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Getting Ahead in the Communist Party: Explaining the Advancement of Central Committee Members in China

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Spectacular economic growth in China suggests the ruling Chinese Communist Party (CCP) has somehow gotten it right. A key hypothesis in both economics and political science is that the CCP's cadre evaluation system, combined with China's geography-based governing logic, has motivated local administrators to compete with one another to generate high growth. We raise a number of theoretical and empirical challenges to this claim. Using a new biographical database of Central Committee members, a previously overlooked feature of CCP reporting, and a novel Bayesian method that can estimate individual-level correlates of partially observed ranks, we find no evidence that strong growth performance was rewarded with higher party ranks at any of the postreform party congresses. Instead, factional ties with various top leaders, educational qualifications, and provincial revenue collection played substantial roles in elite ranking, suggesting that promotion systems served the immediate needs of the regime and its leaders, rather than encompassing goals such as economic growth.

Ever since Weber ([1921] 1958) distinguished rational bureaucracy staffed by impersonal, meritocratically selected technocrats from patriarchical management by loyal partisans, scholars have explored the underlying logics driving government organizations. Today Weber's distinction matters nowhere more than in China, where an authoritarian regime governs nearly a fifth of the world's people. Thirty years of spectacular economic performance has prompted a sizable political science and economics literature linking the bureaucracy of the Chinese Communist Party (CCP) to China's economic success. Though the link seems natural, there are good theoretical and empirical reasons to question whether yardstick competition among local officials really caused China's spectacular growth. Theoretically, a performance-based promotion system in the top echelon of the party requires a relatively unified and far-horizoned leadership that would systematically promote officials with the strongest performance (Olson 1993; Olson and McGuire 1996). Yet theories of authoritarian politics and China scholarship suggest that the top leadership in China is as much preoccupied with internal struggle as with achieving regime-wide objectives like economic growth (Bueno de Mesquita et al. 2003; MacFarquhar and Schoenhals 2006; Nathan 1973; Svolik 2009; Tullock 1987). As such, it is far from clear that CCP cadre management institutions were directly responsible for China's economic performance.

Empirical work on the growth incentive embedded in CCP institutions shows cases where cadre evaluation appeared to encourage strong performance by county and township level officials (Edin 2003; Whiting 2004). Yet systematic tests of whether provincial administrators in high-growth regions were awarded with promotions look only at provincial officials, instead of the Central Committee power elite in the CCP, and measure only promotion in state bureaucratic ranks instead of the more important party ranking (Chen, Li, and Zhou 2005; Li and Zhou 2005; Maskin, Qian, and Xu 2000). And no qualitative or quantitative work addresses the potential selection bias that would arise if politically connected officials could influence the location of their next appointments to claim credit for preexisting growth trends.

This study departs from previous studies in five important ways. First, instead of providing yet another test of the growth impact of CCP institutions, we draw from the broader political economy literature on authoritarian regimes to derive a wider range of hypotheses on the factors that help cadres obtain higher positions in the party hierarchy. Second, instead of examining only the fortune of local administrators, we examine every full and alternate member of the CCP Central Committee through five party congresses from 1982 to 2002. Third, we make use of the CCP's own reports of elections to the Alternate Central Committee (ACC) and a new biographical database of Central Committee (CC) and ACC members to place the entire CCP power elite along a continuum in terms of their status *within the ruling party*. Fourth, we analyze the pattern of ranks within the Politburo Standing Committee (PSC), the Politburo, the CC, and the ACC using a novel Bayesian model of ranks that can estimate the individual-level correlates of rank even in rank data that are only partially observed. Finally, we purge the effects of

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any selection bias in appointment using a suite of estimates of preappointment expected provincial economic growth. In this manner, we are able to discern whether expected and unexpected economic performance had any impact on the party ranking of senior cadres throughout the reform period.

When we examine the entire CCP elite, we find no evidence that exceptional economic growth influenced ranking in the party hierarchy. This finding is robust even when we decompose economic performance into expected and unexpected growth and when we use broader measures of economic performance. We find that educational qualifications strongly (and provincial revenue collection and minority representation weakly) bolstered cadres' ranking in some of the party congresses since 1982. Women faced significant disadvantages in three of the five congresses we examine. Most important, factional ties with various top leaders, as well as princeling status, boosted the chance of climbing higher in the CCP upper echelons through much of the reform period. In sum, CCP cadre management institutions were used by top leaders to maintain cadres' human capital, coopt ethnic minorities, and raise revenue collection, all of which directly provided immediate payoffs to the regime. Our findings suggest that senior party leaders did not bolster public goods provision through yardstick competiton among provincial officials, but they did use promotion institutions to cultivate factions. These findings are consistent with the claim of Bueno de Mesquita et al. (2003) that authoritarian regimes tend to deliver private goods to a relatively narrow winning coalition rather than to society at large.

MERITOCRATIC BUREAUCRACY AND ECONOMIC OUTCOMES

Since Weber made the distinction between bureaucracy and patrimonialism, political scientists have argued that government bureaucracies fall into either the meritocratic, impersonal type or the corrupt, clientelist type (Evans 1995; Frye and Shleifer 1997; Schneider 1993; Weber [1921] 1958). Explanations for why bureaucracies develop in disparate ways range from social capital (Putnam, Leonardi, and Nanetti 1993), to the level of external threat (Kang 2002), to colonial legacies (Acemoglu, Johnson, and Robinson 2000). In turn, bureaucratic types are thought to explain complex economic outcomes. Thus, when market enhancing policies are implemented or when growth is high, some scholars attribute this to a meritocratic bureaucracy (Evans 1995; Schneider and Maxfield 1997).

In contemporary social science, the tendency to infer bureaucratic meritocracy from high growth is perhaps strongest in the study of China. Spectacular growth in the past three decades has motivated scholars to develop a variety of explanations of China's success, ranging from a proreform leadership (Fewsmith 1994; Shirk 1993), to de facto federalism (Qian, Weingast, and Montinola 1995), to labor market mobility (Lin 1992; Sachs et al. 1994).

Scholars have also looked to the efficiency or the institutional configuration of the Chinese bureaucracy as explanations of China's rapid growth. Unlike the Soviet Union, which had a unitary system of governance, the primary organizational principle in China was geography-based, thus giving rise to a multidivisional form (M-form) bureaucracy. Some claim the M-form bureaucracy, coupled with an extensive cadre evaluation system, provided strong incentives for regional administrators to compete with each other to generate high economic growth, in order to win promotion in the ruling CCP (Chen, Li, and Zhou 2005; Li and Zhou 2005; Maskin, Qian, and Xu 2000). This body of literature presents mixed evidence that the top two provincial officials (governors and party secretaries) in the reform period were typically rewarded with promotions for realizing better economic performance than their predecessors or their peers (Chen, Li, and Zhou 2005; Li and Zhou 2005; Tao et al. 2010).

In addition to the quantitative findings, a larger qualitative literature on the evolution of the cadre evaluation system also seems to support the view of a performance-based elite promotion system in China. After the founding of the People's Republic of China, the CCP introduced formal methods of evaluating cadres, which were formally linked to their promotion. As Burns (1989) puts it, the cadre management system centered on "lists of leading positions, over which party units exercise the power to make appointments and dismissals; lists of reserves or candidates for these positions; and institutions and processes for making the appropriate personnel changes." The Central Organization Department emerged as the key human resource manager of the CCP, although senior-level promotions were ultimately decided by members of the Politburo (Nathan and Gilley 2002).

During his reign, Mao focused on ensuring the survival of the CCP regime according to his vision. Thus, he promoted cadres who shared or at least were willing to go along with his radical vision of society (Lee 1991). After reform began in 1978, education credentials came to play a prominent role in the advancement of lower level cadres, and older cadres were strongly encouraged to retire (Cui 2003; Landry 2008; Manion 1993; Walder, Li, and Treiman 2000). Furthermore, the post-Mao leadership formalized a system of scoring junior and senior cadres, which served to keep track of their administrative performance on a series of "soft" and "hard" targets. Hard targets reflected core tasks crucial to the regime, including economic growth, fiscal collection, and maintaining stability, whereas soft targets included recruiting party members and propaganda work. Shortfalls on the soft targets could be outweighed by impressive performance in key areas. However, failing to fulfill hard targets led either to low overall scores or to mandatory administrative punishment (*yipiao foujue*), a powerful tool in the CCP's arsenal for political control (Edin 2003). The existence of a comprehensive system for scoring officials on their policy performance seems to suggest that the Chinese elite was motivated by the scoring system to perform well (Edin 2003; Landry 2008; Whiting 2004).

However, on both theoretical and empirical grounds, there are strong reasons to be skeptical of an easy linkage between a formal system of cadre evaluation and economic growth in China. Theoretically, linking a limited set of policy or economic outcomes with bureaucratic characteristics may result in a false causal inference. For one thing, bureaucratic characteristics tend to be time-invariant, whereas policy and growth outcomes change frequently over time. Moreover, seemingly reform policies can mask underlying corruption or inefficiencies. In Pinochet's Chile, for example, liberalization policies implemented by the "Chicago Boys" also allowed favored conglomerates to borrow heavily overseas to buy up prized financial assets, laying the groundwork for the financial crisis in the early 1980s (Haggard and Maxfield 1996; Silva 1996). The empirical findings on corruption also cast doubt on the linkage between bureaucratic characteristics and economic outcomes. Except in extreme cases, high growth generally is not harmed by a moderate degree of corruption (Svensson 2005).

Although meritocratic criteria may influence elite ranking in China, concluding that economic performance is the *dominant* criterion driving elite ranking goes too far. A merit or performance dominant bureaucracy assumes that the Chinese regime is unified and far-horizoned, and thus is motivated to maximize performance and long-term output (Olson and McGuire 1996). But leaders of nondemocratic regimes face constant threats of dethronement from mass uprisings or coups by regime insiders (Tullock 1987; Wintrobe 1998). Thus, autocratic rulers likely place a greater priority on maintaining short-term state capacity and buying the support of winning coalitions than on providing broadly encompassing goods such as economic growth (Bueno de Mesquita et al. 2003; Gandhi and Przeworski 2006). China has witnessed its share of large-scale uprisings and intense political struggle at the top. Even after the chaotic Cultural Revolution, the top leader of the CCP was twice removed from power by irregular means.[1] Besides the Tiananmen protests in 1989, China also dealt with a series of large-scale protests motivated by ethnic and economic grievances in recent years (Pei 2006). Huang (1996) and Sheng (2007) argue that the conflict of interest between central and local governments compels the center to use its appointment control to obtain preferred outcomes, thus implicitly forgoing the appointment of the most capable officials. Because top Chinese leaders must address urgent concerns of stability and retaining office on a daily basis, we expect that the CCP often uses the personnel management system to promote these immediate goals, while neglecting more encompassing objectives like growth or fairness.

If political survival is a main concern, the top leader of a regime may shape policies, including the appointment of subordinates, to serve that end. One elite faction may pursue policies purely to undermine a rival faction, thus risking overall regime stability (Ramseyer and Rosenbluth 1998). Instead of expending resources to prevent a general uprising, leaders seeking to thwart an elite challenger may devote those resources to monitoring or cultivating the loyalty of senior officials (Svolik 2005). And instead of promoting officials with the strongest performance record, rival leaders may promote untalented but loyal followers to prevent a coup or to raise their relative standing (Easter 1996; Egorov and Sonin 2011; Nathan 1973). In other cases, an insecure leader may shuffle or promote officials simply to prevent an alliance between ambitious local officials and the populace (Debs 2007).

Empirically, the literature linking China's cadre evaluation system and growth produces highly ambiguous and problematic results. First, the qualitative literature provides an in-depth description of the cadre evaluation system, which suggests a causal link between cadre evaluation and growth. However, the existence of an evaluation system as such does not prove a causal link. Furthermore, quantitative studies of city and county officials do not find much evidence for growth-based promotions (Guo 2007; Landry 2008). At the city level, for example, Landry (2008) finds that exceptional economic performance has almost no effect on the most likely internal promotion of mayors, to the position of party secretary.

Quantitative studies on provincial officials provide some evidence that exceptional economic growth in a province is correlated with the promotion of the top two officials in a province (Chen, Li, and Zhou 2005; Li and Zhou 2005; Maskin, Qian, and Xu 2000). These studies present a number of empirical shortcomings, however. First, they focus only on provincial officials, leaving out the majority of the party elite, who served in the central party apparatus, the army, and the central economic bureaucracy. A comprehensive analysis of elite incentive should include all full and alternate members of the CC, who make up the bulk of the power elite in the CCP (Kung and Chen 2011; Shirk 1993). Measures of promotion are often poor: Li and Zhou (2005) define promotion as movement to a ministerial position or higher, but in the state bureaucracy, movement from provincial governorship to a ministerial position is horizontal, as provinces and ministries have the same bureaucratic rank. To measure whether such a rotation constituted a promotion, it is more useful to see whether a person's party rank improved, a clearer sign of advancement.

Methodologically, no previous study of elite promotion has taken into account the potential for selection bias (Chen, Li, and Zhou 2005; Guo 2007; Landry 2008; Li and Zhou 2005; Maskin, Qian, and Xu 2000). Aspiring provincial governors and party secretaries may perceive certain provinces as strong economic performers prior to a rotation, and may use political connections to engineer their appointments to claim credit. Under strategic appointment, observed correlation between exceptional growth and advancement might have been caused by strong political connections, rather than by economic performance per se.

[1] For example, party secretary Hu Yaobang was removed from power at the infamous "party life meeting" at Deng Xiaoping's house in early 1987. See Deng (2005).

HYPOTHESES

There are good theoretical and empirical reasons to doubt whether economic performance was the dominant factor driving elite advancement, despite China's geography-based administrative system and elaborate cadre evaluation system. Instead of only testing the impact of economic performance on elite ranking, we draw on the political economy literature on authoritarianism to derive two different sets of hypotheses. If we assumed that China was a unified regime with a long time horizon, we would expect performance, merit, and representation to play the dominant role in elite ranking. However, theories of elite power struggle in authoritarian regimes suggest that factional affiliation and princeling ties may play an important role in CCP elite ranking.

If the top leadership in China were unified and had a long time-horizon, it would have had a "sufficiently encompassing interest" in generating long-term growth and in developing tax collection capacity in an optimal way (Olson and McGuire 1996). Furthermore, to prevent large-scale collective action threatening the regime, a unified Chinese leadership would have combined repression with limited redistribution of its resources or greater representation for certain groups (Acemoglu and Robinson 2006; Desai, Olofsgard, and Yousef 2009; Gandhi and Przeworski 2006; Olson and McGuire 1996; Wintrobe 1998). To accomplish these ends, a unified CCP regime would have promoted officials with a talent for generating growth, collecting taxes, and repressing dissent so that "no matter how strong a candidate's factional backing, he cannot be promoted without a record of administrative achievement" (Nathan and Gilley 2002). From this understanding of elite ranking in China, we derive the following hypotheses:

> ***Hypothesis R1.*** *More educated cadres win higher rank than less educated cadres.*

> ***Hypothesis R2.*** *Regional administrators who generate more economic growth or raise living standards are rewarded with high ranking in the Central Committee.*

> ***Hypothesis R3.*** *Regional administrators who produce higher growth in fiscal revenue win higher rank.*

> ***Hypothesis R4.*** *Officials with more experience in the CCP gain higher rank because they have accumulated more administrative experience.*

Finally, ensuring female and minority representation in the higher reaches of the party may enhance the regime's legitimacy. Furthermore, minority representation may enhance stability by coopting potentially restive minority groups into the system (Dreyer 1976, 113). Thus a unified authoritarian regime might systematically promote women and minorities.

> ***Hypothesis R5.*** *Being female or a member of an ethnic minority confers an advantage in the competition for high rank.*

If a regime experiences concealed or overt power struggles at the top, faction-based promotion predominates. In factional politics, loyalty counts more than skills or performance on core tasks such as growth, revenue collection, and stability. Skills may even be a liability. Egorov and Sonin (2005) provide a convincing theoretical argument that dictators facing the threat of a coup will not always choose the most competent advisors: Clever subordinates may be too good at figuring out the expected payoffs of betraying the dictator; less capable officials are more loyal. Bueno de Mesquita et al. (2003) also argue that autocrats find ways to channel resources to their supporters, including promoting them to senior regime positions. In a similar vein, China scholars long have postulated that Chinese leaders built networks of loyal followers to mitigate the fundamental uncertainty in elite politics (Dittmer 1995; Nathan 1973; Pye 1980, 1992). These factional networks bound the interests of followers to their patrons through the distribution of economic resources and offices (Nathan 1973; Nathan and Tsai 1995; Shih 2008a).

When a factional patron comes under attack, followers remain loyal because they expect large payoffs for protecting the patron, who in turn promotes followers to the upper echelons of the regime. If elite political struggles loomed large in the minds of top CCP leaders, then followers' records of loyalty would have been more important criteria for promotion than economic performance. When the factional logic dominates, individual leaders promote close allies rather than the most capable individuals, even if these appointments lead to the underprovision of regime-wide public goods such as economic growth. In this case, ambitious officials face incentives to signal their loyalty to factional patrons, instead of performing tasks for the regime (Shih 2008*b*). However, even when confronted with intense elite struggle, authoritarian leaders may still prioritize tax collection capacity, to finance both private goods for regime supporters and the regime's repressive capacity (Acemoglu and Robinson 2006; Bueno de Mesquita et al. 2003).

The main implication of the factional view of Chinese politics is the following factional hypothesis:

> ***Hypothesis F1.*** *Followers of the top leaders of the party, including the faction members of Mao Zedong, Deng Xiaoping, Zhao Ziyang, Hu Yaobang, Jiang Zemin, and Hu Jintao, are ranked higher than other officials when their respective patrons are in power.*

In testing this hypothesis, we consider only the followers of the de facto or de jure leaders of the party because as the most powerful leaders, they had the strongest incentive to avoid factional politics and to ensure high regime performance. Thus, if factional politics was not pervasive, we would not expect the highest officials in the regime to rank CC members according to factional considerations.

A related hypothesis concerns the children of senior officials, or "princelings," who entered the CCP elite. Although their parents were never their direct superiors, other senior officials might expect to win some advantage from the families of princelings by promoting them. But helping princelings does not make sense in the context of a unified leadership, because princeling status per se does not help further regime goals. To the extent that CCP elites were divided and were tempted to draw on princeling resources, we expect the following.

Hypothesis F2. *Children of senior officials should be ranked higher than other officials.*

DATA

We test our hypotheses on data measuring elite ranks within the CCP. Our dependent variable is the rank ordering of officials in the upper echelon of the Chinese Communist Party, for the 12th through 16th Party Congresses (1982–2007), a difficult quantity to measure. This (still imperfect) metric of pecking order improves on existing measures, which limit analysis to provincial officials. In this section we outline what we do know about these ranks; in the next, we explain how to use ranks we know well to infer ranks we observe imprecisely, a useful strategy for exploring the distribution of political power in authoritarian regimes.

The ranking of CCP elites in the CC took place roughly once every five years at the National Party Congress. Formally, this ranking comprised three different processes, but fundamentally, all three steps reflect a single ranking highly coordinated by the PSC. Starting at the 12th Party Congress in 1982, delegates received ballots with more names than there were seats in the Central Committee. Delegates then voted for the candidates by placing checks next to their names, and delegates could check as many names as there were seats in the Central Committee. Candidates were accepted into the CC in the order of their vote totals, with the lowest-vote receipients eliminated (Organizational Division of the Central Organization Department 2001). Alternate members of the CC were also elected in this way. The elected CC then voted the Politburo, the PSC, and the party secretary general into office. In this final round of voting, there were as many candidates as there were seats, and CC members without exception voted everyone on the ballot into offce.

Although congressional delegates ostensibly voted freely, all the party congresses were tightly monitored and controlled by the party secretary general and members of the Politburo. To begin, the chairman or the party secretary general sent early signals to the rest of the political elite on who should or should not be on the list of CC candidates (Li 2007). As the congresses approached, candidates for the CC, the ACC, and the Politburo were chosen by leadership groups, which included the serving PSC and a few powerful retired cadres handpicked by the party secretary general (Organizational Division of the Central Organization Department 2001).

The selected candidates then underwent a vetting procedure carried out by the Central Organization Department, which also took instructions from the party secretary general and other members of the Politburo (Cui 2003; Nathan and Gilley 2002). When the delegates finally arrived in Beijing to vote, they were isolated from each other and received strict voting instructions from the party secretary general and other members of the Politburo on how to vote (Deng 2005; Li 2007). In the famous case of Deng Liqun's surprising elimination from the 13th CC, it turns out that the party secretary general at the time, Zhao Ziyang, sent cadres to the provinces prior to the congress to instruct delegates not to vote for him (Deng 2005).

Because of extensive intervention from PSC members, where one landed on the party pecking order largely reflected the priorities of the top leadership, which could change dramatically over time. Periodic appearance of "helicopters," or officials who obtained rapid promotion from the ACC to the Politburo, further suggests that elite ranking at the Party Congress was a single exercise of elite reshuffling closely guided by the party secretary general and the Politburo, rather than three autonomous processes. At the end of a congress, highly ranked officials typically enjoyed much more formal and informal power for the subsequent five years, whereas those who were not selected even as ACC members missed being in the power center of the CCP (Kung and Chen 2011).

We note that there is broad agreement on a set of tiers, with the PSC and the Politburo at the top, followed by the full members of the CC, and then the alternate members of the CC (Kung and Chen 2011; Lieberthal 2004). In particular, we treat as top-ranked the party secretary general (who sets the agenda in the PSC) and the chairman of the Central Military Commission (CMC), who were usually the same person. Next came the members of the PSC, who voted on every major issue confronting the regime—and whose relative political status was implied by the order of their names in public announcements.[2] Following in descending order of political power are the vice chairmen and members of the CMC (who controlled large segments of the armed forces), the Politburo (composed of regional party secretaries of major provinces, several vice premiers of the State Council, and senior military leaders), and a handful of alternate members of the Politburo. Next come the vast majority of the Central Committee who were not members of the higher party organs, whose members' individual ranks were not precisely known, beyond the general importance of these tiers.

Finally, a key to our ranking strategy lies at the bottom of this hierarchy. Beneath full members of the CC lie the ACC members, who failed to win election to higher tiers at the party congress; for example, ACC members tended to be provincial governors rather than the more powerful regional party secretaries. Starting with the Eighth Party Congress, the official press announced ACC members in the order of votes received

[2] For example, at the August 1966 11th Plenum, Liu Shaoqi fell from number 2 in the PSC to the last position in the PSC in the *People's Daily*. See MacFarquhar and Schoenhals (2006).

from delegates (the vote totals themselves remained secret). Because detailed instructions were given to delegates when they voted on the ACC slate, the votes received by ACC members should reflect the priorities of the top leadership in ranking cadres. This underexploited data on a large body of individuals contains a wealth of information about the factors influencing the ranking of all top officials. We will show in the next section how these data help us impute ranks for the entire hierarchy.

We combine our rank data with covariates measuring the characteristics of the members of the ACC, the CC, the Politburo, and the PSC. Most of these variables are drawn from a new biographical data set of Central Committee members developed by Shih, Shan, and Liu (2008). This database contains all CC and ACC members from the first party congress in 1921 to the sixteenth party congress in 2002, and follows the structure for political biographical data developed by Adolph (n.d.) in his work on the career trajectories of central bankers. Shih, Shan, and Liu collect and record nearly every state, party, and military position held by CC members throughout their careers, rather than just positions held by CC members at the time they served in the CC. Using start and end years for each position held, they reconstruct complete career histories for each member of the ACC, the CC, the Politburo, and the PSC as they stood at the start of each party congress.[3] They also collect demographic variables including birth year, gender, year of induction into the party, education, ethnicity, and princeling status.

These biographical data allow us to test whether education, performance, or factional affiliations are responsible for party rank. Basic demographic information captures merit criteria such as level of education, as well as representation criteria such as gender and minority status. We assume that factional ties occur between officials and leaders with shared birthplaces, as well as officials and leaders who overlapped in educational institutions or work units (Lieberthal and Oksenberg 1988, 156).[4] If any such coincidence occur between a CC or ACC member and a top leader, we code that member as having a factional tie to that leader.

We also combine the CC database with provincial economic data to calculate the relative economic performance of provincial administrators (governors and party secretaries) who were also CC or ACC members (China Data Online 2005). In the five party congresses under consideration, provincial administrators made up between 14 and 20% of all CC members. We consider the fraction of provincial CC members to be both large from a substantive point of view, and sufficient to estimate whether provincial economic performance was rewarded with party advancement. Although we are only able to test the economic performance hypothesis on the provincial administrators in the pool, Maskin, Qian, and Xu (2000) argue that China's success hinged on competition among provincial administrators, whose economic performance could be more readily observed than that of cadres in the central bureaucracy. If superior performance by provincial administrators did not lead to political rewards in the party, we doubt that nonprovincial officials received such rewards either.

We focus mainly on growth in GDP and fiscal revenue, but collect 22 other measures of performance for use in robustness analyses. We assume that each member of the CC hierarchy who was also a provincial party secretary or governor is judged based on the performance of his province in the five years between party congresses. In constructing our performance measures, we must solve two further problems. First, because competition for party rank is zero-sum, only officials who outperform their peers should advance. Thus we create *relative* performance scores that subtract rest-of-China performance from each province's growth rate. Second, provincial officials should not receive credit for growth trends determined before they took office. Thus, we subtract preappointment expected performance from each province's growth rate. The resulting variable measures the performance improvement in each province under its current leadership, relative to other provincial leaders. We create performance scores of this type for GDP growth, fiscal revenue growth, and other indicators of provincial performance. For example, the province of an official with a GDP growth score of +2 beat preappointment growth expectations by two percent *more* than the average province in China. Members of the ACC or CC who were not regional administrators in the five years prior to a party congress receive neutral performance scores of zero.[5]

Estimating expected provincial performance is also the key to dealing with selection bias. Although high-flying officials might manuever themselves into provinces with strong growth prospects, or be sent to turn problem provinces around, we can purge these and other selection effects from our performance measures if we accurately model expectations of provincial performance on the eve of appointment. Party leaders doling out performance rewards share our problem of isolating the innovations in growth due to provincial leaders, rather than provincial trends or China-wide shocks. Because we cannot be certain how historical leaders solved this problem, we propose three strategies, and check whether any of them provide evidence of yardstick competition. The first strategy is the simplest: We subtract the growth rate in the preappointment year from growth during an official's

[3] To accurately capture CC members' careers, Shih, Shan, and Liu (2008) assign a four-digit number to each position in the CCP bureaucracy from 1949 to 2002. The first three digits denote the political organ to which this position belongs, and the last digit represents the level of the position. The score 3021, for example, breaks down to 302, which stands for the General Political Department of the People's Liberation Army (PLA), and the final digit, 1, which denotes the highest level in that department, the Department Chief.

