).

31. There are other, non-balancing interpretations of American intervention in World War I (Bass 1964).

32. These include Hassall (1905), Barker (1906), Hill (1906, 1914) [which was by far the most comprehensive], Atkinson (1908), Turner (1908), Mowat (1924), Reddaway (1936), Petrie (1947, 1949), Marriott (1950), Taylor (1954), Spooner (1958), Langer (1968), Wolf (1968), Beller (1970), Livet (1970), Ward and Gooch (1970), Fernandez Alvarez (1975), Dmytryshyn (1977), Hillgarth (1978), Jones (1980), Stradling (1981), McKay and Scott (1983), Lynch (1984), Bonney (1991), Anderson (1998), Morris (1998), Stoye (2000), Knecht (2001), and Sturdy (2002).

33. Older sources are much preferable to more contemporary sources that spend more time on interpretation than sheer and even trivial description. More contemporary treatments tend to focus on selected alliances of major significance while the older studies were less likely to discriminate on the basis of historical significance.

34. We excluded alliances when it was not clear whether there was a specific target or who the target was.

35. These marginal frequencies represent the sum (across columns) of responses (or non-responses) to capability change under 10% and over 10%, which is equivalent to responses to the lead state regardless of its rate of change in power. The same logic applies to the marginal frequencies in table 3.

36. The 10 percent or higher calculation is based on the change in a state's absolute capability base and not changes in its relative share. A 10 percent increase in relative capabilities, would be too high a threshold for establishing minimal significance. Most of our analyses are based on an N of 451, which represents the sum of the number of possible great power alliance responses minus the leading state's alliance response per half-decade (the leading state cannot ally against itself).

37. Since we coded for alliance against the targeted state or no alliance, our data does not discriminate between bystanding and bandwagoning.

38. We also need to think more about the question of whether partial balancing of this kind supports the basic balancing proposition or contradicts it. Which is more relevant for the empirical confirmation of the balancing hypothesis -- the fact that balancing coalitions systematically form in response to high levels of capability concentration, or the fact that not all great powers participate in these balancing coalitions?