[4] For overlapping work experience, we identify an official as being in a leader's faction if the official worked in the same work unit as the leader for over a year, *and* was within two administrative steps of him.

[5] Our results are not sensitive to the assignment of an arbitrary score to nonadministrators: A robustness check shows the same results if we also include in the model a dummy variable for officials with no observed regional economic performance.

tenure, attributing the change to the official's efforts. We use this performance measure in our baseline models. As an alternative, we employ standard time series methods to forecast growth over officials' tenures using only preappointment data. We create forecasts using both the workhorse AR(1) model and the AIC-minimizing ARMA(p, q) model. We use these sharper estimates of expected growth in robustness checks that allow both sophisticated judgment of performance by party leaders and complex patterns of strategic appointment of provincial officials.[6] See the supplemental Online Appendix (available at http://www.journals.cambridge.org/psr2012004) for further details.

METHODS

Modeling the ranking of members of the CCP Central Committee—or any other partially observed ranking of political actors, whether the members of a legislature, a bureaucracy, or some other organization—challenges political scientists' inferential toolkit. Despite the omnipresence of hierarchy in politics, these problem have not yet been noted or solved.

Rank Data Problems

Political rank data pose three problems for quantitative-analysis-as-usual: Rank data are interdependent, require context to interpret, and are typically incomplete.

Interdependence. Virtually all regression models in political science assume that observations are identically and independently distributed (iid). Rank observations are intrinsically interdependent: Only one member can rank first, only one can rank second, and so on. Direct inference on ranks using conventional methods will thus be invalid, giving incorrect standard errors.

Context. In general, observed and counterfactual ranks are meaningful only in context. Without context, the "first-ranked swimmer" could mean anything; the "first-ranked swimmer in the Olympics" and the "first-ranked swimmer in my neighborhood" usually represent very different levels of ability. Context matters even when we know the underlying strength of individuals giving rise to their ranking: The ranking boost provided by an increase in latent strength depends on the competition posed by higher-ranked individuals.

Contextual ranks complicate interpretation of estimated regression relationships. Frequentist inference (e.g., the use of t-tests and p-values to reject null hypotheses) assumes that estimated relationships are representative of a broader pattern that could be replicated in other random samples. The collection of these imagined datasets forms a "superpopulation" from which we construct confidence intervals and significance tests. Rank data constrain us to a finite population perspective: What we learn about the impact of a covariate on rank in one year's congress tells us little about its effect on rank within a different set of members in another year (Gelman et al. 2003).

Although conventional hypothesis testing makes little sense for a rank data model, Bayesian confidence intervals are still valid, as they do not depend on the existence of any data beyond the sample analyzed. Moreover, the confidence intervals we obtain for our results are the best we will ever get—because there will never again be another 14th Party Congress, there is no possibility of finding "more data" to produce more precise estimates of its hierachical structure.

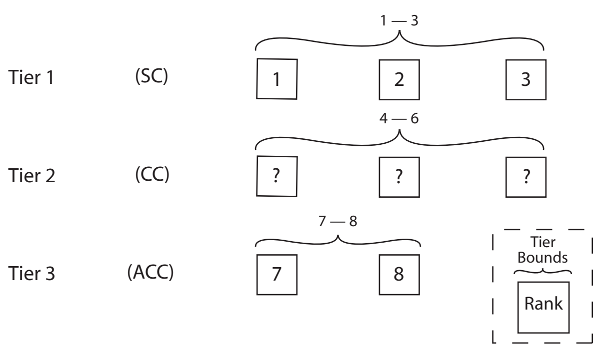
Incomplete Ranks—Ties and Tiers. An ideal rank dataset records a unique rank for each member, but such detailed ranks are seldom available. Rank data may be incomplete, or partially observed, in two ways. First, there may be "ties," defined as cases where the rankers' measurements could not distinguish the (presumably different) latent strength of two or more consecutively-ranked individuals. In an ordered list of ranked objects that omits numerical ranks, such ties may hide anywhere in the dataset, leading to measurment error if rank is treated as identical to order. Our method of constructing ACC ranks is vulnerable to ties, and these potential ties must be accounted in our model.

A broader version of this problem arises when the ranks for a large "tier" of individuals are known to political actors, but concealed from the analyst. Figure 1 illustrates the relationship between tiers and ranks using a hypothetical dataset of eight individuals. Although the ranks of the three highest and two lowest members are known exactly, the fourth through sixth ranks form a tier within which individuals' relative ranks are unknown. Discarding these cases altogether, however, would waste valuable information, because the upper and lower bounds of the tier tell us that these middle individuals rank below 1, 2, and 3 and above 7 and 8. Although we can only specify the exact rank of five out of eight observations, we can correctly identify the higher-ranking individual in 25 of the 28 possible pairwise comparisons. The best analytic strategies for rank data will find ways to include this relative rank information.

The observed ranks of members of the ACC, CC, Politburo, and PSC show essentially the same pattern as the smaller dataset shown in Figure 1. We know the ranks of ACC members and PSC members, but all we can say about the CC is that their ranks lie somewhere in the middle. Surprisingly, this simple observation about CC members unlocks essential information for inferring the relationship between the characteristics of party members and their political power, but we will not be able to examine this hidden trove of data using off-the-shelf quantitative methods.

[6] Even if actual Chinese leaders did not use ARMA models to judge their subordinates' performance, experience in macroeconomic forecasting suggests that these models perform at least as well as more data- and knowledge-intensive methods often used in the 1960s–1980s, and may thus serve as shortcuts to estimating how sophisticated political actors of that period may have formed their expectations (Diebold 2000).

FIGURE 1. Observed Ranks, Missing Ranks, and Tiers



Note: In this hypothetical dataset, the ranks of the top three individuals (who occupy the top tier, or the PSC) and the ranks of the bottom three individuals (who occupy the lowest tier, or the ACC) are known precisely. The ranks of the middle three individuals are unknown, but still bounded by the rank of their tier, the CC.

Flawed Analytic Strategies

Three seemingly appropriate methods from the political science toolkit—linear regression, censored regression, and ordered probit—either assume properties that rank data clearly violate or ignore useful information contained in the ranks. Even though ranks take on unique and interdependent integer values, least-squares regression inappropriately assumes that ranks are iid Normal, computes incorrect standard errors, and predicts impossible ranks. Because linear regression requires us to listwise delete any observations with imprecisely observed ranks, we also lose information captured by the bounds on those ranks, which is statistically inefficient and a possible source of bias (King et al. 2001).

Censored regression (or tobit) models a Normally distributed outcome that is observed when it falls in certain ranges, but censored elsewhere (Schnedler 2005). This avoids bias and inefficiency from deleting partially observed cases, but because censored regression also assumes ranks are iid Normal, its standard errors are still biased. And neither censored nor linear regression protects against measurement error from miscoding tied ranks.

If we abandoned our analysis of the rank-level data, we might focus on estimating the probability of an individual appearing in a given tier using ordered probit. Ordered probit does not assume ranks are Normal, and mitigates (but does not eliminate) the dependence of observations. But it exacts a large cost for these improvements, throwing out even more of the observed data than linear regression, and adding a new parameter to estimate for each uniquely identified tier, so that estimation becomes harder the more we know. To analyze partial ranks without violating model assumptions or discarding data, we look beyond the usual suspects, and develop statistical models calibrated to the unusual properties of rank data.

A Bayesian Model of Partially Observed Ranks

To understand the relative power of Chinese Communist officials, we propose and apply a Bayesian model of partially observed ranks with three advantages over conventional methods. First, we allow interdependence of ranks through the rank likelihood (Hoff 2008). Second, we generalize this approach to allow for partial observation of ranks, preserving all the information available in our data—a step we expect will be critical for most applications of the method to political data. Finally, we interpret our results in the context of each rank dataset: Rather than assume there is a "superpopulation" from which each party congress is a random sample, Bayesian methods let us treat each party congress as sui generis, thus quantifying the magnitude and uncertainty of effects of covariates on ranks in historical context.

We denote the rank of each individual $i \in 1, \dots, n$ as a unique integer $y_i \in 1, \dots, n$. Lower values of y_i represent *higher* ranks; i.e., $y = 1$ indicates the highest-ranked individual, and $y = n$ the lowest. Each individual lies within a tier, or range of ranks, which is known even if y_i is missing. Our model of y_i rests on three assumptions:

Assumption 1. *Each ranked member has a unique latent strength* y_i^*.

Assumption 2. *Higher latent strength entails better expected rank:* $y_i^* > y_j^* \Leftrightarrow {}_i$ outranks ${}_j \Leftrightarrow y_i < y_j$.

Assumption 3. *Latent strengths are iid Normal.*

To examine the relationship between ranks and observable covariates, we parameterize the latent strength y_i^* using a linear model, which for identification has no constant and unit variance:

$$
\begin{aligned}
y_i^* &\sim \text{Normal}(\mu_i, 1) \\
\mu_i &= \beta_1 x_{1i} + \beta_2 x_{2i} + \dots \beta_p x_{pi}.
\end{aligned} \tag{1}
$$

For convenience, we refer to the random component of latent strength as $\alpha_i = y_i^* - \mu_i$; we interpret α_i as representing the unmeasured political talent and good or bad fortune of official i.

The model relies on the notion that if we could only observe latent strength directly, a simple linear regression would be sufficient to understand its conditional expectation. We link partially observed ranks to latent strengths through a set of bounds around each rank, denoted $\{y_i^{\text{lower}}, y_i^{\text{upper}}\}$. For a given observation, there are three possible kinds of bounds. From most to least informative, these are

Bound Type 1. *Rank known exactly:* $y_i^{\text{lower}} = y_i = y_i^{\text{upper}}$.

Bound Type 2. *Rank known up to a potential tie with an adjacent ranked individual:* $y_i^{\text{lower}} = y_i^{\text{upper}} + d$, *where* $d + 1$ *indicates the number of ranks included in the tie.*

TABLE 1. Goodness of Fit

	Party Congress				
	12th	13th	14th	15th	16th
All observations (N)	410	285	320	342	356
Fully observed (N_{full})	141	114	135	157	166
Partially observed (N_{part})	269	171	185	185	190
Completely unobserved (N_{miss})	0	0	0	0	0
Percent fully observed ($100 \times N_{\text{full}}/N$)	34.4	40.0	42.2	45.9	46.6
Percent of variance explained	29.5	46.5	40.4	47.6	34.7
Percent prediction error of known ranks ($\sqrt{\text{MSE}}$)	31.1	26.6	26.4	25.1	25.5
Percent of tiers correctly predicted	65.6	66.3	75.3	69.6	71.6
Bayesian information criterion	532.3	447.6	403.2	468.0	396.0

Setting $d = 1$ discounts the observed difference in rank between any two immediately adjacent ranked individuals as potential measurement error, but assumes that two individuals separated by two ranks or more must have correspondingly ordered latent strengths.

Bound Type 3. *Rank known up to a tier:* $y_i^{\text{lower}} < y_i < y_i^{\text{upper}}$*, where* y_i^{lower} *is set to the lower bound of the tier, and* y_i^{upper} *is set to the upper bounds of the tier.*

In our application, we treat the ranks of (most) members of the PSC as known exactly (Type 1), members of the CC as known only within the tier bounds of the CC (Type 3), and members of the ACC as known up to a potential tie, as a check on the possibility that two adjacently ranked members of the ACC actually won the same number of votes (Type 2).[7]

With bounds in hand for each ranked individual, we form a rank likelihood model around Assumptions 1, 2, and 3. Rank likelihoods capture the probability, conditional on covariates, that a given individual falls between the next higher and next lower ranked observations, and make no distributional assumptions about the ranks themselves (Hoff 2008; Pettitt 1982). Rank likelihood models do not require an independence assumption, accept even partially observed ranks, and require sophisticated Markov chain Monte Carlo (MCMC) methods to estimate. Full details regarding model parameterization, priors, and estimation procedures can be found in the Appendix.

Estimating and Fitting the Model

We estimated the model on the members of each party congress *separately*, without any pooling of parameter values across years, to allow the nature of political competition in each Party Congress to vary freely.[8] Each model controls for contemporaneous faction affiliations, relative changes in economic and revenue performance, educational attainment, gender, minority status, age, and party tenure. We report the goodness of fit for our baseline specifications in Table 1.[9] First, we compute the percentage of variance in latent political strength explained by the measured covariates (analogous to R^2 in linear regression). Second, we present the average error in percentiles when the fully observed ranks are predicted using only the measured covariates. Third, we calculate the percentage of all individuals classified in the correct tier based on their observed characteristics. Finally, we report the Bayesian information criterion (BIC) for each model (Spiegelhalter et al. 2002). As the BIC weighs the explanatory benefit of additional covariates against the cost of added model complexity, it serves as our preferred tie-breaker among substantively similar models, and it supported the set of controls employed here. Even though a majority of ranks were observed only partially in each party congress, the models fit well, explaining at least one-third and as much as one-half of the variance in latent strength, correctly predicting tiers for at least two-thirds of officials, and predicting members with exactly known ranks with as little as 25% error. Nevertheless, there remains a substantial chunk of variance left over, suggesting significant opportunities for further research.

Interpretation of Results

As usual for a regression model, substantive interest centers on calculating conditional expectations and first differences for carefully chosen counterfactual values of the model covariates. Because the rank benefit of any covariate depends not only on a given individual's

[7] The analyses reported in this paper assume $d = 1$; runs with higher d yielded substantively identical results and slightly wider confidence intervals.

[8] We leave the problem of temporal interdependence in party ranks to future work. A time-series version of the rank-data model would make more efficient use of the available data, but would also need to deal with a number of thorny issues, including the fact that the membership of elite bodies changes in each period.

[9] For each estimated model, we ran three parallel MCMC chains, discarding the first 5,000 iterations, and sampling every 10th iteration thereafter through 5,000 more iterations. All diagnostics suggest rapid convergence: Chains mixed well after the burn-in period, and $\hat{R}$ values were generally below 1.1.

characteristics, but also on the competition, we must also supply the hypothetical rivals for our hypothetical ranked individual. Usually, it makes sense to insert our hypothetical individual into a historical ranked body. As an example of a properly formed conditional expectation, we might ask what rank to expect for an otherwise average official with a college degree in the 15th Party Congress. A proper first difference, on the other hand, would ask how many percentiles in rank an average official would gain in the 15th Party Congress by earning a college degree. The Appendix gives further details on calculating these quantities.

FINDINGS

Overall, our findings do not suggest any linkage between growth performance of regional administrators who were also ACC, CC, or Politburo members and their *party* ranking in the CCP. However, some regime-strengthening factors, such as education and, to a lesser extent, fiscal performance and minority status, had positive impacts on ranking in the party elite. At the same time, there is considerable evidence that factional affiliation played a large role in political advancement.

Because the raw parameters estimated in our models are on a latent scale of no direct interest, and because we estimate five separate models, each with numerous covariates, it is easiest to explore our results through graphical summaries. These graphics show either the expected ranks of members with differing characteristics (for the 16th Party Congress see Figure 2; for other years, see the supplemental Online Appendix), or the expected change in rank associated with a change in one covariate, all else equal (Figures 3 and 4). To guide readers through our results, we first explore how different factors affected expected elite ranking at the most recent party congress for which we had data, the 16th Party Congress (2002). Then we take a broader view, and examine how the impact of these factors has waxed or waned between the 12th Party Congress and the 16th. Finally, we consider a broad range of alternative specifications to confirm the robustness of our findings.

Getting Ahead at the 16th Party Congress (2002–07)

Figure 2 lists the correlates of CCP rank in the 16th Party Congress in order from the biggest hinderances to the greatest advantages. The dotted vertical line marks the expected rank of a member with average values of all covariates. Black circles show the expected rank of members with the characteristics listed at left. The thin horizontal lines give 95 percent Bayesian confidence intervals; the thicker horizontal lines are single standard error bars. Also plotted to the left and right of the main estimates are gray diamonds showing the expected rank of members with the given characteristics *and* a random effect (or unmeasured political ability) one standard deviation below or above the mean, respectively. Thus the right gray diamond shows the rank we expect for a particularly talented or fortunate politician who has a high value of the listed covariate and otherwise average *observed* characteristics. These gray diamonds show the magnitude of the unmeasured effects not captured by the model, and also reveal how our modeled covariates provide a crucial boost or barrier for even extremely powerful elites—note, for example, the plight of skilled or lucky politicians who lack an education (banished to the ACC), or extremely talented or fortunate observationally average older members (potential Politburo material).

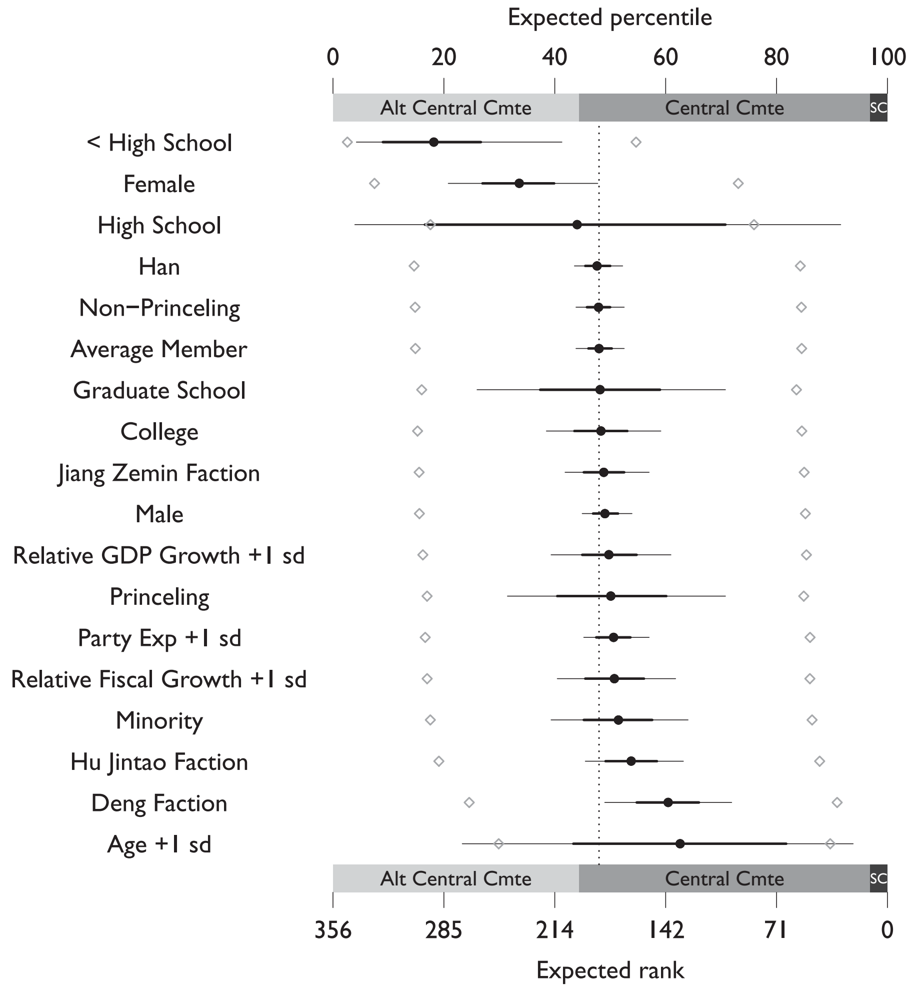
So how do our hypotheses fare in the 16th Party Congress? We start with our meritocratic regime hypotheses, which held that education, growth, revenue performance, and experience should be favored, along with broad representation of women and ethnic minorities. On face, education had a strong effect, as college graduates and graduate degree holders outranked high school graduates and dropouts by as much as 25 percentiles in party rank. Of course, by the 16th Party Congress, education was such a necessary condition for advancement that only 2% of members lacked a college degree. Despite a scramble to obtain graduate degrees among Chinese officials, having a graduate degree did not provide any additional advantage for an official's ranking at the 16th Party Congress.

Contra Maskin, Qian, and Xu (2000), Li, and Zhou (2005), and Chen, Li and Zhou (2005), we find little effect of growth performance on the prospects of local administrators who were ACC or CC members. At the 16th Party Congress, and controlling for revenue growth and political connections, relative provincial GDP growth provided no advantage to the expected ranking of an official. In this same party congress, collecting more taxes relative to the other provinces provided only a tiny, statistically uncertain boost to one's ranking [95% CI: −6.6, 11.5].

We find contrasting results for women and ethnic minorities. At the 16th Party Congress, a woman with otherwise average characteristics was expected to rank over 15 percentile points [95% CI: −1.3, −29.8] below a similar man. Even an exceptionally talented or fortunate woman with a high random effect was expected to rank more than a decile behind a similarly exceptional male official. Not surprisingly, in the 16th Party Congress only 4 of over 100 full CC members were women. Minorities, in contrast, appear to suffer no disadvantage, and may even have seen a slight benefit (3.9 percentiles) from a deliberate policy of promotion, though this effect was far from precisely estimated [95% CI: −9.8, 17.3]. A skilled or lucky minority official with a high random effect could reasonably expect to rise nearly to the 90th percentile, as did Hui Liangyu, an ethnic Hui in the Politburo.

Finally, years of experience in the CCP seemed to give elites an advantage in obtaining a higher rank. Here, we distinguish the effect of experience within the party from that of age itself. The model's estimates for the effect of age are ambiguous: No other variable had such a large average effect, but neither did any

FIGURE 2. Formal Selection Criteria and Rank in the 16th Party Congress: Estimated Relationships



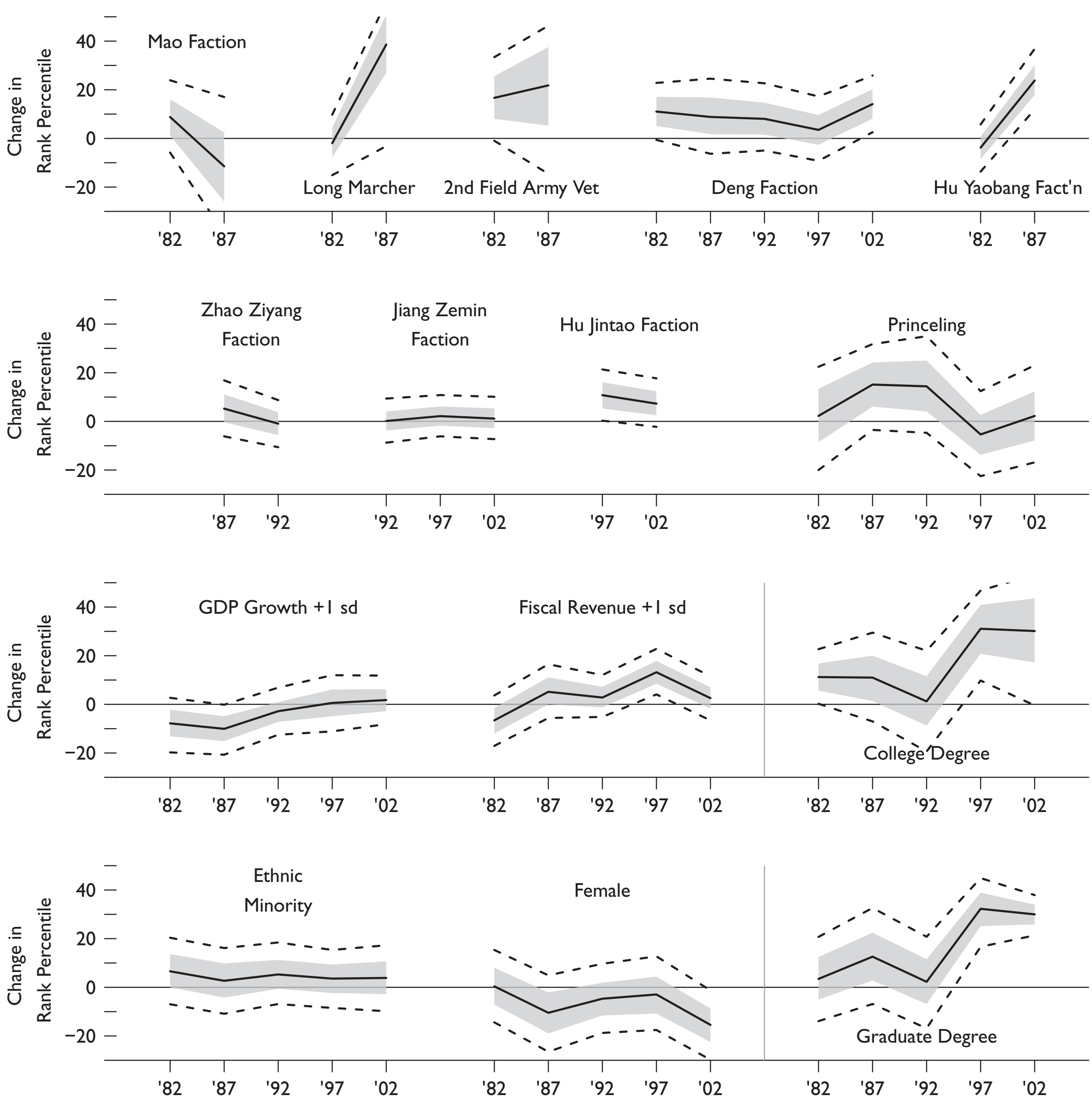
Note: For each row of the plot, we set the indicated covariate at one (for binary covariates) or the mean plus one standard deviation (for continuous covariates), and hold all other variables at their means. The black circles indicate expected rank for an individual with these observed covariates and an average random effect (or degree of unmeasured political ability). Gray diamonds show expected ranks for members with random effects one standard deviation above or below the mean. Thick horizontal lines mark 67% Bayesian confidence intervals and thin horizontal lines indicate 95% Bayesian confidence intervals. The dotted vertical line shows the expected rank of a member with the average covariates. The light gray region indicates the actual ranks of ACC members, the medium gray region shows the CC, and the dark gray region the ranks of the Standing Committee.

other variable have so imprecisely estimated an effect (indeed, no matter which specification we chose, the confidence interval for age remained stubbornly wide). First, age exerted the greatest positive impact on the expected rank of an official at the 16th Party Congress. Even controlling for age, however, we find that experience, measured as the share of an official's adult life spent in the party, also exerted a positive effect on one's ranking, albeit also with a wide confidence interval.

Did elite conflict and factionalism play a role in influencing elite ranking? For the 16th Party Congress, we examine the effect of being in the same faction as departing Party Secretary Jiang Zemin, incoming Party Secretary Hu Jintao, and deceased veteran Deng Xiaoping. Even in 2002, five years after Deng's passing, members of Deng's faction—some 12% of the CC and ACC who still had historical ties with Deng—had average ranks 14 percentile points higher [95% CI: 2.5, 25.8] than the average member, whereas members of Hu Jintao's Communist Youth League faction were expected to rank about seven percentiles above the average member [95% CI: −2.3, 17.7]. Surprisingly, CC and ACC members with ties to Jiang Zemin ranked the same as the average member, a pattern held over from the 14th and 15th Party Congresses. This puzzle can be explained, in part, by Jiang's vigorous promotion of close allies and supporters possessing little administrative experience or personal political resources. Even with Jiang's assistance, the best these individuals could do was to rank at the bottom among ACC members.[10]

[10] Some famous examples include Jiang's secretary Jia Ting'an and his bodyguard You Xigui, who ranked at the bottom of the ACC list. See Zhang (2002).

FIGURE 3. The Effects of Factional Ties, Performance, Demography, and Education on Political Rank: Estimated First Differences over Time



Note: Each plot shows the estimated change in rank percentile for a member with otherwise average measured and unmeasured characteristics, but a higher value for the listed covariate. For performance measures, we estimate the impact on rank of performance 1 standard deviation above the mean. For education variables, we show the difference in rank associated with a switch from less than high school to the listed degree. All other covariates are binary, so we estimate the effect on rank of switching from a 0 to 1. Shaded regions show 67% Bayesian confidence intervals, and dashed lines mark 95% Bayesian confidence intervals.

Finally, holding their educational advantages constant, princelings at the 16th Party Congress differed little from other Central Committee members [+2.2 rank percentiles; 95%: −16.9, 23.0]. All else being equal, the princeling advantage was not apparent at the 16th Party Congress.

In examining ranking at the 16th Party Congress, we find that economic performance did not matter. Obtaining a college education had become an unambiguous prerequisite for membership in the CC elite by 2002. Accumulating more experience in the CCP likewise may have provided officials with a boost in their ranking. However, performance-based criteria such as tax collection exerted only modestly positive effects on one's ranking. Departing from the expectations of a broadly representative regime, women faced distinct disadvantages in seeking high rank, although minorities enjoyed a slight advantage. Meanwhile, factional affiliation with Hu Jintao and Deng Xiaoping allowed connected officials who were average in other respects to rank significantly higher than their peers. In this core indicator, factional considerations continued to matter in 2002.

Getting Ahead in the Reform Era: Changes over Time

To understand how the struggle for power in the CCP has evolved over the reform period, we review our results for the 12th through 16th Party Congresses, covariate by covariate. We take up the results in three parts, first examining the performance and merit hypotheses, then focusing on how factional or princeling ties emerged as determinants of political rank, and concluding with a closer look at the interaction effects of age and party experience.

We start, in Figure 3, with the effects of education. These plots reveal that even at the beginning of the reform era, a college or graduate degree boosted party rank. At the 13th Party Congress in 1987, education continued to exert a positive effect on ranking. However, educational level beyond high school did not affect ranking at the 1992 14th Party Congress. In the aftermath of the 1989 Tiananmen Square Massacre, the party was preoccupied with punishing those who had failed to take a hard line and rewarding those who had helped the party survive the crisis. Over half of the CC was replaced at the 14th Party Congress (Gilley 1998). The benefits of education grew dramatically after 1992. By 2002, college was an unambiguous prerequisite for entry into the top of the elite, setting off a scramble for degrees that appears to have reached a point of diminishing returns, as graduate education produced no added advantage beyond a college degree at the 15th and 16th Party Congresses. All one can say is that the education advantage became more certain when one obtained a graduate degree relative to an official who only completed college, as shown by the narrower confidence interval for graduate degree holders in recent years.

Members of ethnic minorities held a steady but slight and imprecisely estimated advantage throughout the reform period, perhaps because of the party's "united front" policies, which sought to maintain stability and legitimacy through minority representation in the CCP elite (Mackerras 2003, 21). Women faced no disadvantage versus men at the 12th Party Congress in 1982, but were systematically lower-ranked for most of the remaining party congresses. What explains the relatively fair treatment of women at the 12th Party Congress and its subsequent decline? Perhaps female Long March veterans, still politically active at the 12th Party Congress, worked to promote women in the ACC and CC. In 1982 Deng Yinchao (Zhou Enlai's wife), Deng Jinliu, Cai Chang, Li Zheng, and Kang Keqing all exerted considerable influence on CC ranks, either through the Central Advisory Commission or from behind the scenes. By the late 1980s and early 1990s, these veterans had retired from active politics, leaving few senior women officials to lobby for the promotion of new female cadres. In the 1990s, only a small handful of "strong women" emerged in the political scene, such as Chen Muhua and Wu Yi, but they were the exceptions to an increasingly male-dominated elite body. This trend suggests that fairness and representation considerations were low priorities in the ranking of high-level officials in the CCP.

Turning to performance measures, we find growth performance exerted a decisively negative influence on CC ranking from the beginning of the reform period until the 15th Party Congress in 1997. From 1982 to 1992, provincial officials with above average growth performance could expect to be ranked 3 to 10 percentiles *lower* than average performers. From 1997 onwards, officials from high-growth regions enjoyed neither an advantage nor a disadvantage vis-à-vis officials from average-growth regions. Relying on more detailed, party-based ranks, which do not treat lateral bureaucratic rotations as promotion, we thus cast doubt on the notion that regional growth was propelled by the prospect of promotion (Blanchard and Schleifer 2000), and on the findings of Li and Zhou (2005), Maskin, Qian, and Xu (2000), and Chen, Li, and Zhou (2005).

Indeed, the historical record suggests that party secretaries of provinces deemed politically important were automatically inducted into the Politburo, regardless of the province's economic performance. For example, at the 16th Party Congress, party secretaries from economic dynamos such as Guangdong and Shanghai won seats in the Politburo, but party secretaries of mediocre growth performers such as Hubei and Tianjin also gained entry into the body. At the same time, the serving party secretaries of Zhejiang and Jiangsu, which typically had high growth, were never inducted into the Politburo. Our findings also lend support to Huang (1996), who finds that highly ranked regional leaders had an incentive to stifle growth through reducing investment because they did not want to violate the center's macroeconomic control measures.

The picture for fiscal performance is more nuanced. In contrast to economic performance, which benefits society as a whole, tax collection delivers funds directly to Beijing, helping political leaders fulfill various objectives. In 1994, Beijing centralized China's fiscal system, and we see some benefit in rank from exceptional revenue collection over the period 1992–97 during the 15th Party Congress in 1997. As Beijing struggled to ensure that local officials complied with the new fiscal system (Chen 2005), these new incentives were unsurprising. The need to reward local officials for tax collection seemed to have diminished somewhat by the 16th Party Congress in 2002.

Our findings on the effect of factional ties and princeling relationships throughout the reform period suggest that top Chinese leaders consistently sought to promote faction members already in the political elite to higher ranks. Although members of Chairman Mao's residual faction at first were not discriminated against in elite ranking, by the 13th Party Congress, after the demotion of Mao's designated successor Hua Guofeng, Maoists saw their standing fall by an average of 11.5 percentiles compared with non-Maoists, though the effect is very poorly estimated [95% CI: –39.0, 17.0]. Many Maoists were forced out of the CC and ACC in the 13th Party Congress, and their representation in the CC and ACC declined from 16% to 6% (see the Appendix).

ACC and CC members with historical ties to Deng Xiaoping enjoyed positive and substantial rank advantages through most of the reform period, even through the 15th Party Congress, immediately following Deng's death. This effect holds even controlling for a potential confounder, Deng's command of the Second Field Army during the Chinese Civil War (1945–1949), which produced many senior officials. Even when we partial out Deng allies from the Second Field Army, his other followers did well at the 12th and 13th Party Congress.

The two party secretaries of the 1980s, Hu Yaobang and Zhao Ziyang, exerted different effects on the ranks of faction followers. Hu Yaobang followers made up some 41% of the 12th Party Congress, which may explain why being a Hu follower did not produce extra rank advantages in 1982. At the 13th Party Congress in 1987, some of Hu's followers were forced to step down (like Hu before them), reducing the faction to 25% of the CC and ACC. The survivors enjoyed a 20% rank advantage over non-Hu Yaobang followers, perhaps as a result of a selection effect: With the weakest Hu followers weeded out, the stronger survivors may have been better suited to retain their rank.

In contrast, although Zhao followers made up roughly one-fourth of the CC and ACC body at both the 13th and 14th Party Congresses, they held no particular rank advantage in either. Jiang followers, who made up 17% and 20% of the CC and ACC at the 15th PC and 16th Party Congresses, are similarly neutrally ranked. This appears to contradict tales of Jiang forcefully filling the PSC and Central Committee with his own followers (Lam 1994, 1999; Zhang 2002). But just as Jiang tried to promote many of his key followers, including Huang Ju, Zeng Qinghong, and Jia Qinglin, into key positions in the Politburo Standing Committee, he also tried to appoint members of his household staff with little administrative experience into alternate central committee seats. These Jiang supporters were consistently among the lowest vote recipients on the ACC list. Indeed, we see Jiang's faction clustered into two types—one at the top of the elite rank hierarchy and one at the bottom. The net effect of a Jiang factional tie as zero is thus somewhat misleading. Without Jiang's help, many in his inner circle would not have been qualified to even enter the bottom of the Communist elite, so his influence arguably raised the rank of both clusters.

Factionalism persists in recent Chinese politics. We find that members of Hu Jintao's Communist Youth League (CYL) faction enjoyed a distinct rank advantage of 7 to 10 percentiles at both the 15th and 16th Party Congresses, all else equal. To be sure, Youth League members were cultivated in the 1980s to take high offices in the 1990s, and many of them indeed entered the elite CC/ACC bodies in the late 1990s. However, once they entered the ACC, ties with Hu Jintao, who had served as the party secretary of the Communist Youth League, pulled them further upward into the CC and higher.[11]

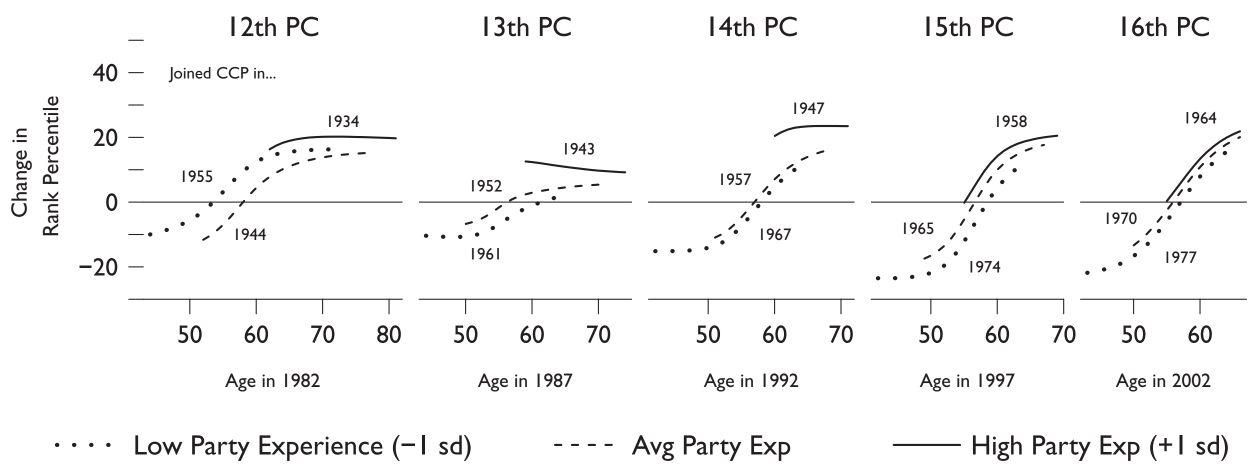
Finally, children of senior officials, or "princelings," held substantial rank advantages at the 13th and 14th Party Congress, all else equal, though the effect has declined since the 15th PC. Although in many cases the parents of these princelings have retired or died, political leaders in China still found it useful to promote these princelings over the average member. The princeling advantage stood out particularly in the 13th and 14th Congress, when Deng Pufang, Yu Zhengsheng, Li Peng, and Xi Jinping were propelled into the upper reaches of the CC or even into the Politburo over revolutionary veterans despite being notably young. From the late 1990s on, however, relatively young Communist Youth Leaguers were also getting promoted into the higher ranks, and the princeling advantage no longer seemed as strong. Nevertheless, future study of the 17th Party Congress may well find a revival of the princeling advantage, as the cohort of princelings who had entered high politics in the late 1980s and early 1990s reached the apex of the CCP.

We turn now to the important but complex interaction of each official's age and party tenure, which we control for using both a simple measure of the years an official spent in the CCP as a fraction of his total years lived, as well as dummy variables for specific revolution cohorts.[12] In each of our models, age—not party tenure—is the more potent determinant of rank. However, even though our estimates partial out the effects of age and tenure, in real data these concepts can never be fully separated (e.g., in 2002, there were no 40-year-olds who joined the CCP in the 1930s). To show how age and tenure reinforce one another, we calculate from the model posterior the expected rank for a range of observed combinations of age and date of party entry; we plot the results by party congress in Figure 4. The graphs trace out "cohort lines" showing the effect of age on rank for officials who joined the party in the same year; for each party congress, we plot a cohort line for members at the 25th, 50th, and 75th percentiles of party tenure. The upward slope of the lines reflects the strong, positive association between age and political rank and the degree of spread between the lines shows the effect of party experience.

[11] We only identify CYL members as Hu followers if they worked within two administrative steps of Hu Jintao during his career.

[12] We thus dummy out the pre-Long March (–1935), Anti-Japanese War (1936–1945), and Civil War (1946–1949) cohorts separately, as suggested by improved BIC scores.

FIGURE 4. The Interactive Effect of Age and Party Tenure on Rank by Party Congress: Estimated First Differences



Note: Plots display the estimated change in rank percentile associated with switching from the average age to the age shown on the horizontal axis. A separate line traces out the effects of age for early, average, and late CCP joiners. All other covariates are held at their means. To avoid extrapolation, each line is limited to show only counterfactuals drawn from inside the convex hull (King and Zeng 2006).

Two main findings emerge from Figure 4. First, the rank benefits of age are strong—indeed, stronger than any other measured covariate—and have grown stronger in recent party congresses. To guard against selection bias (as only unusually powerful officials can maintain their positions in the CC past the mandatory retirement age of 65), we reran each model with a control for the retirement threshold, and the overall age effect persisted strongly. We conclude, then, that age mostly reflects the accumulation of political capital over time.

Second, the rank benefits of party tenure and revolutionary experience loom large in early reform party congresses, but diminished greatly over time. At the 12th Party Congress, CC members from the Long March cohort on average placed above the 60th percentile, much higher than the expected ranking of the later Anti-Japanese War and Civil War cohorts. Unique efforts in the 12th Party Congress to promote young officials meant that some members of the post-1950 cohort ranked higher than revolutionary veterans and even some Long Marchers. In all other party Congresses, greater party seniority brought about higher ranks. At the 13th and 14th Party Congresses, revolutionary veterans on average ranked around the 60th percentile, whereas at the 15th Party Congress, revolutionary veterans ranked around the 75th percentile, but by 2002, all revolutionaries had retired from the CC, and party experience diminished as a determinant of rank.

The overall impression of Figure 4 is that the significance of age has risen whereas the impact of years in the party has faded. Nevertheless, their large combined effect suggests an "up-or-out" system for the CCP elite, as officials who endured years of intraparty political struggle were very likely to accumulate the political experience and capital needed to advance through the party ranks.

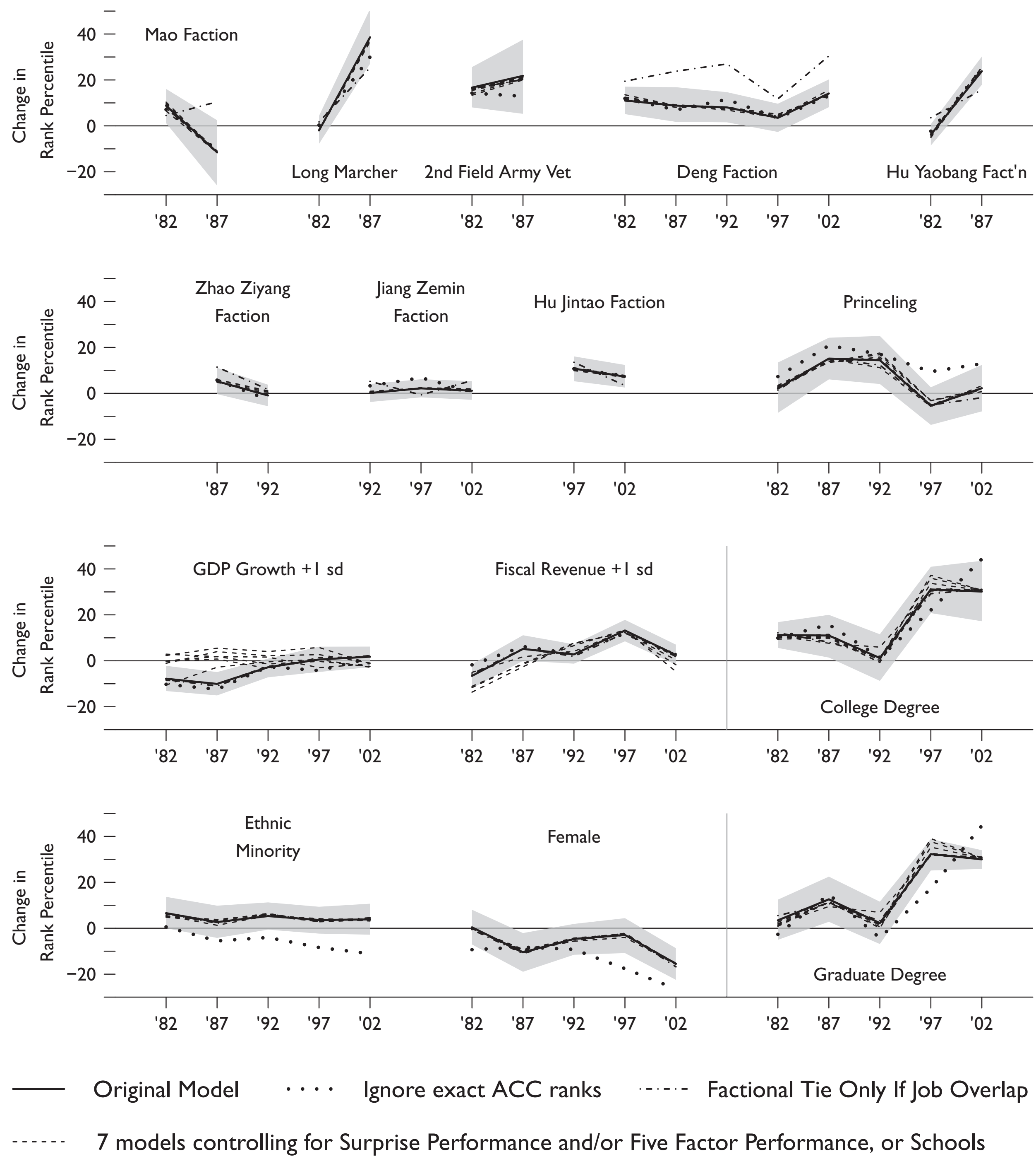
Robustness of Results

We find no relationship between growth performance and party ranking, and a strong relationship between factional ties and rank. Because these findings are debated both for China and for the broader field of authoritarian politics, we conduct additional robustness tests to raise our confidence in the results. In particular, we drop the assumption that the order of votes received for the ACC measures political strength, consider more specific measures of factional affiliation to protect against measurement error, add controls for educational institutions and fields of study, add broader measures of economic performance in case political ranks are awarded holistically, and finally, exploit our provincial performance data to mitigate selection bias. Because the details of these tests could fill a second article, we relegate discussion of theory, measurement, design, and ancillary results to the supplemental Online Appendix. Here, we focus on the consistency of our estimated results for the impact of faction, performance, and education on expected party rank.

Figure 5 displays results for all robustness checks overlapped. The similarity of these results to each other and the baseline estimates is striking, and the few deviations worthy of note.

Our first robustness check is the toughest, because it discards much potentially useful information. We now treat the ranks of ACC members as missing, and allow the rank data model to impute ACC ranks along with CC ranks and other parameters. Remarkably, there is enough information in the tier bounds on the ACC and CC to estimate our models, and for every party

FIGURE 5. The Effects of Factional Ties, Performance, Demography, and Education on Political Rank: Robustness Checks



Note: As in Figure 3, each plot shows the estimated change in rank percentile for a member with otherwise average measured and unmeasured characteristics, but a higher value for the listed covariate. Estimates from nine different specifications join the original model. Shaded regions show 67% Bayesian confidence intervals for the original models. See the supplemental Online Appendix (available at http://www.journals.cambridge.org/psr2012004) for confidence intervals for other models.

congress, the new, more conservative results agree remarkably well with the baseline models on the effects of factions, performance, and education (see the dotted lines in Figure 5). Our main conclusions no longer depend on whether ACC votes are informative, as they do not enter the model. The principal change in the results is that minorities and women both fare poorly, which we take as evidence of a glass ceiling. It is apparently easier for women and minorities to rise in the ACC ranks than it is for them to leave the ACC for higher tiers.

Our factional tie measures are also vulnerable to measurement error. Designed to be sensitive to affiliations hinted at by shared birthplaces, school ties, and shared work environments, our baseline measures may include false positives because of coincidences of birth and school attendance. A more specific measure counts only cases where an official worked closely with the faction leader in one of China's hundreds of ministerial and vice-ministerial work units or state-owned enterprises. Substituting these measures leaves most estimates of faction unchanged, and strengthens results for Mao and Deng, perhaps by zeroing in on core faction members or by excluding marginal members (see the dashed–dotted lines in Figure 5). As a further check against spurious factional results, we confirm that our factional measures also explain considerably more variance in rank than randomly generated factional variables do (see the supplemental Online Appendix for details).

As our remaining robustness checks all agree so closely with each other and with the original model as to be mostly indistinguishable, Figure 5 plots them all as dashed lines. The first such test allows for the possibility that not all college and graduate degrees are equal, and adds to the model controls for the origin of each official's highest university degree (whether from the Central Party School or abroad) and for fields of study (either natural science or social science/administration/law). The average effect of education is unchanged, as is the impact of faction and performance covariates, whereas the effects of degree origins and fields of study vary over time (see the supplemental Online Appendix).

We perform two tests of our findings on performance. In the first, we consider the possibility that GDP growth is too narrow a yardstick, and substitute a set of five orthogonal factors of performance summarizing 24 provincial performance variables. For this model, the quantity plotted under "GDP Growth" in Figure 5 is the effect on rank of the largest factor from our factor analysis, which loads heavily on growth in industrial output, retail sales, gross capital investment, and, of course, GDP itself. Even with this broader measure, we still find no evidence of incentives for growth. Nor do we see any correlation between higher party rank and improvement in the other factors: urban and rural standards of living, social service provision, and employment (see the supplemental Online Appendix for factor analysis details and full results).

In our final test, we return to the problem of selection bias. Our baseline measures of GDP growth provide some protection against selection effects by subtracting preappointment growth rates in each province. Still, better preappointment forecasts—such as AR(1) or ARMA(p, q) forecasts from historical data—should offer more protection against selection bias, and more closely capture the kind of performance most relevant for political rewards. In our remaining robustness checks, also shown as dashed lines in Figure 5, we isolate the surprising performance of provincial officials left over after removing expected growth, as forecast from preappointment data by either an AR(1) model or by the best available ARMA(p, q) model. We use the same forecasting techniques to isolate surprising performance on the five latent performance factors. Across all four of these robustness checks, the rewards to unexpected economic growth hover near zero, whereas the rank effects of factional ties, education, and demographics remain unchanged.

We consider the breadth and consistency of these results to be a strong refutation of the widespread claim that Chinese leaders advance based on yardstick competition. There is no hint that higher provincial growth leads to political advancement: not for officials who perform well compared to the past performance of their own province, not for officials who beat their peers' performance in other provinces, not for officials who perform well on a broad range of factors, and not even for officials who beat the specific expectations one could form of their likely performance on appointment. Factional ties, on the other hand, appear to be strong predictors of party rank no matter what the specification of the model.

CONCLUSIONS

In this study, we use a novel Bayesian model of rank to estimate the impact of covariates on elite ranking within the ruling Chinese Communist Party. Guided by the literature on comparative authoritarianism, we derive hypotheses about covariates that may drive elite ranking in China. The empirical results show that the existing theoretical literature on authoritarian regimes predicts ideal types that, in reality, coexist in the same regime. Communist institutions in China worked well enough to make college education a prerequisite of entry into the upper echelon. The regime also systematically favored ethnic minorities, probably to enhance stability. Finally, when a centralized fiscal system was first implemented in the mid-1990s, the CCP used the cadre evaluation system to ensure that provincial leaders cooperated with central tax officials in maximizing revenue for the central government. Consistent with the predictions of Bueno de Mesquita et al. (2003), the CCP regime deployed the cadre evaluation system to ensure basic state functions, stability, and fiscal income, all of which were necessary to deliver private goods to the ruling elite.

Chinese leaders, however, did not apply the cadre management system to encourage growth. To be sure, the system assigned scores to cadres' growth performance, but we find no evidence that provincial officials who generated higher-than-average growth or higher than expected growth were rewarded with higher party ranks in any year and on any measure. Given our findings, one can no longer argue that China enjoyed spectacular growth *because* of promotion incentives embedded in the political system. To be sure, officials had some incentive to be educated, which increased human capital in the regime. After the beginning of the reform, provincial administrators certainly did not have an incentive to reduce growth in their jurisdictions and may even have seen growth performance as a way to earn promotion. However, there were other (formal and informal) paths upward that were more effective. In terms of formal performance criteria, raising revenue collection, at least in the 1990s, earned advancement. Chinese leaders also did not promote gender fairness at the elite level through the cadre management system, thus allowing systematic discrimination against women to persist.

CCP cadre management institutions also delivered promotions to followers of senior party leaders. First, ties with Deng Xiaoping, Hu Yaobang, or Hu Jintao while they were in power elevated officials in the party hierarchy. Even though these leaders were the nominal and de facto heads of the party, they still engaged in factional politics. In fact, the literature suggests that leaders were able to stay in power *because* they engaged in factional politics instead of selecting the most capable officials (Egorov and Sonin 2011; MacFarquhar and Schoenhals 2006; Pye 1992). Princelings also held distinct advantages in some party congresses, again suggesting that senior CCP leaders favored promoting members of their support coalitions.

China's growth in the past 30 years has been spectacular, but the precise source of this growth remains highly contested. The findings of this paper suggest that the CCP's cadre management system did not directly lead to China's growth. The findings, however, are consistent with the argument that crucial policies enacted in the 1980s, including the household responsibility system, labor mobility, price reform, and the welcoming of foreign direct investment, paved a strong foundation for three decades of growth (Huang 2008; Naughton 1996; Sachs et al. 1994; Shirk 1993). Although Chinese leaders enacted them on an experimental basis, these early policies produced strong momentum for growth that lasted for decades. Furthermore, by encouraging cadres to deliver short-term benefits to the regime, party institutions delivered several crucial conditions for economic growth, including relatively low inflation, a well-financed central authority, and political stability (Huang 1996; Shih 2008a). Without these basic conditions, growth in China likely would have been much lower. The challenge for China going forward is to uncover institutional mechanisms that counteract the natural tendency of political institutions to deliver private goods in the form of rent-seeking opportunities to the support coalitions of senior leaders.

In future research, instead of static evaluation of regimes as "meritocratic" or "clientelistic," scholars should focus on mechanisms that shift a regime's priorities toward public good provision, rather than delivering private goods to a small elite. Exogenous shocks, such as inter-state wars, worldwide depression, and exogenously driven inflation, may change the relative balance of power and degree of elite rivalry, which in turn may alter promotion incentives, and ultimately affect policy outcomes. Outside events may also shift the policy priority of top leaders, leading to adjustments in promotion criteria. For example, the collapse of the Soviet Union motivated China to centralize its fiscal system. Our findings suggest that when faced with the challenge of bolstering central fiscal resources, the leadership placed a heavier emphasis on tax collection when ranking officials at the 15th Party Congress. Following the research of Ramseyer and Rosenbluth (1998) and Boix and Svolik (2010), more work should inquire into the expected behavior of authoritarian regimes under different degrees and types of internal rivalry induced by external shocks.

APPENDIX

This Appendix provides details for the estimation and interpretation of the Bayesian model of partially observed rank data introduced in the main text.

Markov Chain Monte Carlo Rank Likelihood Estimation

Rank likelihood estimation is sometimes used to avoid the strong distributional assumptions used in maximum likelihood models, and is especially useful for inference in models where the response is only known up to a rank (Pettitt 1982). Whereas the maximum likelihood estimate of $\boldsymbol{\beta}$ maximizes the probability of obtaining the observed response y_i given its assumed probability distribution and the covariates, the rank likelihood estimate maximizes the probability that the observed response falls between the next higher and next lower ranked individuals, given the covariates, but without any assumption that y_i follows a particular distribution (Hoff 2008). Rank likelihood allows us to directly include (even partial) rank information in the likelihood, but requires Markov chain Monte Carlo (MCMC) methods to estimate.

As usual with MCMC, our goal is to initiate a random walk through the parameter space which eventually converges to the correct posterior distribution, so that we can simply sample from the Markov chain to calculate estimates and confidence intervals of our quantities of interest (Gelman and Hill 2007). MCMC estimation of the Bayesian partial rank model proceeds using a combination of the Gibbs sampler and Metropolis–Hastings algorithms. We set diffuse Normal priors over the $\boldsymbol{\beta}$ parameters and then construct several Markov chains using the following four-step procedure, iterating over steps 3 and 4 m times to produce each chain:[13]

[13] Using priors over $\boldsymbol{\beta}$ is an optional step that makes the model Bayesian, but is not strictly required to apply the rank likelihood approach. Including diffuse priors made no substantive difference in our results for the party congress data.

1. ***Initialize the latent strengths***, y^*_{i0}**:** Draw, for all members of the party congress, a random feasible rank. A set of feasible ranks must respect all tier bounds and tie restrictions, and use each possible rank only once.
2. ***Initialize*** $\boldsymbol{\beta}_0$**:** Using the starting values y^*_{i0} and the covariate data $\mathbf{x}_i$, compute the starting values of the parameters by least squares. Then compute $\mu_{i0} = \mathbf{x}_i\boldsymbol{\beta}_0$.
3. ***Update the latent strengths***, y^*_{im}**:** Using the $\boldsymbol{\mu}_{m-1}$'s from the previous iteration, for a randomly chosen i, draw a new latent strength y^*_{im} somewhere above the highest latent strength of individuals known to rank below i, but below the lowest latent strength of individuals known to outrank i. Continue drawing until each individual has an updated latent strength.

   Formally, draw y^*_{im} within $[\ell^*_{im}, u^*_{im}]$, where

$$\begin{aligned}&\text{lowest known higher latent strength, } u^*_{im}\\ &\quad = \min\left(y^*_{km} : y^{\text{upper}}_{im} < y^{\text{lower}}_{km}\right)\\ &\text{highest known lower latent strength, } \ell^*_{im}\\ &\quad = \max\left(y^*_{jm} : y^{\text{upper}}_{jm} < y^{\text{lower}}_{im}\right).\end{aligned} \quad \textbf{(2)}$$

   By default, draw each new y^*_{im} using the Gibbs sampler. To perform a single Gibbs draw from y^*_{im}, sample a single probability w_{im} from the interval implied by the tier bounds,

$$w_{im} \sim \text{Uniform}\left(\int_{-\infty}^{\ell^*_{im}} \text{Normal}\left(\mu_{i,m-1}, 1\right), \int_{-\infty}^{u^*_{im}} \text{Normal}\left(\mu_{i,m-1}, 1\right)\right). \quad \textbf{(3)}$$

   Then y^*_{im} is simply the quantile of the standard Normal distribution corresponding to w_{im}.

   The Gibbs sampler can run into computational difficulties when computing Normal cumulative distribution functions if, for a given observation i and MCMC iteration m, the expected value of the distribution of latent strengths $\mu_{i,m-1}$ lies far outside the bounds $\{\ell^*_{im}, u^*_{im}\}$. In those cases, we employ instead the slower Metropolis–Hastings algorithm (of which Gibbs is a special case), for which we need only draw new candidate values of y^*_{im} from the appropriate proposal distribution:

$$y^*_{im} \text{ candidate } \sim \text{Truncated Normal}\left(\mu_{i,m-1}, \sigma^2, \ell^*_{im}, u^*_{im}\right). \quad \textbf{(4)}$$

   As usual, candidates for y^*_{im} replace the older values $y^*_{i,m-1}$ based on a Metropolis–Hastings acceptance probability. To speed up convergence, tune σ^2 to achieve an acceptance rate close to 40% (Gelman et al., 2003).
4. ***Update*** $\boldsymbol{\beta}_m$**:** Using the updated latent strengths y^*_{im} and the covariate data $\mathbf{x}_i$, compute the updated parameters by least squares. Then compute $\mu_{im} = \mathbf{x}_i\boldsymbol{\beta}_m$.

After an intial burn-in, iterate the Markov chains until all y^*_i's and β's appear to have converged to stable distributions, and then sample the posterior distributions of the latent stengths and parameters from the chains. For convenience, one can also sample the posterior distribution of each individual's unmeasured component of latent strength, α_i.

Interpretation of Results

We interpret the model using conditional expectations and first differences for counterfactual scenarios as recommended by King, Tomz, and Wittenberg (2000), with the important caveat that we must also condition on the historical context against which any counterfactual individual is ranked:

Conditional Expectations. Conditional expectations in a rank model must condition not only on the hypothetical characteristics $\mathbf{x}_c$ of a new member c, but also on the characteristics $\mathbf{x}_i$ of all the observed members in the party congress the new member might outrank.[14] To calculate these conditional expected ranks, we draw sets of the parameters $\tilde{\boldsymbol{\beta}}$ from the converged MCMC chains. For each draw h, we calculate a single conditional expectation of the new member's rank,

$$\mathrm{E}\left(\tilde{y}_{ch}|\mathbf{x}_c, \mathbf{x}_i, \tilde{\boldsymbol{\beta}}_h, \tilde{\alpha}_{ih}\right) = \text{rank}\left(\mathbf{x}_c\tilde{\boldsymbol{\beta}}_h; \mathbf{x}_i\tilde{\boldsymbol{\beta}}_h + \tilde{\alpha}_{ih}\right), \quad \textbf{(5)}$$

where $\text{rank}(a; b)$ indicates the rank of object a within the set b.[15] We summarize the posterior distribution of $\mathrm{E}\left(y_c|\mathbf{x}_c, \mathbf{x}_i, \tilde{\boldsymbol{\beta}}, \tilde{\alpha}_i\right)$ using the mean and 95% interval of our h draws. Finally, to facilitate comparison across party congresses, we transform the expected ranks to a percentile scale.[16]

First Differences. As in linear regression models, we are interested in how changing a covariate from $\mathbf{x}^{\text{old}}_c$ to $\mathbf{x}^{\text{new}}_c$ shifts the expected rank for a hypothetical individual. Although this "first difference" is simply β for linear regression, in the Bayesian partial rank model, as in many non-linear models, we must calculate it from the model parameters. To calculate a first difference for the Bayesian partial rank model, we draw a vector of $\tilde{\boldsymbol{\beta}}$'s from the model posterior, and subtract the rank for the baseline scenario from the rank for the new scenario:

$$\begin{aligned}&\mathrm{E}\left(\tilde{y}^{\text{new}}_{ch} - \tilde{y}^{\text{old}}_{ch}|\mathbf{x}^{\text{new}}_c, \mathbf{x}^{\text{old}}_c, \mathbf{x}_i, \tilde{\boldsymbol{\beta}}_h, \tilde{\alpha}_{ih}\right)\\ &\quad = \text{rank}\left(\mathbf{x}^{\text{new}}_c\tilde{\boldsymbol{\beta}}_h; \mathbf{x}_i\tilde{\boldsymbol{\beta}}_h + \tilde{\alpha}_{ih}\right) - \text{rank}\left(\mathbf{x}^{\text{old}}_c\tilde{\boldsymbol{\beta}}_h; \mathbf{x}_i\tilde{\boldsymbol{\beta}}_h + \tilde{\alpha}_{ih}\right).\end{aligned} \quad \textbf{(6)}$$

As before, we summarize the distribution of first differences with their mean and 95% confidence interval, and convert these to a percentile scale for ease of comparison across years and covariates.

Implementation

A complete suite of tools for inference and interpretation of the Bayesian partial rank model is available in the R package partialrank.[17] Monte Carlo experiments across

[14] If $\mathbf{x}_c$ is set to the value for an observed individual, this procedure calculates a fitted value for that observation.

[15] We could instead produce predicted ranks—counterfactuals that reflect not only the uncertainty in our estimates of $\boldsymbol{\beta}$, but also the uncertainty introduced by letting the hypothetical member c have a random degree of unmeasured strength α_c—by calculating

$$\text{Predicted}\left(\tilde{y}_{ch}|\mathbf{x}_c, \mathbf{x}_i, \tilde{\boldsymbol{\beta}}_h, \tilde{\alpha}_{ch}, \tilde{\alpha}_{ih}\right) = \text{rank}\left(\mathbf{x}_c\tilde{\boldsymbol{\beta}}_h + \tilde{\alpha}_{ch}; \mathbf{x}_i\tilde{\boldsymbol{\beta}}_h + \tilde{\alpha}_{ih}\right),$$

where $\tilde{\alpha}_{ch}$ is a randomly chosen $\tilde{\alpha}_{ih}$. The same algorithm can be used, mutatis mutandis, to create predicted first differences.

[16] Because low-rank numbers correspond to high percentiles, and vice versa, we calculate expected rank percentiles as $100 \times (n - \mathrm{E}(y_c|\mathbf{x}_c, \mathbf{x}_i, \tilde{\boldsymbol{\beta}}, \tilde{\alpha}_i))/n$.

[17] Available at faculty.washington.edu/cadolph/software.

a range of hypothetical rank data sets of varied size and degrees of partial observation show that the model meets or exceeds the performance of linear regression, censored regression, and ordered probit in terms of bias, mean squared error, and coverage of confidence intervals (Adolph 2011).

TABLE 2. Summary Statistics for Model Covariates

	Party Congress				
	12th	13th	14th	15th	16th
Female	0.06	0.07	0.07	0.07	0.07
Minority	0.09	0.11	0.11	0.11	0.10
Age at party congress	59.59	56.15	56.90	56.60	56.09
	(8.03)	(6.55)	(6.17)	(5.78)	(5.32)
Party experience	0.66	0.62	0.60	0.56	0.56
	(0.10)	(0.11)	(0.13)	(0.12)	(0.1)
Year joined party	1942.13	1951.60	1957.32	1964.78	1970.52
	(9.44)	(8.97)	(9.90)	(9.08)	(6.97)
CCP since 1935[a]	0.22	—	—	—	—
CCP since 1945[a]	0.42	0.23	0.10	—	—
CCP since 1949[a]	0.13	0.26	0.19	0.05	—
Less than high school	0.17	0.06	0.03	0.01	0.01
High school	0.24	0.15	0.10	0.04	0.01
College	0.55	0.71	0.77	0.78	0.69
Graduate degree	0.04	0.08	0.11	0.17	0.29
Mao faction	0.16	0.06	—	—	—
Long Marcher	0.17	0.01	—	—	—
2nd Field Army veteran	0.06	0.01	—	—	—
Chen Yun faction	0.26	—	—	—	—
Ye Jianying faction	0.23	—	—	—	—
Deng faction	0.27	0.14	0.12	0.11	0.12
Hu Yaobang faction	0.41	0.25	—	—	—
Zhao Ziyang faction	—	0.28	0.26	—	—
Jiang Zemin faction	—	—	0.32	0.28	0.25
Hu Jintao faction	—	—	—	0.17	0.20
Princeling	0.02	0.05	0.04	0.04	0.04
Relative GDP growth[b]	−1.52	−2.12	−0.92	0.03	0.02
	(4.8)	(5.83)	(7.34)	(3.6)	(2.41)
Relative revenue growth[b]	15.82	28.12	13.92	6.14	0.91
	(9.44)	(164.59)	(73.55)	(46.18)	(7.13)

Note: Entries are the observed means for each covariate in each model, by party congress. For continuous variables, the standard deviation is included in parentheses. [a]CCP since 1935, CCP since 1945, and CCP since 1949 are coded 1 for members who joined the CCP before 1935, between 1935 and 1945, and between 1945 and 1949, respectively. However, if CCP by 1935 is omitted from the model, CCP by 1945 then includes all members who joined before 1945, and so on. [b]Relative GDP growth and Relative revenue growth are measured as the difference between the average annual percent growth in GDP (or revenue) in the province governed by the individual and the corresponding population-weighted average annual percent growth in GDP (or revenue) across all other provinces. The GDP and revenue figures in this table reflect means and averages for members who were provincial governors or party secretaries only.

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Web Appendix to Shih, Adolph, and Liu (2012),

"Getting Ahead in the Communist Party: Explaining the Advancement of Central Committee Members in China",
American Political Science Review, 106:1.

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Robustness of Results in Shih, Adolph, and Liu (2012),
"Getting Ahead in the Communist Party: Explaining the Advancement of Central Committee Members in China", *American Political Science Review*, 106:1.

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This appendix collects the rationales, methods, and results behind the robustness checks summarized in "Robustness of Results" section of our article. We investigate whether:

- our results require ACC elections to be informative of ACC and CC political ranks;
- our results are robust to more detailed controls for educational achievement;
- more specific measures of factional ties uncover the same factional effects;
- our factional results can be falsified under appropriate conditions;
- broader measures of performance reveal evidence of yardstick competition;
- selection bias distorts our findings on performance; and whether
- endogeneity of performance to rank would affect our conclusions.

We specify a model to avoid each potential problem in turn, keeping all other elements of our modeling strategy in place. For each robustness check, we re-estimate our partial rank models for every Party Congress and present results for all covariates in the same format as our original model in Figure 3. The main text collects all these results in Figure 5. This appendix allows the reader to isolate each robustness comparison and confirm the similarity of confidence intervals across the many models considered. Comparison of these figures across the appendix reveals a strongly coherent pattern across our robustness checks: we can relax the debatable assumptions of our model one by one, yet the results for each covariate remain substantively unchanged. In every case, factions matter for political success, while provincial performance does not.

A.1 Results do not depend on inclusion of ACC rank data

In the baseline models presented in our article, we assume the process which produces the observed ranks within the ACC is the same one which produces the unobserved ranks within the CC. Specifically, we assume votes in ACC elections proxy political rank for ACC members and that ranks in both bodies are determined by latent strengths generated in a similar way by observable covariates like economic performance, faction, education, and demographics. An obvious objection to our approach arises if one suspects the process of rank determination in the ACC is substantially different from the process generating CC ranks, or if one doubts ACC votes proxy for political strength.

As a robustness check against these objections, we treat *both* the CC and ACC ranks as missing, and reestimate our models using only the tiers of CC and ACC members. This offers a test of our assumption that ACC ranks are meaningful and measured by votes, and an alternative set of results for skeptics. We now include precise ranks only for members of the Standing Committee; otherwise, the model relies solely on the rank information conveyed by the tiers (that is, we merely assume members of the Central Committee outrank Alternates). Although the precise rank of most members is now treated as only partially observed, the tier bounds may by themselves provide sufficient information to confirm the pattern of relationships between latent strength and our covariates. Indeed, even with this more limited information data, we still obtain quick convergence, with $\hat{R} < 1.1$ for all parameters after 10,000 MCMC iterations; as before, the first 5,000 are discarded as a burn-in period.

This robustness check should be difficult, as we discard much of the information we believe is present in our outcome variable. Remarkably, re-estimating the model without informative ACC ranks produces substantively similar results for almost every covariate across all five Party Congresses (Figure A1). Belonging to the right factions still suggests a meaningfully political rank, and strong economic performance still earns no boost in political rank.

The only difference in our results lies in the expected ranks of ethnic minorities and women, and is not entirely surprising. While the model estimated on ACC rank data found a small and imprecisely estimated rank advantage of membership in an ethnic minority, the tiers-only model finds a somewhat larger *disadvantage*. Yet this apparent contradiction is exactly what one would

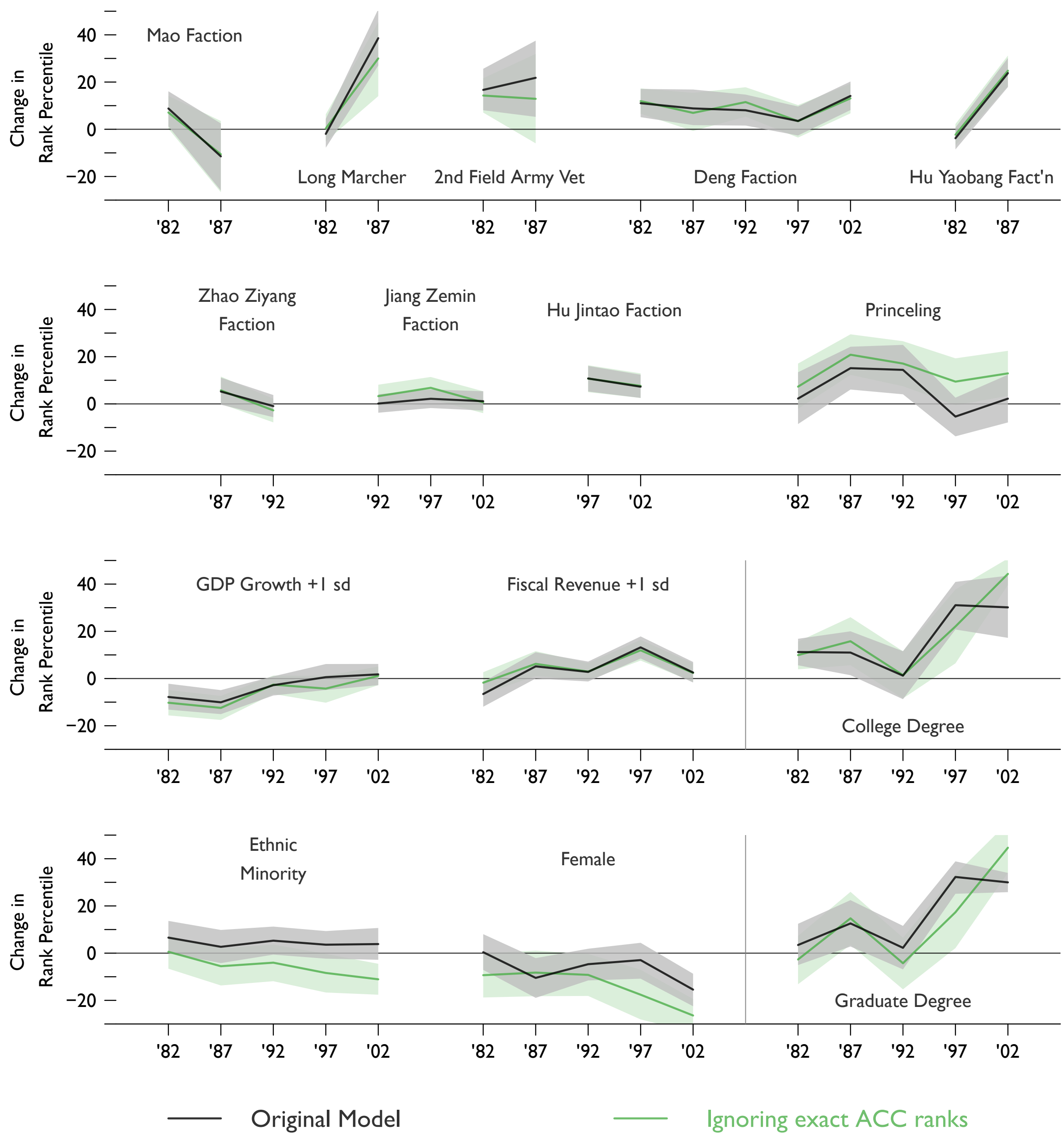

Figure A1: **Robustness of results with ACC ranks omitted.** Each plot shows the estimated change in rank percentile for a member with otherwise average measured and unmeasured characteristics but a higher value for the given covariate. Estimates from the robustness check treating ACC ranks as unknown (in green) join the original model from Figure 3 (in black). Shaded regions show 67% Bayesian confidence intervals.

expect from the combination of a quota for members of minorities and a glass ceiling blocking their ascent to the higher tiers of the Central Committee and Standing Committee.[1] If political principals pick representatives to fill seats designated seats for ethnic minorities, those members are unlikely to rise above the lowest tier (the ACC), but when the vote for ACC membership occurs, they are likely to receive many "automatic" votes to ensure some minorities are present in the ranks, and thus spuriously outrank most other ACC members. These conflicting forces are exactly those needed to violate the assumption that ranking within the ACC is consistent with ranking outside it. Women also seem to face a glass ceiling: across most Party Congresses, women have lower expected ranks based on tiers, suggesting all else equal, women above the ACC may have lower rank than we would expect based on female ACC members.

Yet these two unsurprising exceptions to our common ranks assumption do not change any of our other results, confirming other covariates have more consistent effects across the political hierarchy. Therefore, in the interest of making the most of the available data, and because the results of greatest interest seem unaffected by the assumption of homogenous political strength, we continue to use fully-observed ACC rank data in our remaining robustness checks.

A.2 Results are robust to more detailed education measures

In our baseline models, we find college and graduate degree holders enjoy a large rank advantage in the later reform Party Congresses, although we do not find an added benefit of post-graduate education controlling for college degrees. But are all college or graduate degrees really equal for political purposes? And does controlling for previously omitted variation in degrees alter any of our other conclusions?

For each official with a college or graduate, we create four binary variables indicating either the origin of that official's highest degree or its field. These variables are only partially exclusive, so a single official could be coded as having zero, one, or two of these characteristics, but not

[1] Although the Cultural Revolution saw the purge of many minority cadres, the late Cultural Revolution period saw the induction of a sizable cohort of minority cadres into the party (Dreyer, 1976). Even so, minorities only make up a small share of Communist cadres (6.6 percent in 1990) and a much smaller share of the political elite (Mackerras, 1994). At the 17th Party Congress, only one member of the Politburo, Hui Liangyu, was a minority. This suggests a very narrow channel for minorities to advance in the political system, especially at the highest levels.

more:

Central Party School Almost all officials in our dataset underwent short-term training at the Central Party School (CPS), but only a small share earned college or graduate degrees at CPS. Only these officials are coded as 1's on our CPS measure, and our expectations for them are mixed. The elite networks at the Central Party School (CPS) may offer continuing benefits to cadres' careers. On the other hand, some of these CPS degree-holders may have lacked the ability to gain degrees from other institutions, so CPS degrees may instead proxy lower aptitude and predict poor performance.

US/European Union degrees measures whether an official attended college or graduate school in the United States or European Union. Because China increased engagement with the outside world throughout the 1980s and the 1990s, the advantage in party rank yielded by a degree from the West may have risen over time. On the other hand, the CCP continue to demonstrate a deep distrust of "hostile foreign forces"', so American and European degrees may have a neutral or even negative impact on party rank throughout the period studied.

Science degrees measures whether an official was a science or engineering major in college or in graduate school. After Deng Xiaoping officially made the "Four Modernizations" the official goal of the regime in 1978, a large cohort of Chinese cadres with science and engineering background entered the upper echelon of the regime. This may be a coincidence—many cadres who had studied in the Soviet Union in the 1950s, mostly in the sciences, were being systematically promoted into the upper echelon after the mid 1980s. On the other hand, these degrees may have helped power the rise of these elites.

Social science degrees measures whether an official was a social science, law, administration, business, or education major in college or in graduate school. Over the last decade, official ideology in China focused increasingly on the rule of law and greater social engineering to realize a "harmonious society". This ideological shift may have given cadres with a social science background greater advantage in CC ranking. If true, we would expect a social science background to help cadres climb higher in the CC hierarchy at the 2002 16th Party

Congress, but not before.

We add these four dummy variables to the baseline specifications for each Party Congress, and re-estimate the models to separate out the effects of degree type from the baseline effect of a college or graduate degree. As usual, convergence was quick, with $\hat{R} < 1.1$ for all parameters after 10,000 MCMC iterations, with the first 5,000 discarded as a burn-in.

As Figure A2 shows, the effect of the average college or graduate degree is completely unchanged by controlling for source or subject of degrees, nor does controlling for the degree type alter any conclusions about the effect of faction, performance, or demographics on political rank, though we note that the confidence interval for revenue is now very wide. Graduate degrees still offer no additional gains in rank over college degrees, even after controlling for field of study and degree origin.

Figure A3 shows the *added* effect of having a specific kind of college degree or graduate degree on top of the average effect of have any college or graduate degree. The results are imprecise, and across the board almost always within a standard error of no effect. Degrees from CPS may have had a positive effect on rank in the 15th and 16th Party Congresses, but the confidence interval is too wide to for strong conclusions about the growing power of CPS social networks. We can say that degrees from abroad are never more helpful than domestic degrees, and that in the early 1990s might even have been a disadvantage. Finally, field of study appears largely irrelevant to rank. Science majors may have enjoyed a small benefit in the early 1990s as the cohort of science majors trained in the 1950s was then at its zenith, and social science majors had some advantage in the early 1980s when die-hard Maoist who had majored in Marxism were still in power. These effects are small and fleeting.

A.3 Results are robust to *plausible* alternative measures of faction

Factional ties are both our most theoretically important covariates and the most difficult to measure. Chinese political factions need not advertize their membership to benefit their affiliates, so we must turn to a variety of membership proxies of varying specificity and sensitivity. Here, we put our factional variables to the test, and show that a range of measures with complementary strengths and weaknesses yield consistent results. We also show that our factional measures

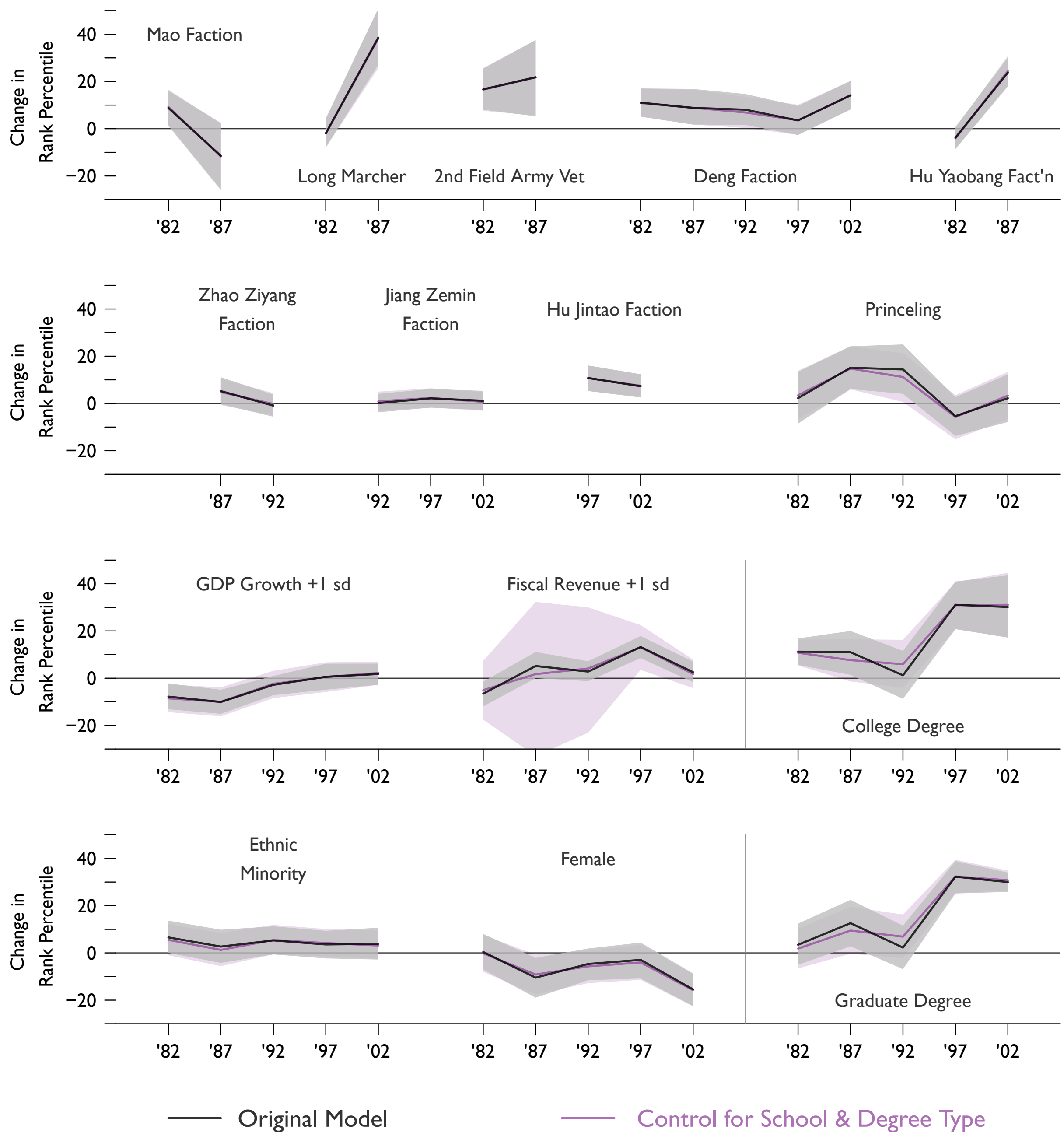

Figure A2: **Robustness of results controlling for degree focus and origin: Average effect of degrees.** Each plot shows the estimated change in rank percentile for a member with otherwise average measured and unmeasured characteristics but a higher value for the given covariate. Estimates from the robustness check controlling for degree focus and origin (in lavender) join the original model from Figure 3 (in black). Shaded regions show 67% Bayesian confidence intervals.

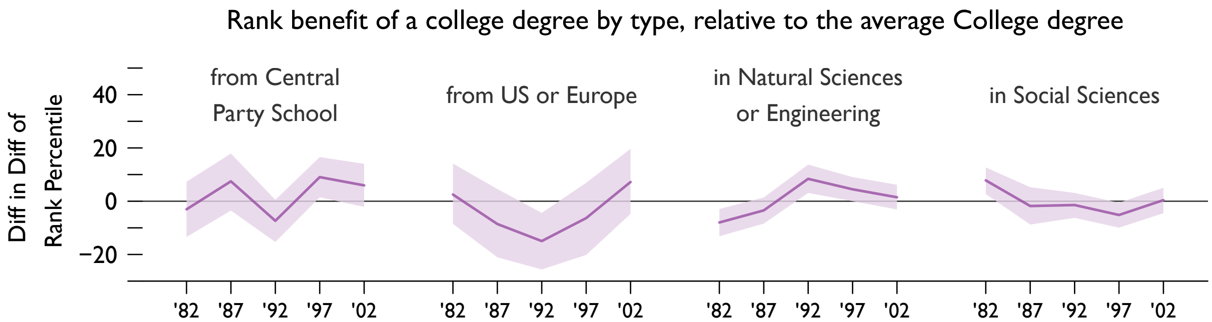

Figure A3: **Additional benefits of advanced degrees by focus and origin.** Each plot shows the additional rank benefit of a college or graduate degree of the listed type on top of the average effect of a college or graduate degree. Shaded regions show 67% Bayesian confidence intervals.

explain politically meaningful variation by showing they outperform purely random factional variables.

A.3.1 Narrow measures of faction predict rank similarly to broader measures

Measures of factional affiliation in Chinese politics are necessarily imprecise. We must make assumptions about which specific measures of factional ties to count, and how to aggregate those measures into a single variable denoting factional membership. Our most important measure of faction is narrowly drawn, indicating whether an official coded once worked in close proximity to the faction leader. Other measures are broader, noting whether an official shares a birthplace or school tie with the faction leader. We experimented with different methods of aggregation, and found little substantive difference between averages, weighted averages, and a binary indicator of whether any factional measure shows a tie. We opted for the binary indicator to sidestep any remaining questions of the appropriate weights to place on factional indicators.

When we aggregate both broad and narrow measures of faction using a binary indicator for any ties, we obtain the "greedy" measure of faction used in the main text. This measure is highly *sensitive*: it is less likely to miss members of a faction than other approaches, but is more likely to falsely include non-members in factions. A complementary strategy, used in this robustness check, recodes the factional tie variables counting only officials who once worked closely with factional leaders. These "job overlap" versions of our factional variables are highly *specific*: they may not catch as many true positives as the greedy faction measures, but should avoid more false positives. Because these approaches have opposite strengths and weaknesses, similar results should greatly diminish worries about factional measurement.

As Figure A4 shows, factional effects persist when we narrow the criteria for evidence of a factional tie.[2] If anything, the effects of ties to Deng may be stronger, while other factional variables see only tiny changes. More specific measures of faction may be more likely to pick out the core members of a faction, and thus may yield stronger factional effects than measures which are so sensitive they include some non-members in their ranks, diluting estimated factional effects. Note also that with more specific measures, the effects of Mao and Long March ties grow approach each other, as narrowing the scope of the Mao factional variable makes these covariates more similar.

Overall, we are unsurprised by these results, and take them as a vote of confidence both for the factional approach and the various proxies employed here. In other robustness checks, we return to using the broader, "greedy" factional measures from the main text, both because we think they are the most informative overall proxies, and to be conservative in our definition of faction to be sure we include the full range of members, not just those at the top.

The non-effect of economic performance is completely unaffected by our choice of factional measures. These variables will need to stand or fall on their own.

A.3.2 Randomizing factional affilliation removes rank effects

We are convinced by the "job overlap" robustness check that our factional measures are meaningful and our results insensitive to arbitrary changes in our measures of faction. But there is such a thing as a result that is "too robust". In an earlier literature on factions in the Chinese military, Parish (1973) critiqued Whitson (1969) for constructing what he saw as a non-falsifiable theory of faction sizes, which treated any stability or change in the factional composition of the military over time as the result of a conscious struggle for power, while failing to account for the possibility that the factional labels themselves were meaningless, and the changing sizes of faction pure statistical noise.

While our study focuses on factional rank rather than size, it would be subject to a similar

[2] As usual, we obtain quick convergence, with $\hat{R} < 1.1$ for all parameters after 10,000 MCMC iterations, and as before, the first 5,000 iterations are discarded as a burn-in period.

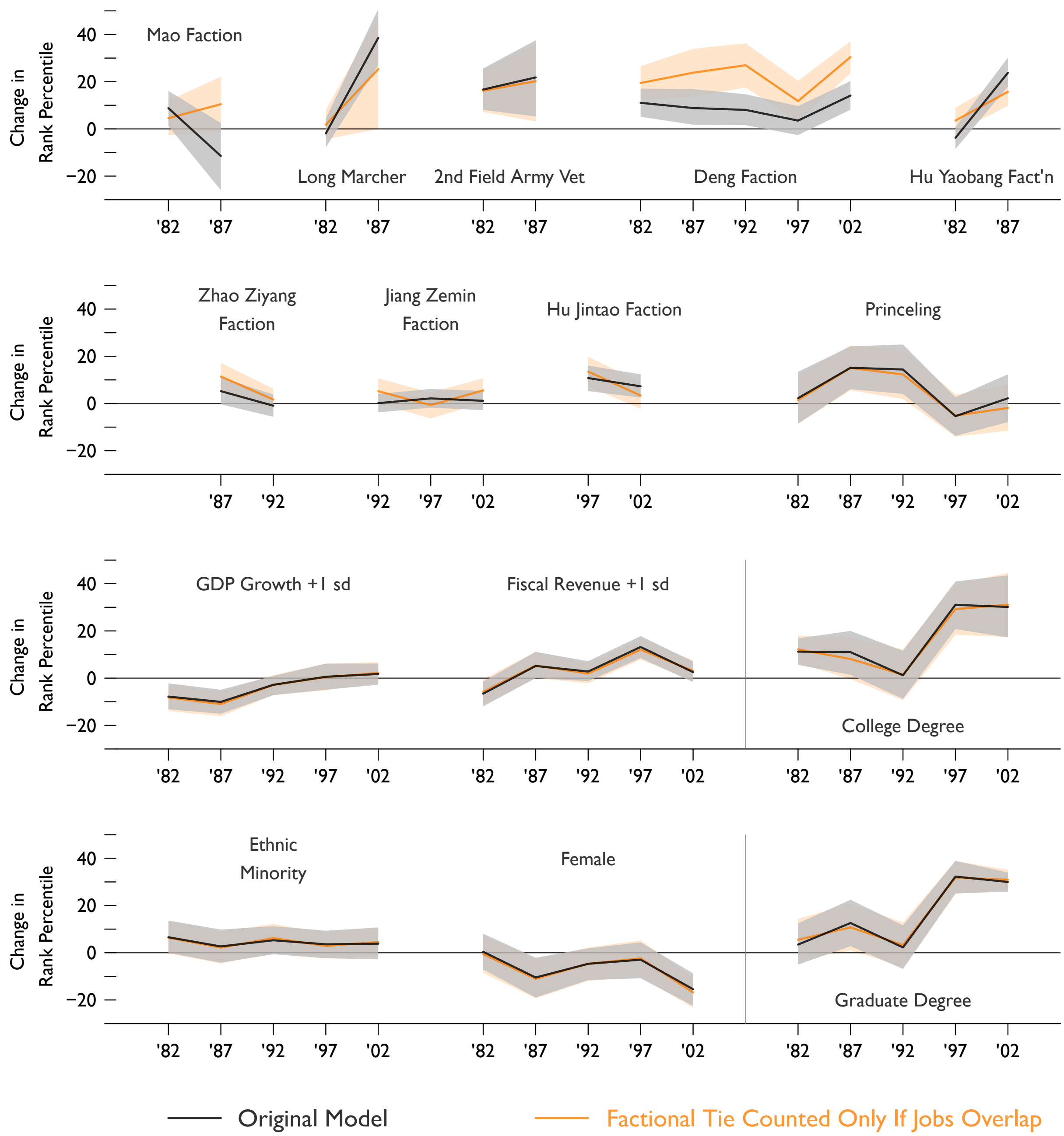

Figure A4: **Robustness of results using narrow measures of factional ties.** Each plot shows the estimated change in rank percentile for a member with otherwise average measured and unmeasured characteristics but a higher value for the given covariate. Estimates from the robustness check counting only overlapping work experience as evidence of a factional tie (in orange) join the original model from Figure 3 (in black). Shaded regions show 67% Bayesian confidence intervals.

	Number of parameters estimated	Proportion of factional variables with confidence intervals bounded away from zero 67% CI	95% CI
Expected by chance	—	0.33	0.05
Found using random factions	80	0.33	0.09
Found using measured factions	16	0.50	0.19

Table A1: **Factional variables have precisely estimated positive or negative effects on rank more often than expected by chance or observed in random data.**

critique under one condition: if a *totally random* scrambling of factional assignments produced factional rankings "as strong" as the ones we see with real data, we would suspect that our results are potentially spurious. If the confidence intervals computed from our model are correct, we need not worry: by chance we should expect to see only 5 percent of factional results with 95 percent confidence on one side of zero, when we observe such results 19 percent of the time.

But can we trust these confidence intervals? One way to check is to generate a set of random factional variables with the same total members as the observed factions, but with the membership rolls randomly shuffled. When we substitute these random factional variables into our rank data model, the absolute effect of "faction" on rank should diminish, and few of the results should have 95 percent confidence bounded away from zero. For each Party Congress, we run this experiment five times, generating a total of 80 estimates of random faction effects (5 runs $\times$ 16 random factional variables).[3] As Table A1 shows, random faction variables behave very much like statistical noise, while our factional variables appear to convey meaningful information about political rank. From a frequentist point of view, we would expect to see so many "significant" results with probability less than 0.01 if factions really were random noise.

One might object that all or most of our factional variables should have confidence intervals far from zero, but this cannot be the case. Only in Lake Wobegon can all factions, like all children, be far from average. In any real ranked system, most factions are destined to lie at or near the middle of the pack, as that is where most of the officials are. Even in a ranking system riven by factions, many factions *should* have rank effects estimated near zero. Very powerful and

[3]As usual, we obtain quick convergence, with $\hat{R} < 1.1$ for all parameters after 10,000 MCMC iterations, and as before, the first 5,000 iterations are discarded as a burn-in period.

very weak factions are simply exceptions to this tendency, but we have shown there are enough such exceptions to make them important determinants of cadres' political fortunes.

A.4 The non-effect of performance is very robust

Our most controversial finding is that provincial officials' economic performance goes politically unrewarded. This counters conventional wisdom, so we take special care to investigate whether this non-effect depends on debatable assumptions. Here we address three potential criticisms of our approach in the baseline models: that performance is multidimensional, potentially subject to selection bias in the appointment of provincial officials, and potentially endogenous to political power. In each case, we show either that our results hold in a model relaxing the relevant assumptions, or that even if upheld the objection is not sufficient to salvage the yardstick competition hypothesis.

The simplest issue we take up is the possibility that officials are rewarded for other kinds of performance besides GDP and revenue growth. To create broad measures of performance, we apply factor analysis to a large dataset of provisional performance measures, and use the factor loadings as performance covariates in our rank data model. If even these broader performance variables fail to reveal political rewards, it will be hard to see where else to look for a "yardstick" to judge officials against.

The most important issue we address is the potential for selection bias. In our baseline models, we implicitly assumed that changes in economic performance in a province after a new governor is appointed are a product of the governor's actions and other random factors, but *not* correlated with the decision to appoint the official to that province. We now relax this assumption. For each province and year of appointment, we forecast provincial performance over the next five years using only historical, pre-appointment data. We treat these forecasts as proxies for the expectations contemporaneous Chinese officials might have formed about future provincial performance. By subtracting these expectations from actual performance, we create a measure of changes in performance potentially due to the efforts of new officials. By construction, this "surprise" performance could not have been predicted at the time of appointment, and thus could not have been a factor in the appointment decision. We implement this (to our knowledge) novel solution to selection bias using by applying well-known tools of time series analysis to our provincial performance dataset.

Finally, we consider the potential added problem of endogeneity. Suppose that instead of (or in addition to) assigning political favored candidates to provinces with bright economic futures, party leaders systematically aid the provinces of future party stars to artificially engineer economic success. This would make economic performance in period t endogenous to political rank in period $t + 1$. But is this endogeneity a problem for our model? And could such endogeneity possibly hide evidence in favor of the yardstick competition hypothesis?

The remainder of this section discusses several technical issues related to missing performance data, estimation of the latent performance factors, and estimation of historical expected performance measures. We then present the results of our robustness checks for selection bias and broader performance factors, and conclude with a discussion of our endogeneity, including an instrumental variables strategy.

A.4.1 Multiple imputation strategy for provincial performance data

To simplify implementation of our selection bias strategy, we multiply impute the missing values in our provincial performance dataset. This avoids complications that would arise later if we tried to compute factor loadings or forecast from time series with missing values and varying lengths. Table A2 shows the fraction of data missing by decade and variable for the performance measures used in later analyses. Many variables are missing from some provinces in early years, but virtually all are nearly complete for the heart of the dataset covering the period studied by our rank data model (1982–2002). Missingness overall is just 18 percent, with only 2 percent and 6 percent missing for the key variables of economic growth and fiscal revenue, respectively. Indeed, with trivial exceptions, these key variables are only ever missing for two provinces. We expect (and confirm) that multiple imputation causes only minimal changes in our results.

We use the widely-employed Amelia software for multiple imputation (King et al., 2001). Amelia assumes all observed and missing data are jointly multivariate normal, and uses an expectation maximization algorithm to fill in a range of predicted values for each missing case across a set of five imputed datasets. Because we have a panel of economic performance variables,

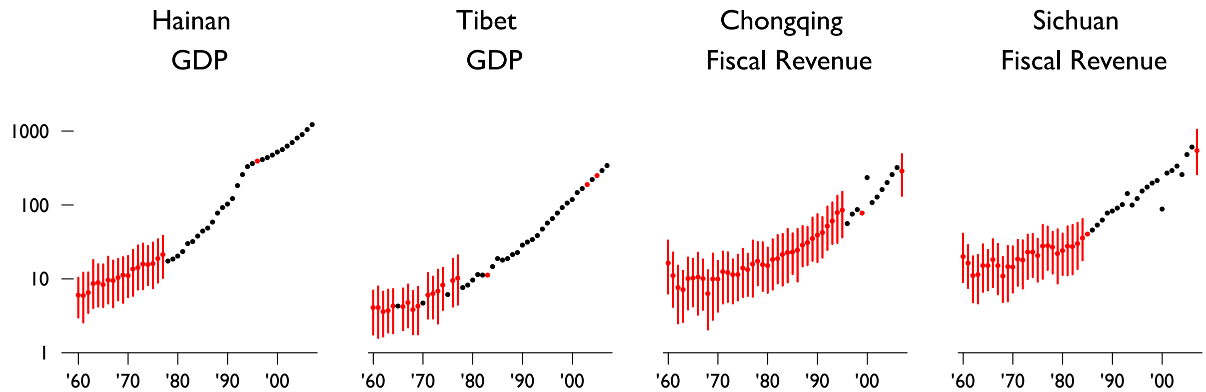

Figure A5: **Multiple imputation of missing GDP growth and fiscal revenue.** The plot shows observed data in black, and the mean multiple imputation for missing values in red. Red vertical lines show 90% confidence intervals. All other provinces are fully observed for these variables with trivial exceptions for the last time period.

we also allow for intertemporal and contemporaneous correlation (Honaker and King, 2010).[4] As in any multiple imputation, the goal is not precise estimation of the missing cases but accurate quantification of their uncertainty.

Figure A5 shows how the model imputed the missing vases for all provinces with missing GDP or fiscal revenue data (the remaining 29 provinces are fully observed for each variable, except in 2002).[5] For our data, we surveyed 744 such plots (24 variables x 31 regions), and found most to fit as well or better than these examples, though the overall impression is difficult to summarize.

Our preferred goodnes of fit test asks how well the imputation model would impute the known cases were each to be omitted in turn, a process known as *overimputation*. Because multiple imputation focuses on quantifying the uncertainty in missing data, the best test of

[4]We include all available performance measures in our imputation model, log any count variables as recommended to make variables more approximately Normal, and explore a variety of specifications, all of which yield very similar results. The model we select allows for third degree polynomial smooths over time. Including splines, lead, and lags all made little difference in the fit as judged from overimputation or time series plots.

[5]Data were missing for Hainan, Chongqing, and Sichuan because Hainan and Chongqing were not provinces in the earlier periods. Instead, they were sub-provincial districts, and in Chongqing's case, part of Sichuan. The missing values for these cases are of interest only in forecasting expected performance once these districts become provinces, as district chiefs were typically not even ACC members. For forecasting purposes, the imputed values are the best available. Imputation estimates, based on patterns in other provinces, what GDP, revenue, and other performance these districts might have been had they been provinces all along, and thus provides the best basis on which to form expectations of future performance.

	Proportion missing by decade 1960s	1970s	1980s	1990s	2000s	Overall missing	Coverage of 90% CIs in observed cases
GDP growth	0.06	0.05	0.00	0.00	0.00	0.02	0.95
Fiscal revenue	0.06	0.06	0.05	0.02	0.13	0.06	0.93
Urban disposable income pc	0.87	0.79	0.03	0.00	0.00	0.35	0.86
Urban living expenditure pc	0.89	0.81	0.05	0.00	0.00	0.36	0.86
Investment in fixed assets	0.09	0.05	0.00	0.00	0.00	0.03	0.96
Industrial output	0.08	0.06	0.00	0.00	0.00	0.03	0.94
Retail sales of consumer goods	0.07	0.05	0.00	0.00	0.00	0.03	0.94
Urban food expenditure pc	0.95	0.87	0.10	0.02	0.00	0.41	0.85
Rural food expenditure pc	0.98	0.84	0.07	0.02	0.00	0.40	0.88
Rural living expenditure pc	0.95	0.83	0.03	0.00	0.00	0.38	0.88
Rural net income pc	0.89	0.75	0.00	0.00	0.00	0.34	0.88
Hospital beds per 10k persons	0.16	0.06	0.00	0.00	0.06	0.06	0.89
Fiscal expenditure	0.06	0.06	0.05	0.02	0.13	0.06	0.95
Employment rate	0.26	0.17	0.02	0.00	0.00	0.09	0.83
Primary teachers per 10k persons	0.15	0.09	0.03	0.02	0.00	0.06	0.80
Grain production	0.03	0.02	0.00	0.00	0.00	0.01	0.82
Urban employment rate	0.23	0.16	0.03	0.00	0.00	0.09	0.91
Primary school-aged enrollment rate	0.66	0.52	0.14	0.05	0.03	0.29	0.95
Tourism income	1.00	0.91	0.17	0.02	0.00	0.44	0.83
Foreign capital used	1.00	0.98	0.35	0.07	0.17	0.53	0.82
Hospitals	0.25	0.14	0.04	0.00	0.04	0.10	0.87
Total exports	0.38	0.22	0.05	0.00	0.00	0.13	0.93
Doctors per 10k persons	0.20	0.07	0.00	0.00	0.08	0.07	0.87
Health Institutions	0.20	0.11	0.01	0.00	0.01	0.07	0.86
Averages	0.44	0.36	0.05	0.01	0.03	0.18	0.88

Table A2: **Patterns of missingness in Chinese provincial performance data, 1960–2002.**

the overimputed data is whether the confidence intervals around the overimputed values have appropriate coverage, or contain the truth in the advertised fraction of cases. The final column of Table A2 shows our final multiple imputation model performs fairly well on known data: 88 percent of known cases fall inside the 90 percent confidence interval. On the key variables of GDP growth and fiscal revenue, the multiply imputed values are, if anything, somewhat overcautious.

Although multiple imputation creates extra work for the analyst—to estimate our rank data model, we must now run the rank data model on five separate imputed datasets, then combine the posterior draws from each into a single pool—interpretation of the results of our models is unchanged, and can be displayed using the same graphics as before. As Figure A6 shows, when we re-estimate the baseline model for each Party Congress using the multiply imputed data, for most covariates we can barely tell that anything has changed.[6] Our results for GDP growth and fiscal revenue are just slightly different, especially for early periods where more officials may have been missing prior performance data, but even in these cases our substantive conclusions have not changed: there is still no political reward for provincial performance.

A.4.2 Using factor analysis to summarize performance along multiple dimensions

We have argued so far that Chinese provincial officials are not rewarded for strong relative GDP growth and only sometimes for strong revenue growth with higher ranks in the CCP. We think this cuts to the core of the claim that meritocratic rewards supported economic growth in the reform period. Nevertheless, it is possible officials reap political rewards for good performance on a broad range of metrics, with economic growth playing too small a role to detect by itself. We might control for all the different performance measures in Table A2 in our rank data models, but with a fixed number of officials to study in each Party Congress, we have little hope of precisely estimating so many different parameters simultaneously—nor do we harbor much hope that any one metric by itself will generate significant political rewards if even GDP growth does not. Instead, we perform a factor analysis to identify whether the bulk of the variation across the 24 different measures of performance listed can be reduced into a set of coherent latent variables small enough in number to add as controls. If the CCP has any noteworthy tendency to reward performance of any kind, it should show up in one or more of these factors.

Figure A7 reports the loadings from an exploratory factor analysis allowing for five different orthogonal factors of performance.[7] We use varimax rotation, and performed a separate factor

[6] We obtain quick convergence, with $\hat{R} < 1.1$ for all parameters across all imputed datasets after 10,000 MCMC iterations, and as before, the first 5,000 iterations are discarded as a burn-in period. The remaining draws from all 15 chains (three for each of the five imputed datasets) are then pooled, as recommended by (King et al., 2001).

[7] We experimented with factor analyses using as few as one and as many as eight factors. There is no one "correct" technique for choosing the optimal number of factors, and the most popular approaches disagree in our case. Guttman (1954) noted that it makes little sense to keep factors which explain less variance than the original variables. On this standard, we should have at most 12 factors. The other popular method is to keep those factors which perform best on a scree plot; i.e., look for a precipitous drop in variance explained, and keep all the factors that come before it. Raîche, Riopel and Blais (2006) offer a systematic method of keeping the pre-drop-off factors (called "optimal coordinates") which suggests as few as four factors would be best. We chose the model in between these recommendations which yielded the most theoretically coherent factor loadings, the five factor model presented here.

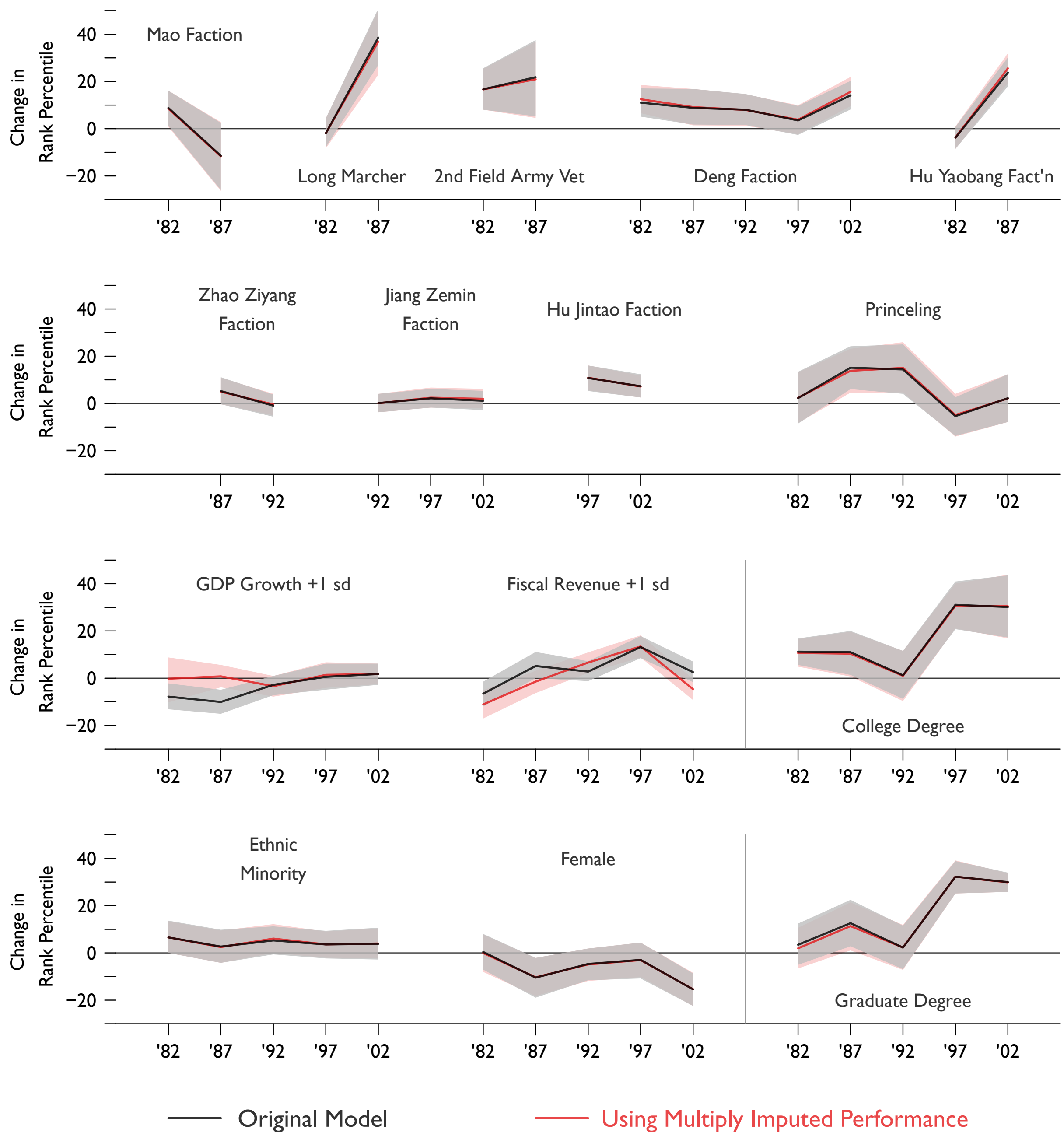

Figure A6: **Robustness of results using multiply imputed provincial performance data.** Each plot shows the estimated change in rank percentile for a member with otherwise average measured and unmeasured characteristics but a higher value for the given covariate. Estimates from the robustness check using multiply imputed GDP growth and fiscal revenue (in red) join the original model from Figure 3 (in black). Shaded regions show 67% Bayesian confidence intervals.

analysis for each imputed dataset. We checked for and found similar loadings across the five imputation datasets. For the reader's convenience, Table A7 reports average factor loadings across the imputations. Of course, in the analyses below we run the partial rank data model on each of the five imputed sets of factors separately, and only combine results after all estimation is completed.

As in any factor analysis, the names we give to factors are impressionistic, but we consider the loading of variables onto these factors quite persuasive, though the total variance explained is relatively small. (Of course, this suggests it really is difficult to condense overall performance into a small set of factors, which makes the idea of systematic rewards for overall performance harder to entertain.) The strongest factor (accounting for 6.6 percent of the total variance) loads onto several proxies for economic growth, including GDP itself, total investment, industrial output, retail sales, fiscal expenditures, and to much lesser extent, fiscal revenues (which does not otherwise load heavily on any factors). The next two factors neatly separate measures of urban wellbeing (6.2 percent of the total variance) from rural wellbeing (4.2 percent of the total variance). A fourth factor (3.4 percent of the variance) appears to collect measures of social service provision, particularly through schools and hospitals, but also through general state expenditures. The final factor (3.1 percent of the variance) is less well defined, but loads moderately on employment metrics, and weakly otherwise. Collectively the five factors account for 23.6 percent of the total variance of the 24 original performance measures.

Henceforth, we present two kinds of specifications for each Party Congress: one relying on GDP growth and fiscal revenue, the original performance measures included in our baseline models, and another replacing those measures with the five factors estimated here.

A.4.3 Good performance is unrewarded by rank on any dimension

We replace the original performance measures of GDP growth and revenue with the five factors of performance derived from our factor analysis, and re-run our rank data model for each Party Congress in turn.[8] The results are shown in Figures A8 and A9. The first figure collects all the

[8] We obtain quick convergence, with $\hat{R} < 1.1$ for all parameters across all imputed datasets after 10,000 MCMC iterations, and as before, the first 5,000 iterations are discarded as a burn-in period. The remaining draws from all 15 chains (three for each of the five imputed datasets) are then pooled, as recommended by (King et al., 2001).

	Growth	Urban Standard of Living	Rural Standard of Living	Social Service Provision	Employment
Urban disposable income pc		0.65	0.13		
Urban living expenditure pc		0.85	0.14		
Retail sales of consumer goods	0.48				
Industrial output	0.49				
GDP growth	0.66				
Investment in fixed assets	0.59			0.10	0.12
Urban food expenditure pc		0.34	0.31		
Rural food expenditure pc			0.41		
Rural living expenditure pc		0.13	0.49		
Rural net income pc			0.50		
Hospital beds per 10k persons				0.44	
Fiscal expenditure	0.41			0.30	0.14
Employment rate					0.38
Urban employment rate					0.30
Grain production					0.23
Primary teachers per 10k persons				0.11	0.23
Fiscal revenue	0.21				
Primary school−aged enrollment rate					
Doctors per 10k persons					
Tourism income					
Foreign capital used			0.11		
Hospitals				0.16	0.14
Health Institutions				0.11	
Total exports				0.16	

Figure A7: **Factor analysis of Chinese provincial performance measures, 1960–2002.** Rows are observed variables, columns are latent factors, and cell entries are factor loadings, averaged across five multiple imputations. Loadings below 0.10 not shown.

covariates in common to our original model and the new 5-factor model, and shows that our results for factions, education, and demographics are quite unaffected by which set of performance measures we include. The second figure compares the new and old performance measures; as both models include a proxy for economic growth, these are contrasted directly; all other performance measures get their own graphics. The results are clear: with the possible exception of Social Services performance in the 14th Party Congress, performance is never rewarded. Taken collectively, the five factor model produces at best one positive result out of twenty tests of performance: no better than we would expect by chance if performance were irrelevant to political rank.

A.4.4 Measuring unexpected performance using time series forecasts of historical data

Yardstick competition suggests officials' political futures depend on the economic performance of the provinces they govern. This leaves political principals with the problem of determining which portion of a performance variable like GDP growth to treat as endogenous to political agents' actions, and which portion to treat as exogenous. To solve this problem, a rational judge of economic performance will exclude from performance evaluations any growth that accrues either from China-wide forces or from pre-appointment trends in the province, and instead bestow political rewards on officials who do the most to raise *unexpected* growth in their provinces. We propose to simulate this decision process by estimating the baseline provincial growth a contemporary official would expect at the moment an official was appointed, then infering the amount of unexpected growth leaders should have attributed to provincial officials.

Measuring unexpected growth is especially important if factional patrons take expected provincial performance into account when matching political appointees to provincial party secretary chairs and governorships, which could introduce selection bias into the baseline models we presented in the main article. Perhaps officials expected to rise quickly through the CCP hierarchy based on their other attributes (such as factional affiliation, education, or political savvy) tend to receive appointments to provinces with rosy futures. If we applied our rank data model to such a system, we would find a (spurious) positive association between performance and rank due to this selection bias.

Of course, we have not found any link between performance and rank, so it cannot be

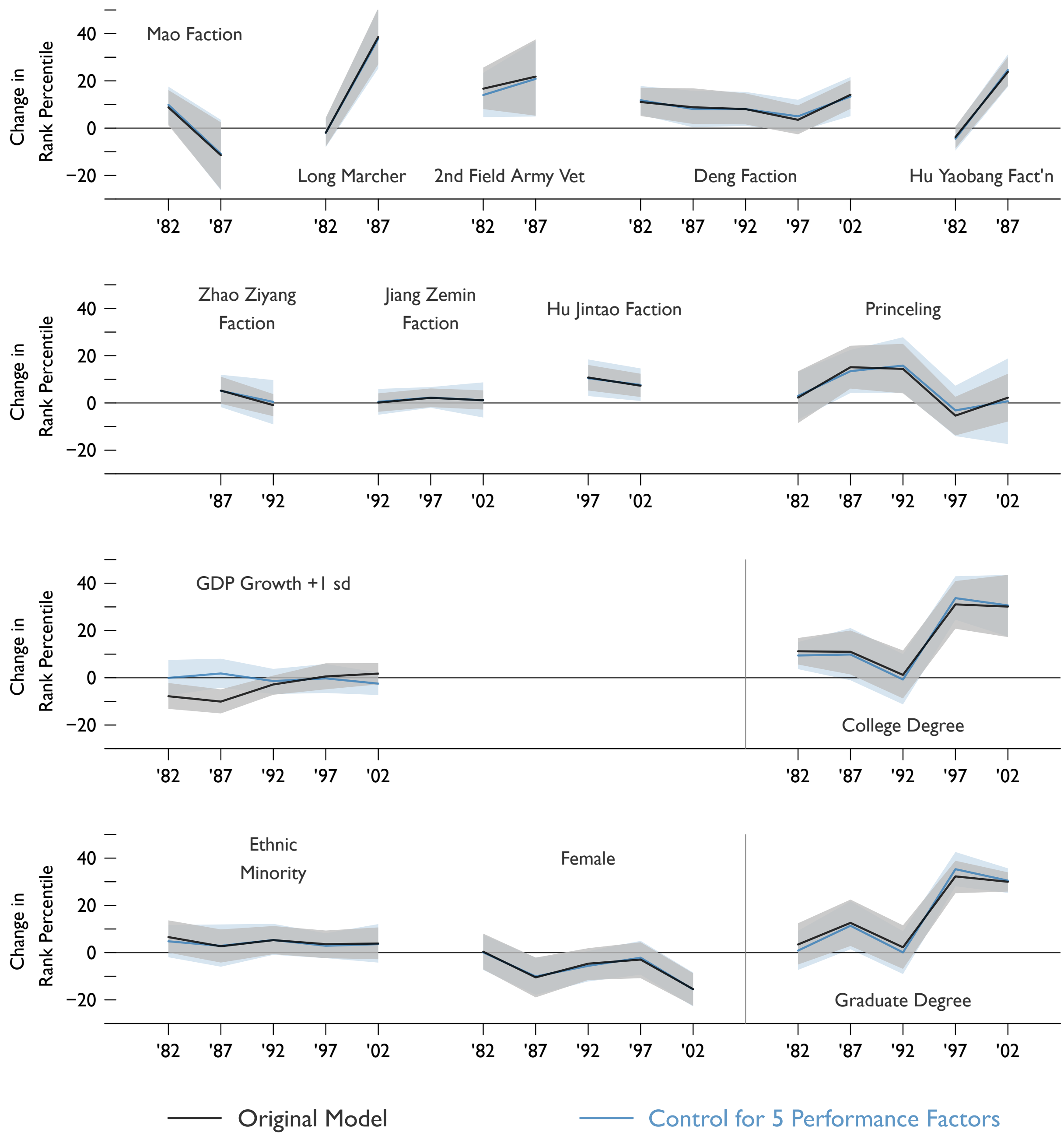

Figure A8: **Robustness of results controlling for five performance factors, part 1.** Each plot shows the estimated change in rank percentile for a member with otherwise average measured and unmeasured characteristics but a higher value for the given covariate. Estimates from the robustness check replacing the original performance measures with five broad performance factors (in blue) join the original model from Figure 3 (in black). Shaded regions show 67% Bayesian confidence intervals.

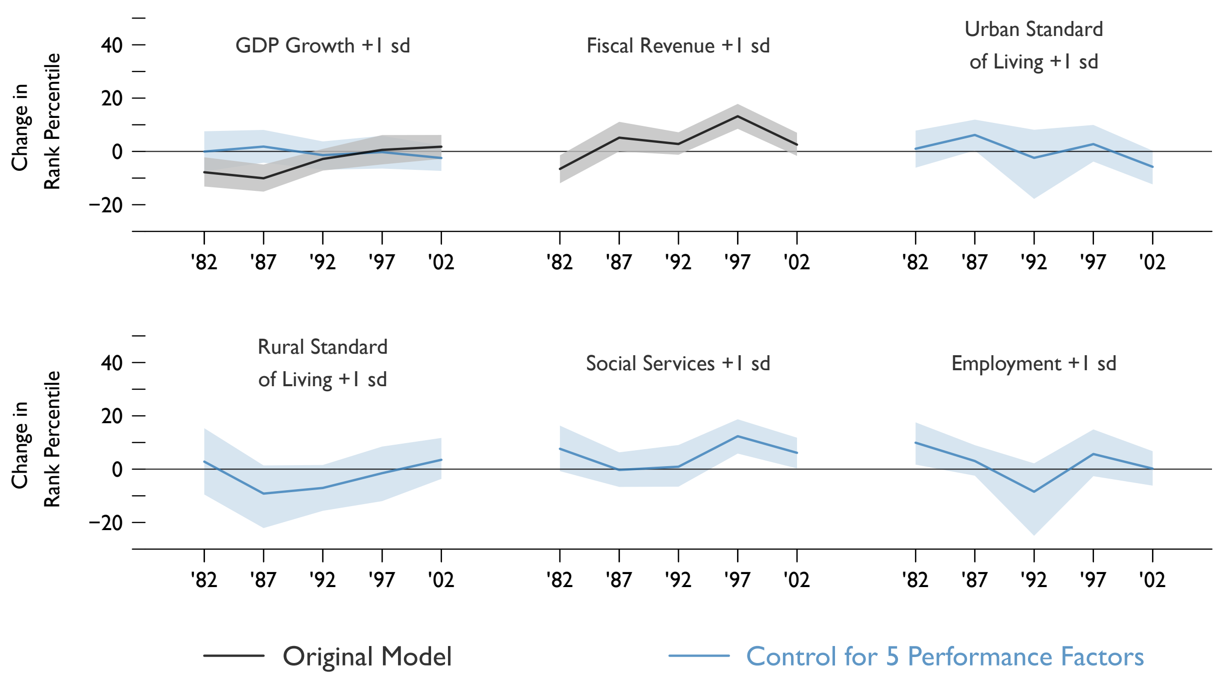

Figure A9: **Robustness of results controlling for five performance factors, part 2.** Each plot shows the estimated change in rank percentile for a member with otherwise average measured and unmeasured characteristics but a higher value for the given covariate. Estimates from the robustness check replacing the original performance measures with five broad performance factors (in blue) join the original model from Figure 3 (in black). Shaded regions show 67% Bayesian confidence intervals.

the case that fast-tracked officials nab the best provinces and claim credit for pre-determined economic growth—there doesn't seem to be any credit to be had! Nevertheless, there is still the possibility of selection bias, with a different explanation: suppose the officials with the greatest talent are sent to under-performing provinces as troubleshooters, and then get rewarded only for improvements relative to expectations. Even the best troubleshooter might not be able to raise his province's growth rate to the China-wide average, but could still make dramatic improvements relative to expectations. Our main measures of economic performance compare performance to pre-appointment levels, and so should provide some protection against this sort of selection bias. However, as the period prior to appointment may not fully capture expectations about a province's future, our performance variables in the baseline models might fail to fully credit this accomplishment, and miss the full effect of performance on rank under non-random assignment of officials to provinces.

Regardless of the selection process used by CCP leaders, we can mitigate selection bias by attempting to assign credit for unexpected growth as if we were contemporaneous leaders. The key step is forming historically-informed expectations for performance in each province on the eve of each provincial appointment. We can then test whether deviations from expected performance are appropriately rewarded or punished. Because we cannnot be sure exactly how contemporaneous leaders formed these expectations, we check the robustness of our findings across a series of different models of expectation formation, to see if *any* model yields a link between performance and rank.

In constructing models of unexpected performance, it helps to define several terms, stated formally in Table A3. First, for a given performance metric $Performance_{rt}$ (which may be GDP, fiscal revenue, or some other public good), define $Performance\ Growth_{rt}$ as the percentage change in the province r's performance from the previous period, $t-1$. To illustrate, suppose we had a two-province country, each of which has a new party secretary or governor. Under their new management, province A grows at 4 percent, and province B at 8 percent. Now define $Relative\ Growth_{rt}$ as the difference between a province's growth rate and the average growth in the rest of the country. In our two-province example, A's relative growth is -4 percent, and B's relative growth is $+4$ percent. So far, the governor of province A appears the better performer.

$\textit{Performance Growth}_{rt}$	$=$	$100\% \times \left(\textit{Performance}_{rt} - \textit{Performance}_{r,t-1}\right) / \textit{Performance}_{rt}$
$\textit{Relative Growth}_{rt}$	$=$	$\textit{Performance Growth}_{rt} - \textit{Performance Growth}_{ROC,t}$
$\textit{Expected Growth}_{rt}$	$=$	$\mathrm{E}\left(\textit{Performance Growth}_{rt} \middle| \textit{Performance Growth}_{r,s<t}\right)$
$\textit{Surprise Growth}_{rt}$	$=$	$\textit{Performance Growth}_{rt} - \textit{Expected Growth}_{rt}$
$\textit{Relative Surprise Growth}_{rt}$	$=$	$\textit{Surprise Growth}_{rt} - \textit{Surprise Growth}_{ROC,t}$

Table A3: **Concepts for measuring the economic performance of provincial officials.**

Now suppose party leaders have a method of estimating $\textit{Expected Growth}_{rt}$ for each province based only on data collected prior to the current provincial party secretary or governor's appointment. In our example, suppose party leaders expected province A to grow at 2 percent, and province B at 10 percent. Subtracting expected from actual growth yields $\textit{Surprise Growth}_{rt}$, and reverses the ranking of the provinces: province A has beaten expectations by +2 percent, while province B is −2 percent below expected growth. Finally, both because the race for political rank is a zero-sum competition in which relative performance matters, and to remove country-wide shocks likely outside the control of provincial officials, we construct $\textit{Relative Surprise Growth}_{rt}$ by subtracting off the average surprise growth in the rest of the country. On this metric, province A improves to +4 percent (as this province beat expectations in a year when other provinces fell behind them) and province B sinks to −4 percent (for falling behind just as the other provinces surged ahead).

All these quantities can be uncontroversially calculated once we have an estimate of $\textit{Expected Growth}_{rt}$. Because we cannot be sure how contemporary officials formed expectations about provincial performance, we consider several methods for creating these expectations, and check whether *any* of them suggest that officials tend to rise in rank after any kind of unexpectedly good performance. We focus on three forecasts. The first, a relatively *naïve model*, expects performance to remain unchanged from the pre-appointment period, and thus attributes any change in performance in a province (relative to the rest of China) to new officials. This is the measure of relative performance we used in our baseline models, and protects us against selection bias to the extent appointing leaders do not look beyond current economic performance in selecting candidates for provincial posts.

The second method, an *intermediate model*, assumes that party leaders formed forecasts "as if" provincial performance were autoregressive time series of order one. The AR(1) model assumes past shocks to a time series have gradually diminishing effects on future values. This model should produce improved estimates of expected performance growth, and thus sharper estimates of the positive or negative influence of provincial officials.

To see how we produce expected growth estimates, consider an example. Suppose that a new party secretary or governor takes office in province A in 1982. We want to calculate the pre-appointment expectation of annual growth in GDP in province A over the years 1982 to 1987 to serve as a baseline level of performance against which the new official can be judged. First we estimate a AR(1) time series model of GDP growth in province A, using historical data on the province's GDP from 1960 to 1981. We then use that model, and the observed 1981 GDP growth, to forecast GDP growth, year by year, from 1982 through 1987. This method yields an AR(1)-based five-year forecast of relative growth performance that a contemporaneous official could have made for province A before appointing a new party secretary or governor in 1982, and thus serves as a useful baseline for separating the performance of the new official from any pre-existing economic trends that might be correlated with his appointment itself.

As the example suggests, the data available to our forecasting model, and thus the estimated model itself, depend on both the province and the year we start our forecast. This differs from most forecasting problems, because we need to limit our model to information available to party leaders at the time of appointment. Thus for each performance measure we consider, we must estimate a total of 4,030 separate AR(1) models (31 provinces $\times$ 26 starting periods from 1977 to 2002 $\times$ 5 multiple imputation datasets). Although the large number of models precluded extensive checks, there was little evidence of non-stationarity in cases we examined, and estimated AR(1) parameters were generally well below 1.0, as is generally expected with a response variable that is a growth rate, rather than a level.

Our third and final forecasting model is a more *sophisticated* implementation of the Box-Jenkins time series methodology, which picks the ARMA(p,q) model with the lowest Akaike Information Criterion (AIC) score. The AIC rewards models for producing a better fit but

	% reduction in error compared to a naïve model assuming persistent growth	
	AR(1)	ARMA(p, q)
GDP growth	19.2	15.1
Fiscal Revenue	20.0	15.2
Economic Growth (Factor 1)	−24.7	8.6
Urban Std of Living (Factor 2)	27.2	20.9
Rural Std of Living (Factor 3)	32.5	32.2
Social Services (Factor 4)	23.4	20.1
Employment (Factor 5)	17.5	14.9
Average percent reduction in prediction error	16.4	18.2

Table A4: **ARMA forecasts of provincial performance predict better than simple extrapolation of past performance.** Results are averaged across the five mulitple imputation datasets.

penalizes models with more complex structure, and tends to select models that predict new data well. It is widely used to select the optimal lag structure for forecasting in time series for which theory offers little guidance. Even though ARMA models ignore both economic theory and indeed other variables, from their introduction into economics in the 1970s ARMA models have usually proved better predictive tools than the large systems of structural equations which previously dominated economic forecasting (Diebold, 2000). By the same token, we think it is at least arguable that the AIC-minimizing ARMA model may mimic the predictions of a generic effective forecaster with the right intuition about which information to extrapolate into a prediction.

We allow the AIC to choose the best fit from a suite of specifications including any autoregressive model up to AR(3), any moving average model up to MA(3), and any autoregressive moving average up to ARMA(2,2). Naturally, if the model with the lowest AIC is the AR(1) model, then our "sophisticated" model will produce the same forecast as our "intermediate" model. We estimate a seperate ARMA(p,q) for each province and appointment year, using only historical data. Thus for each performance measure we consider, we must estimate a total of 44,330 different ARMA(p,q) models (11 possible ARMA specifications $\times$ 31 provinces $\times$ 26 starting periods from 1977 to 2002 $\times$ 5 multiple imputation datasets). For each province and starting period, we forecast only from the model with the best AIC.

To make sure our intermediate and sophisticated expectations actually measures something a simple difference in growth would miss, we compare the accuracy of our three forecasts in Table A4. Using AR(1) and ARMA(p, q) models to predict future provincial growth *does* improve on a forecast that assumes growth rates will remain constant, though the reduction in error is hardly dramatic: between 15 and 20 percent.

In the analyses which follow, we use the compiled database of AR(1) and ARMA(p,q) forecasts to construct the expected and surprise performance for actual officials averaged over their past terms as provincial party secretaries or governors. As in our original measures of performance, we treat as structural zeros the "performance" of cadres who did not serve as provincial party secretaries or governors in the five years prior to a Party Congress.

A.4.5 Performing above expectations yields no rewards in political rank

We showed in our baseline models that officials whose provinces grow faster than they did immediately before appointment gain no political rank for their good performance. We now show that these results are robust—on both narrow and broad performance metrics—even if there is selection bias in officials' assignment to provincial leadership posts. When we isolate just that part of GDP growth, revenue growth, or other performance gain which could not have been predicted by historical trends in provinces or China-wide economic shocks, we still find no indication that future political fortunes depend on provincial performance. This finding holds for unexpected growth estimated by AR(1) or ARMA(p,q) models, as Figures A10 and A11 show. It also holds under models that use the five broad performance factors introducted in Section A.4.2, as we see in Figures A12, A13, A14, and A15. [9] The consistency of these results under three different models of expected performance cast doubt on the notion that a hidden scheme of strategic appointments obscures evidence of performance rewards. Either party leaders do not reward rank, or they judge individual performance by standards unrelated to the contributions we estimate officials actually make to their provinces' economic trajectories. Neither conclusion gives much reason to celebrate the economic benefits of "yardstick competition".

[9] In every case across these numerous models, we obtain quick convergence, with $\hat{R} < 1.1$ for all parameters across all imputed datasets after 10,000 MCMC iterations, and as before, the first 5,000 iterations are discarded as a burn-in period. The remaining draws from all 15 chains (three for each of the five imputed datasets) are then pooled, as recommended by (King et al., 2001).

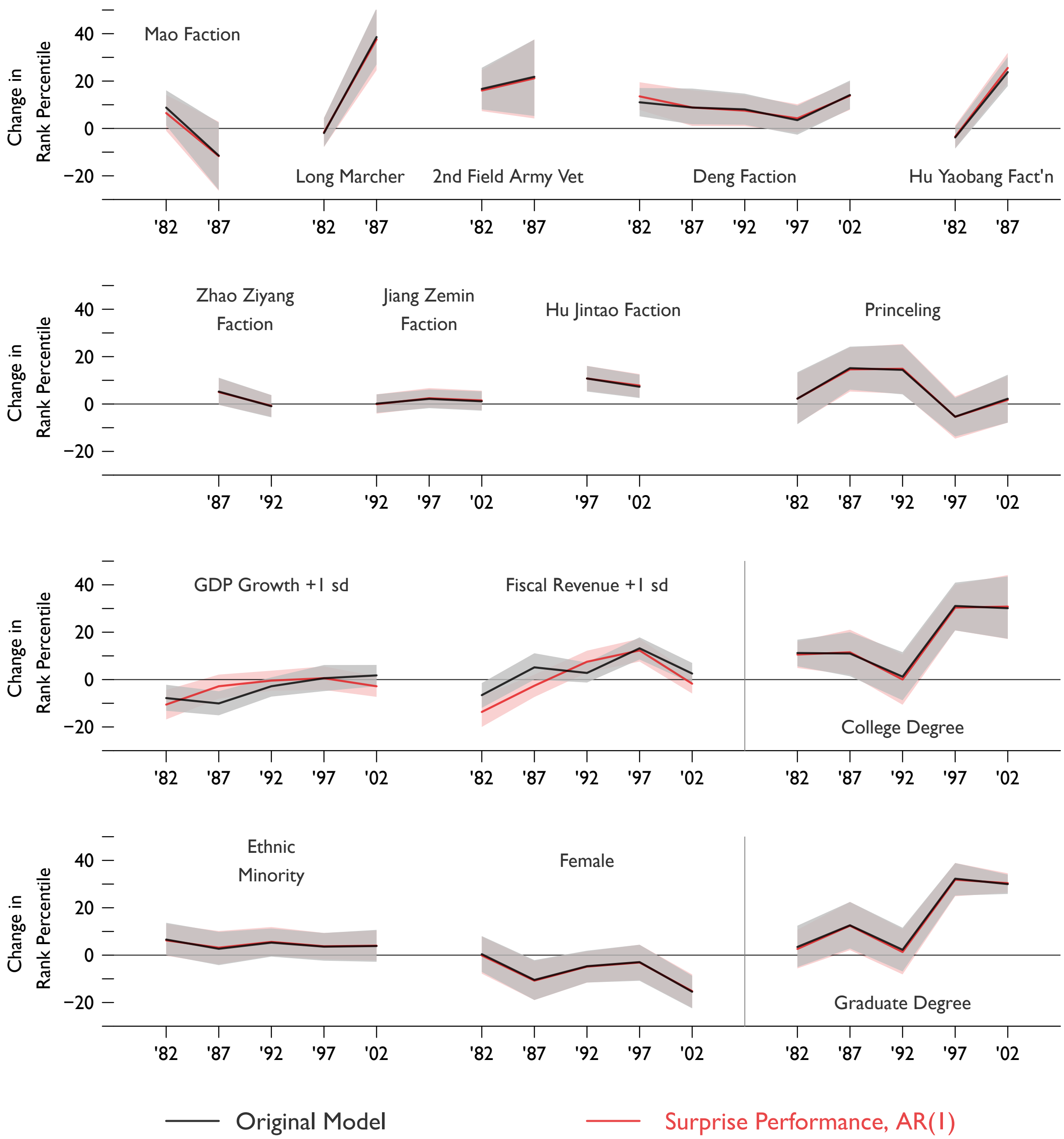

Figure A10: **Robustness of results isolating AR(1) surprise GDP and revenue growth.** Each plot shows the estimated change in rank percentile for a member with otherwise average measured and unmeasured characteristics but an altered value for the given covariate. Estimates from the robustness check isolating the portion of GDP and revenue growth which is unexpected based on an AR(1) forecast (in red) join the original model from Figure 3 (in black). Shaded regions show 67% Bayesian confidence intervals.

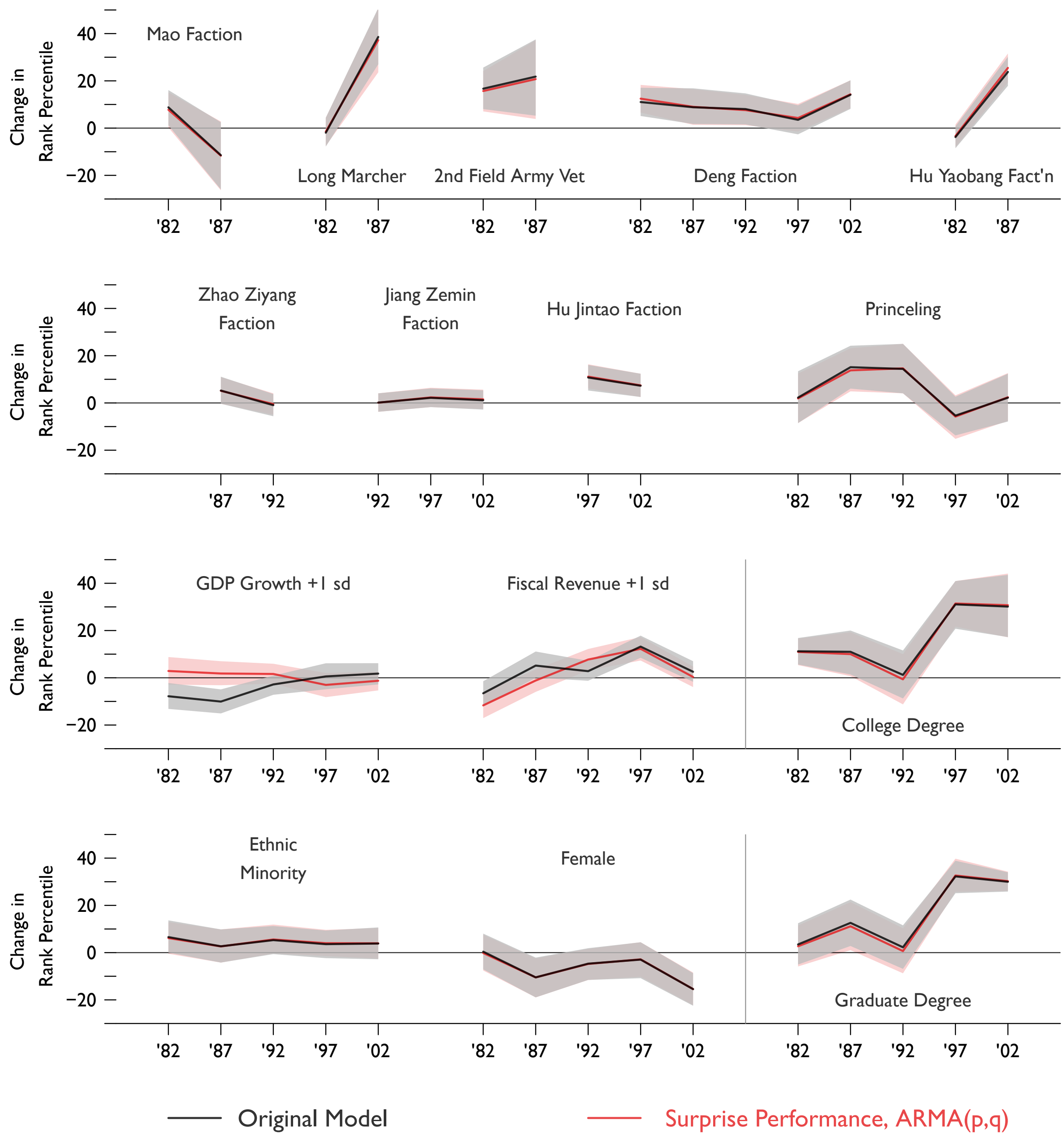

Figure A11: **Robustness of results isolating ARMA(p,q) surprise GDP and revenue growth.** Each plot shows the estimated change in rank percentile for a member with otherwise average measured and unmeasured characteristics but an altered value for the given covariate. Estimates from the robustness check isolating the portion of GDP and revenue growth which is unexpected based on the AIC-minimizing ARMA(p,q) forecast (in red) join the original model from Figure 3 (in black). Shaded regions show 67% Bayesian confidence intervals.

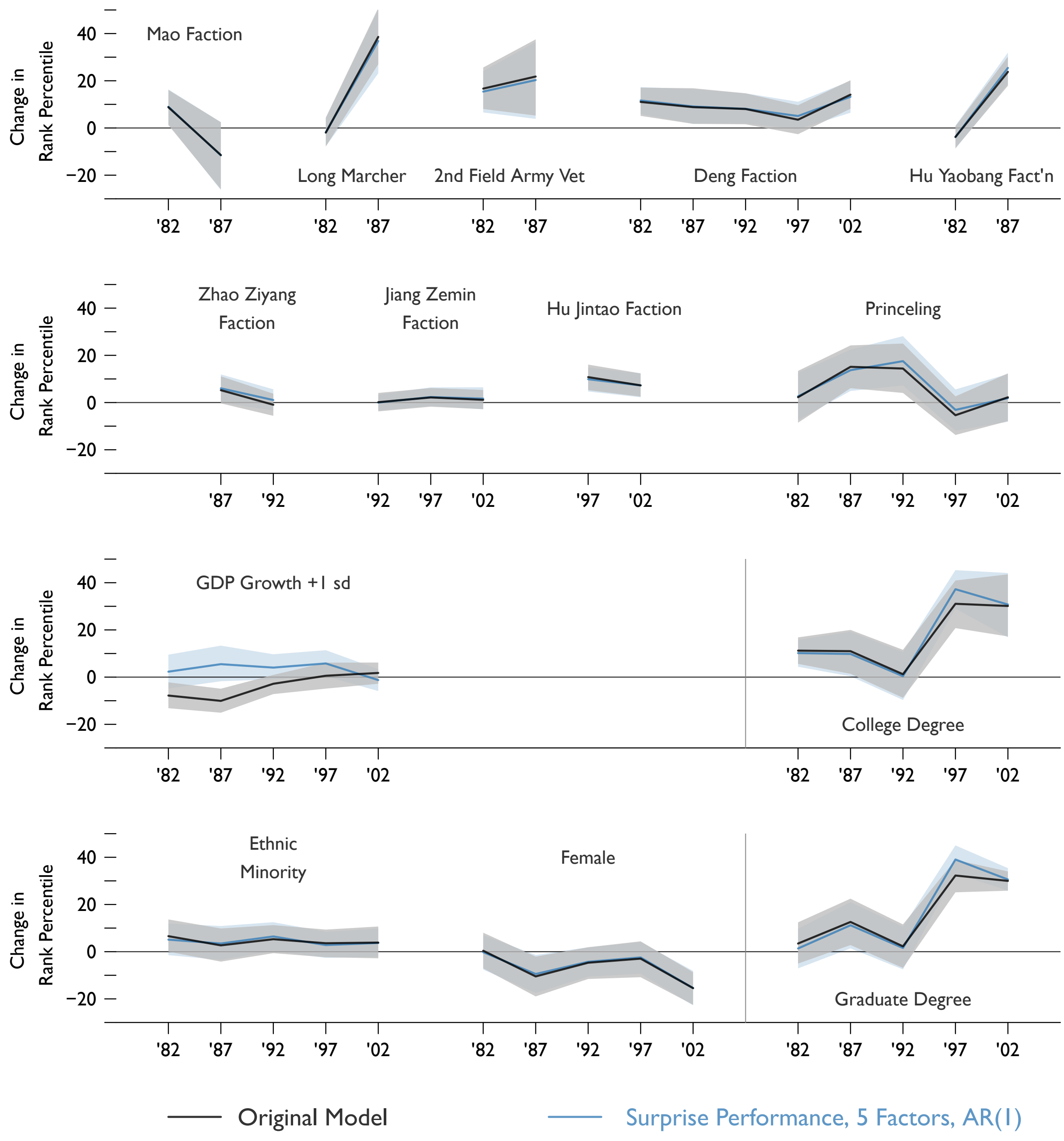

Figure A12: **Robustness of results isolating AR(1) surprise performance on five latent factors, part 1.** Each plot shows the estimated change in rank percentile for a member with otherwise average measured and unmeasured characteristics but a higher value for the given covariate. Estimates from the robustness check isolating the portion of the five performance factors which are unexpected based on an AR(1) forecast (in blue) join the original model from Figure 3 (in black). Shaded regions show 67% Bayesian confidence intervals.

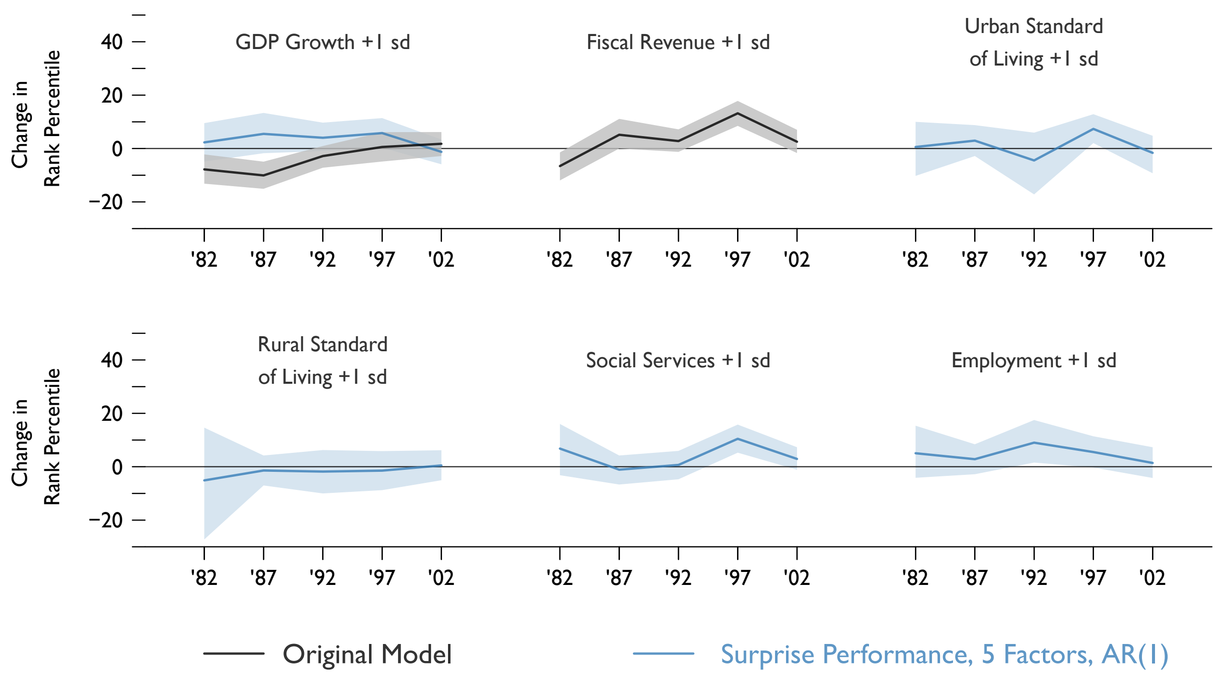

Figure A13: **Robustness of results isolating AR(1) surprise performance on five latent factors, part 2.** Each plot shows the estimated change in rank percentile for a member with otherwise average measured and unmeasured characteristics but a higher value for the given covariate. Estimates from the robustness check isolating the portion of the five performance factors which are unexpected based on an AR(1) forecast (in orange) join the original model from Figure 3 (in blue). Shaded regions show 67% Bayesian confidence intervals.

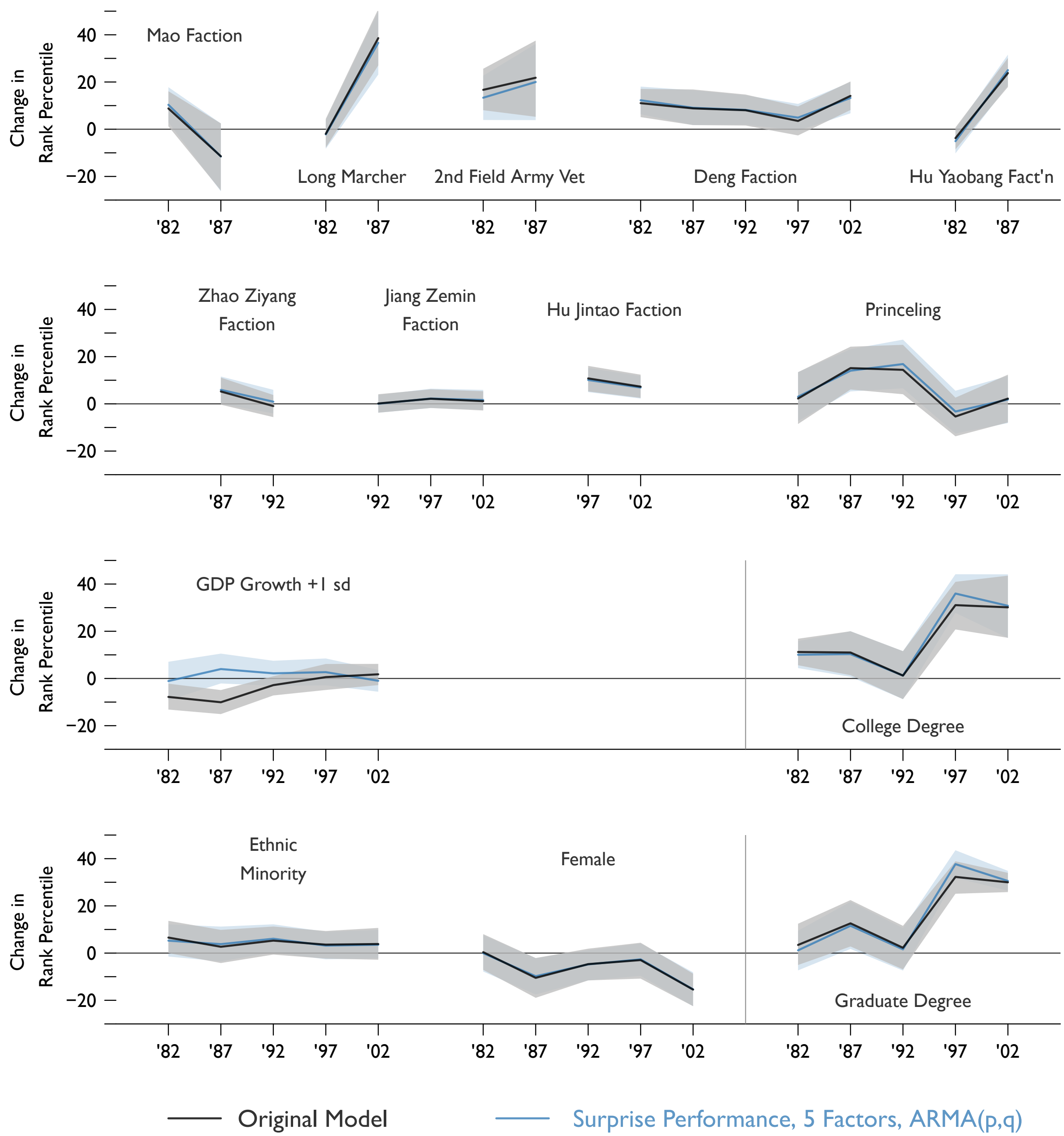

Figure A14: **Robustness of results isolating AR(p,q) surprise performance on five latent factors, part 1.** Each plot shows the estimated change in rank percentile for a member with otherwise average measured and unmeasured characteristics but a higher value for the given covariate. Estimates from the robustness check isolating the portion of the five performance factors which are unexpected based on the best ARMA(p,q) forecast (in blue) join the original model from Figure 3 (in black). Shaded regions show 67% Bayesian confidence intervals.

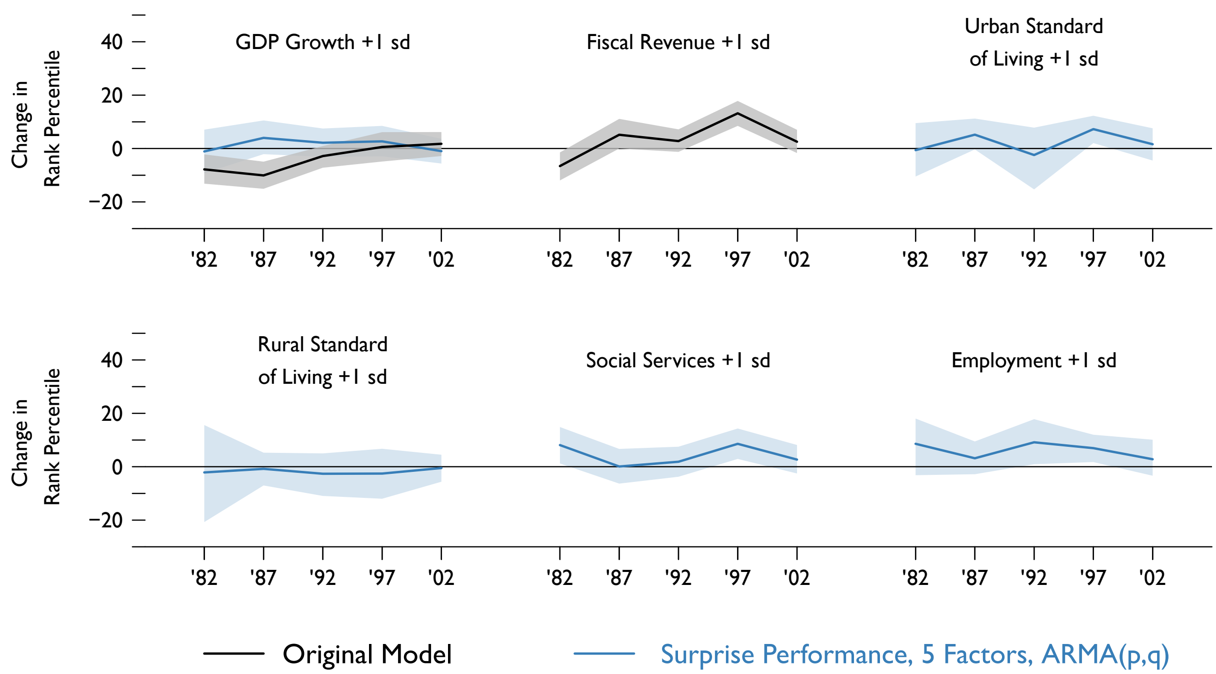

Figure A15: **Robustness of results isolating AR(p,q) surprise performance on five latent factors, part 2.** Each plot shows the estimated change in rank percentile for a member with otherwise average measured and unmeasured characteristics but a higher value for the given covariate. Estimates from the robustness check isolating the portion of the five performance factors which are unexpected based on the best ARMA(p,q) forecast (in blue) join the original model from Figure 3 (in black). Shaded regions show 67% Bayesian confidence intervals.

A.5 Could economic performance be endogenous to political rank?

The concern that early career performance may be endogeous to later career success rests on the following scenario: suppose some young officials are widely expected to fly high later in their careers. These officials are appointed to run provinces, and because of their political connections, receive greater support from the central government, and turn that support into strong economic performance. High rank follows, not because of good performance, but the connections which produced it. In this world, we would see a positive association between performance and rank in our baseline rank regressions, and even in our surprise performance model, but we would not still be unable to conclude that performance caused rank. Indeed, an association between these variables might be indirect evidence of the importance of other variables that created the early expectations of career success—factors which might even include our factional variables!

While this would be a potentially potent criticism of inferences drawn from a positive coefficient on economic performance, in our case, we have little need to worry. In our baseline models (and indeed, in every robustness check), we find no relationship between economic performance and political rank, all else equal. If the yardstick hypothesis holds *and* CCP leaders aid the provinces of future leaders, the two forces could only be canceling each other out. This in turn could only happen if future leaders have systematically *lower* economic performance than other officials, and without aid, would be punished for it. The only way endogeneity could rescue "yardstick competition" from our non-findings is to suppose there is a countervailing campaign to undo all the rewards for good performance by promoting powerful but ineffective provincial officials. Endogeneity can only "mask" performance rewards in our model is if those rewards don't matter!

In any event, we consider this scenario unlikely, and the concern with endogenous prior economic performance a minor one. Nevertheless, we present an instrument for economic growth, an instrumental variables strategy for correcting any endogeneity in a rank data model, and discuss the difficulties in estimating such a model in our case.

A.5.1 Distance to the ocean as an instrument for growth in Chinese provinces

The requirements for an instrument for growth in our application are stricter than usual. Ordinarily, it would be enough that our instrument exogenously determine some portion of relative economic growth, but not directly affect the rank of officials. In this case, the instrument needs several other properties. First, because we think appointing officials may take predictable influences on economic performance into account during the appointment of officials to the provinces, the instrument should have effects which are only discernable after appointment, even though the variation in the instrument must be determined *ex ante*. This suggests looking at unexpected shocks that interact with predetermined provincial characteristics. Second, because only relative performance across provinces can affect rank, the instrument cannot have the same effect across all provinces: it needs to help some and hurt others to shift relative performance, and thus rank. Therefore, the instrument must have both time-varying and cross-sectional effects.

The distance from each province to the ocean appears to meet all criteria. Provincial distance to the ocean was clearly determined long before the period under study, and we have no reason to suspect it has a direct effect on future rank. Although distance from the ocean is itself time invariant, the economic *effects* of ocean distance varied over periods and provinces in response to shocks which could not have been predicted *ex ante*, as a brief history of Chinese coastal development suggests.

In the Mao period and even into the first part of the reform period, coastal provinces were considered war zones which were potential locations of a possible US-backed invasion by the Kuomintang. Thus, the provincial leadership of Zhejiang, Fujian, and Guangdong was mainly pre-occupied with security concerns instead of economic development *per se* (Zweig, 2002). Mao further launched an inland industrialization drive, lasting into the early 1980s, to build up industries in case of an American attack from the coast (Naughton, 1988). Under economic reform, and especially with the formation of special economic zones, coastal regions for the first time benefited from being close to international investors interested in China's enormous pool of cheap labor (Zweig, 2002). Coastal provinces suffered a sharp reversal in the aftermath of the 1989 crackdown, when many Western businesses boycotted exports from China, yet this negative shock was rapidly reversed into a positive one, as Deng Xiaoping pushed for further liberalization of foreign

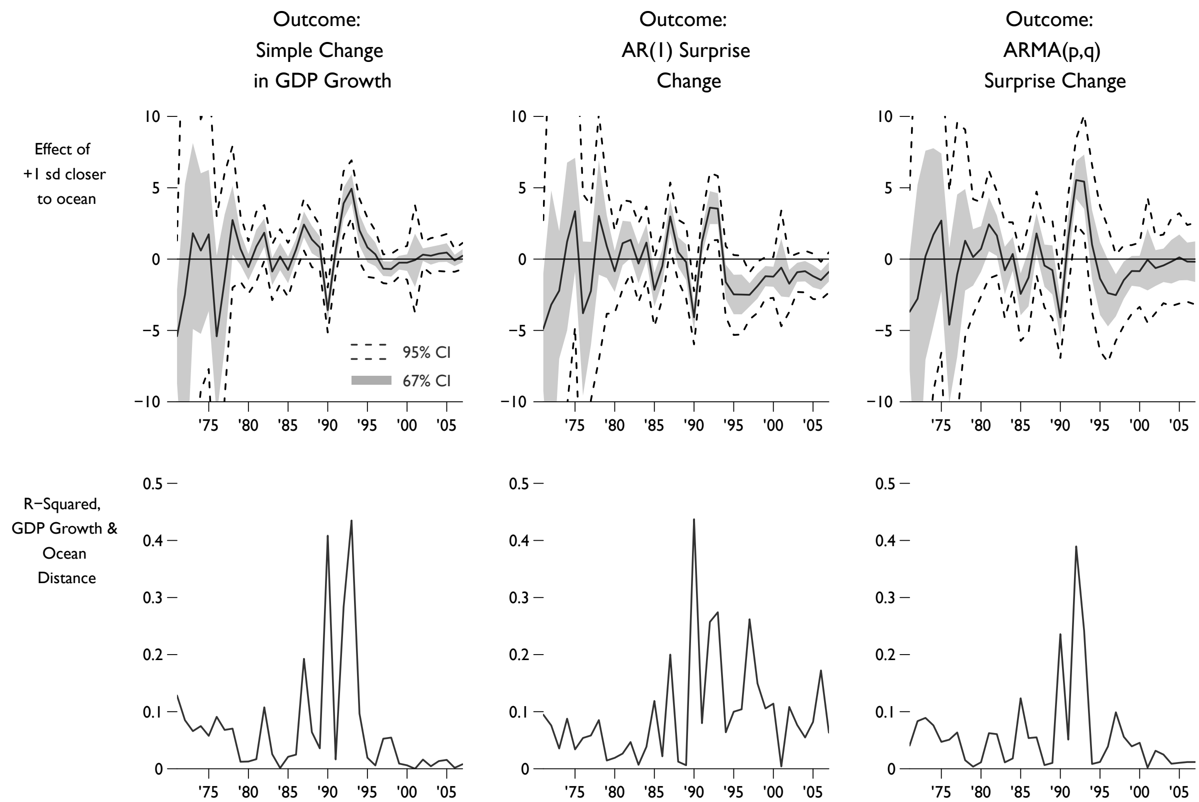

Figure A16: **Evaluation of provincial distance from the ocean as an instrument for economic growth.** The first row of plots shows the varying effect of ocean distance on unexpected economic growth over time, where unexpected growth is calculated either as the simple change in growth from the past levels (column 1), the change in growth from the AR(1) forecast (column 2), or the change in growth from the AIC-minimizing ARMA(p,q) forecast. Unpredictable shocks over time cause proximity to the ocean to hurt growth (around 1990) and later to aid growth (from 1991 to 1993). The second row of plots shows that these two shocks explain a large share of the variance in provincial growth.

investment regulations, bringing a large new wave of FDI all along China's coast.

To confirm this history, we turn to our provincial economic data once again. For each year between 1971 and 2007, we estimate a separate bivariate cross-sectional regression between GDP growth and the distance between the provincial capital and the ocean. For each year, we also estimate the bivariate relationship between ocean distance and the AR(1) surprise GDP growth and ARMA(p,q) surprise GDP growth, respectively. We report in Figure A16 the evolving relationship between exogenous geographic variation and growth. We find that both growth and unexpected growth plummet in coastal provinces around 1990, only to sharply rebound over the next few years, exactly as the historical record suggests it should. During these two shocks, proximity to the ocean explains between 20 and 40 percent of the total variation in GDP growth across Chinese provinces; in other times, proximity to the ocean makes little difference. (We experimented with a log transformation of distance to the ocean, and found very similar results.) We conclude that distance to the ocean captures how different provinces reacted to strong exogenous shocks in the late 1980s and 1990s, and thus constitutes an instrument for growth in our rank model.

A.5.2 A Bayesian instrumental variables model of partially-observed ranks

To add instrumental variables to our Bayesian model of partial rank data, we maintain the assumption that the partially observed ranks y_i are determined by the rank order of official i's latent strength, y_i^*. But now suppose that a covariate x_i is endogenous to latent strength, and that we have a set of instruments $\mathbf{z}_i$ for this covariate, as well as several exogenous covariates $\mathbf{w}_i$. We now rewrite the model so that y_i^* and x_i are jointly and endogenously determined:

$$\begin{bmatrix} x_i \\ y_i^* \end{bmatrix} = \text{Multivariate Normal}\left(\begin{bmatrix} \mu_{x_i} \\ \mu_{y_i^*} \end{bmatrix}, \Sigma = \begin{bmatrix} \sigma_x^2 & \sigma_{x,y^*} \\ \sigma_{x,y^*} & 1 \end{bmatrix}\right) \tag{A-1}$$

where

$$\mu_{x_i} = \mathbf{z}_i\boldsymbol{\gamma} + \mathbf{w}_i\boldsymbol{\lambda} + \tau \tag{A-2}$$

$$\mu_{y_i^*} = x_i\beta + \mathbf{w}_i\boldsymbol{\theta} \tag{A-3}$$

This is the usual instrumental variables estimation problem, with three caveats: 1.) one of our endogenous variables, y_i^*, is unobserved, 2.) for identification y_i^* contains no constant term and a variance constrained to 1 (as in the original partial rank model), and 3.) for any official j who was not a provincial party secretary or governor at some point in the five years prior to the Party Congress, x_j is a structural zero, so $\mathrm{E}(x_j)$ is also constrained to zero and case j must be excluded from estimation of $\boldsymbol{\gamma}$, $\boldsymbol{\lambda}$, and τ.

We set diffuse Normal priors over β, $\boldsymbol{\theta}$, $\boldsymbol{\gamma}$, $\boldsymbol{\lambda}$, and τ and an Inverse-Wishart prior over $\boldsymbol{\Sigma}$. We then construct several Markov chains using the following eight-step procedure, iterating over steps 5 through 8 m times to produce each chain:

1. **Initialize the latent strengths, y^*_{i0}:** Draw, for all members of the Party Congress, a random feasible rank. A set of feasible ranks must respect all tier bounds and tie restrictions, and use each possible rank only once.

2. **Initialize $\mathbf{\Sigma_0}$:** Set starting values for the variance of the endoegneous covariate, σ^2_x and the covariance of the errors between the endoegneous variables, σ_{x,y^*}.

3. **Initialize $\boldsymbol{\gamma_0}$, $\boldsymbol{\lambda}_0$, and τ_0:** Using the starting values y^*_{i0} and $\mathbf{\Sigma_0}$, the endogenous covariate x_i, the instruments $\mathbf{z}_i$, and any other exogenous covariates $\mathbf{w}_i$, compute starting values of $\boldsymbol{\gamma_0}$, $\boldsymbol{\lambda}_0$, and τ_0 using the Gibbs sampler for linear models with instrumental variables (Rossi, Allenby and McCulloch, 2005). Then compute $\mathrm{E}(x_i|\mathbf{z}_i, \mathbf{w}_i)_0 = \mathbf{z}_i\boldsymbol{\gamma}_0 + \mathbf{w}_i\boldsymbol{\lambda}_0 + \tau_0$.

4. **Initialize β_0 and $\boldsymbol{\theta}_0$:** Using the starting values y^*_{i0}, the instrumented value of the endogenous covariate $\mathrm{E}(x_i|\mathbf{z}_i, \mathbf{w}_i)$, and any other exogenous covariates $\mathbf{w}_i$, compute starting values of β_0 and $\boldsymbol{\theta}_0$ using the Gibbs sampler for linear models with instrumental variables (Rossi, Allenby and McCulloch, 2005). Then compute $\mu_{y_i,0} = x_i\beta_0 + \mathbf{w}_i\boldsymbol{\theta}_0$.

5. **Update the latent strengths y^*_{im}:** Employ the same Gibbs–Adaptive-Metropolis-Hastings method as in step 3 in Methods Appendix.

6. **Update $\boldsymbol{\Sigma_m}$:** Because the variance of y^*_i is constrained to unity, update each random term in $\mathbf{\Sigma}_m$ separately. There are several methods available using a Metropolis-Hastings step. Either treat first σ^2_x and then σ_{x,y^*} as truncated Normal (with bounds for each determined by the most recent draw of the other), or treat each variance parameter as Normal (and unbounded) but reject any proposals that make $\mathbf{\Sigma}_m$ not positive definite (Browne, 2006). The first method entails fewer MCMC iterations but adds computation time at each step; the latter is faster per step but slows mixing. In either case, tune the Metropolis-Hastings proposal distribution for each variance parameter to achieve an acceptance rate of approximately 40 percent (Gelman et al., 2003).

7. **Update $\boldsymbol{\gamma_m}$, $\boldsymbol{\lambda}_m$, and τ_m:** Using the updated y^*_{im} and $\boldsymbol{\Sigma_m}$, the endogenous covariate x_i, the instruments $\mathbf{z}_i$, and any other exogenous covariates $\mathbf{w}_i$, compute updated values $\boldsymbol{\gamma_m}$, $\boldsymbol{\lambda}_m$, and τ_m using the Gibbs sampler for linear models with instrumental variables (Rossi, Allenby and McCulloch, 2005). Then compute $\mathrm{E}(x_i|\mathbf{z}_i, \mathbf{w}_i)_m = \mathbf{z}_i\boldsymbol{\gamma}_m + \mathbf{w}_i\boldsymbol{\lambda}_m + \tau_m$.

8. **Update β_m and $\boldsymbol{\theta}_m$:** Using the updated y^*_{im}, the instrumented value of the endogenous covariate $\mathrm{E}(x_i|\mathbf{z}_i, \mathbf{w}_i)_m$, and any other exogenous covariates $\mathbf{w}_i$, compute updated values of β_m and $\boldsymbol{\theta}_m$ using the Gibbs sampler for linear models with instrumental variables (Rossi, Allenby and McCulloch, 2005). Then compute $\mu_{y^*_i,m} = x_i\beta_m + \mathbf{w}_i\boldsymbol{\theta}_m$.

After an intial burn-in, iterate the Markov chains until β, y^*_i, $\boldsymbol{\theta}$, $\boldsymbol{\gamma}$, $\boldsymbol{\lambda}$, τ and $\boldsymbol{\Sigma}$ converge to stable, well-mixed distributions, then sample the posterior distributions of the latent stengths and other parameters from the chains.

A.5.3 Results

We apply the instrumental variables partial rank data model to the 15th Party Congress using first our original performance data, then AR(1) unexpected performance measures, and finally ARMA(p,q) unexpected performance. In each case, we instrument for economic growth using province's distance to the ocean. In the 15th Party Congress, provincial leaders would need to explain their growth performance over the years 1992–1997, which includes a large positive shock to growth in provinces near the coast. We thus expect a negative estimate of γ in this period.

Unfortunately, even with adapative proposal distributions for each of the Metropolis-Hastings steps and many iterations ($> 100,000$), our Markov chains for σ^2_x and σ_{x,y^*_i} fail to converge, or even to mix at all, across any of the models. A limited number of Monte Carlo experiments with artificial data suggest the instrumental variables partial rank data model converges only under unhelpfully restrictive conditions, such as strong endogeneity which is well measured by a very strong instrument. In practical situations with real world instruments and unknown degrees of endogeneity, estimating the covariance of a potentially endogenous covariate and a latent construct seems prohibitively difficult, at least using these methods.

In our application, where economic growth does not appear to be correlated at all with latent strength, obtaining good estimates of their precise relationship may be too much to ask of our data. Although this non-result is not very satisfying, we think it may be just one more hint that there really is no relationship between performance and rank—or that if there is reciprocal causation between performance and rank, it is subtle rather than strong. As we argue above, any such endogeneity still could not save the yardstick competition hypothesis.

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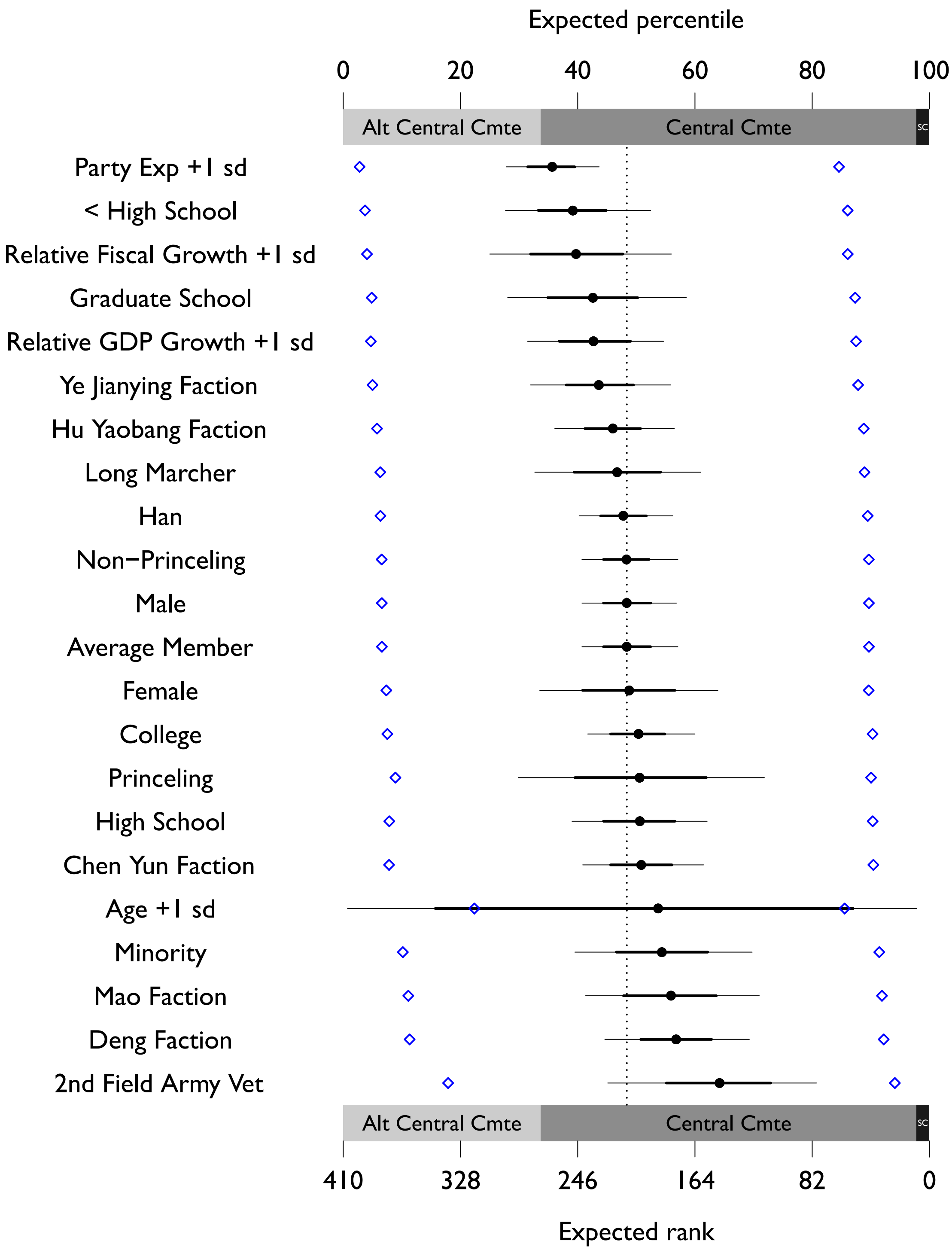

Figure A17: **Formal selection criteria and rank in the 12th Party Congress: Estimated relationships.** For each row of the plot, we set the indicated covariate at one or, for continuous covariates, one standard deviation above the mean, and hold all other variables at their means. The black circles indicate expected rank for an individual with these observed covariates and an average random effect, or degree of unmeasured political ability. Blue diamonds show expected ranks for members with random effects one standard deviation above or below the mean. Thick horizontal lines mark 67% Bayesian confidence intervals and thin horizontal lines indicate 95% Bayesian confidence intervals. The dotted vertical line shows the expected rank of a member with the average covariates. The light gray region indicates the actual ranks of ACC members, the medium gray region shows the Central Committee, and the dark gray region the ranks of the Standing Committee.

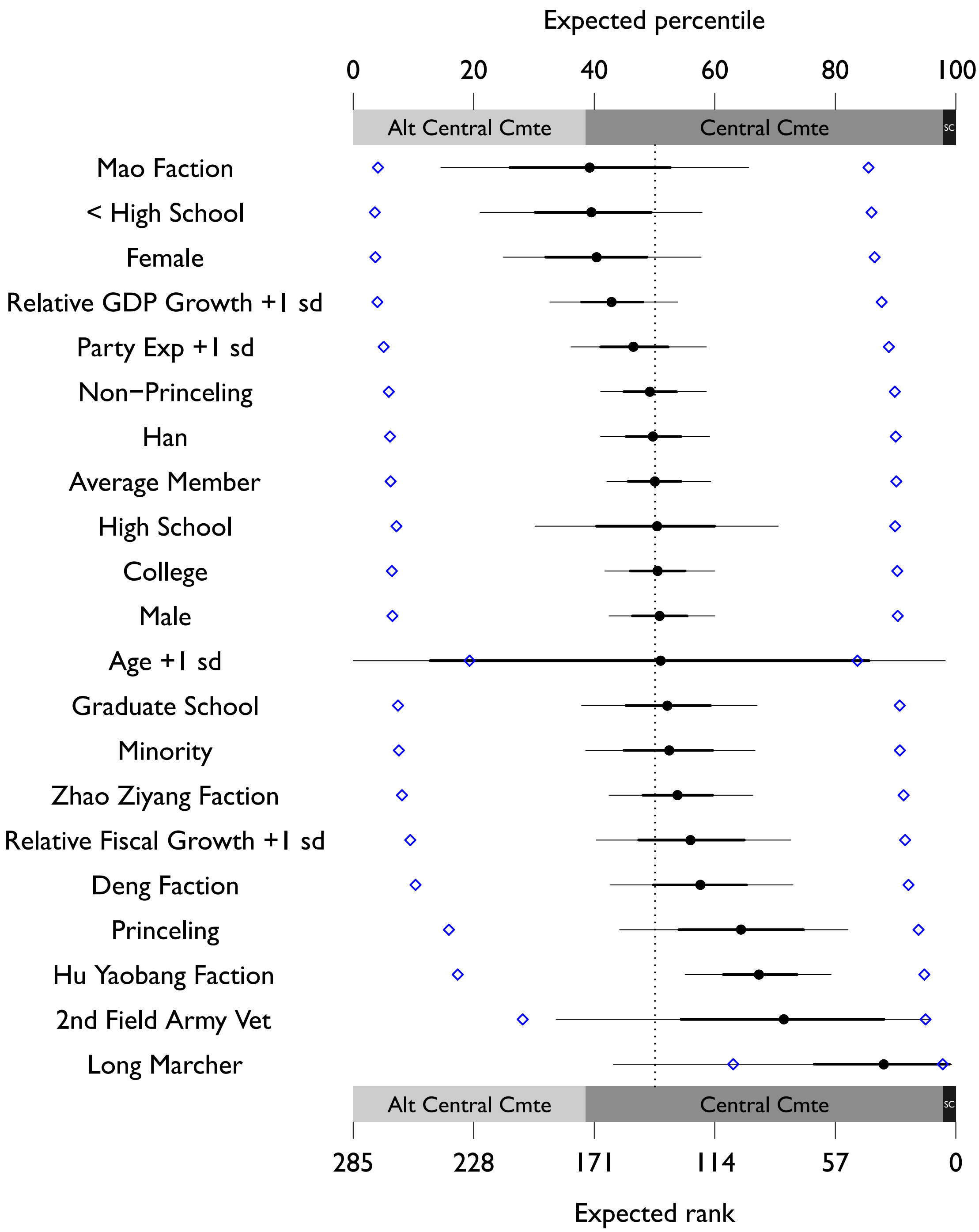

Figure A18: **Formal selection criteria and rank in the 13th Party Congress: Estimated relationships.** For each row of the plot, we set the indicated covariate at one or, for continuous covariates, one standard deviation above the mean, and hold all other variables at their means. The black circles indicate expected rank for an individual with these observed covariates and an average random effect, or degree of unmeasured political ability. Blue diamonds show expected ranks for members with random effects one standard deviation above or below the mean. Thick horizontal lines mark 67% Bayesian confidence intervals and thin horizontal lines indicate 95% Bayesian confidence intervals. The dotted vertical line shows the expected rank of a member with the average covariates. The light gray region indicates the actual ranks of ACC members, the medium gray region shows the Central Committee, and the dark gray region the ranks of the Standing Committee.

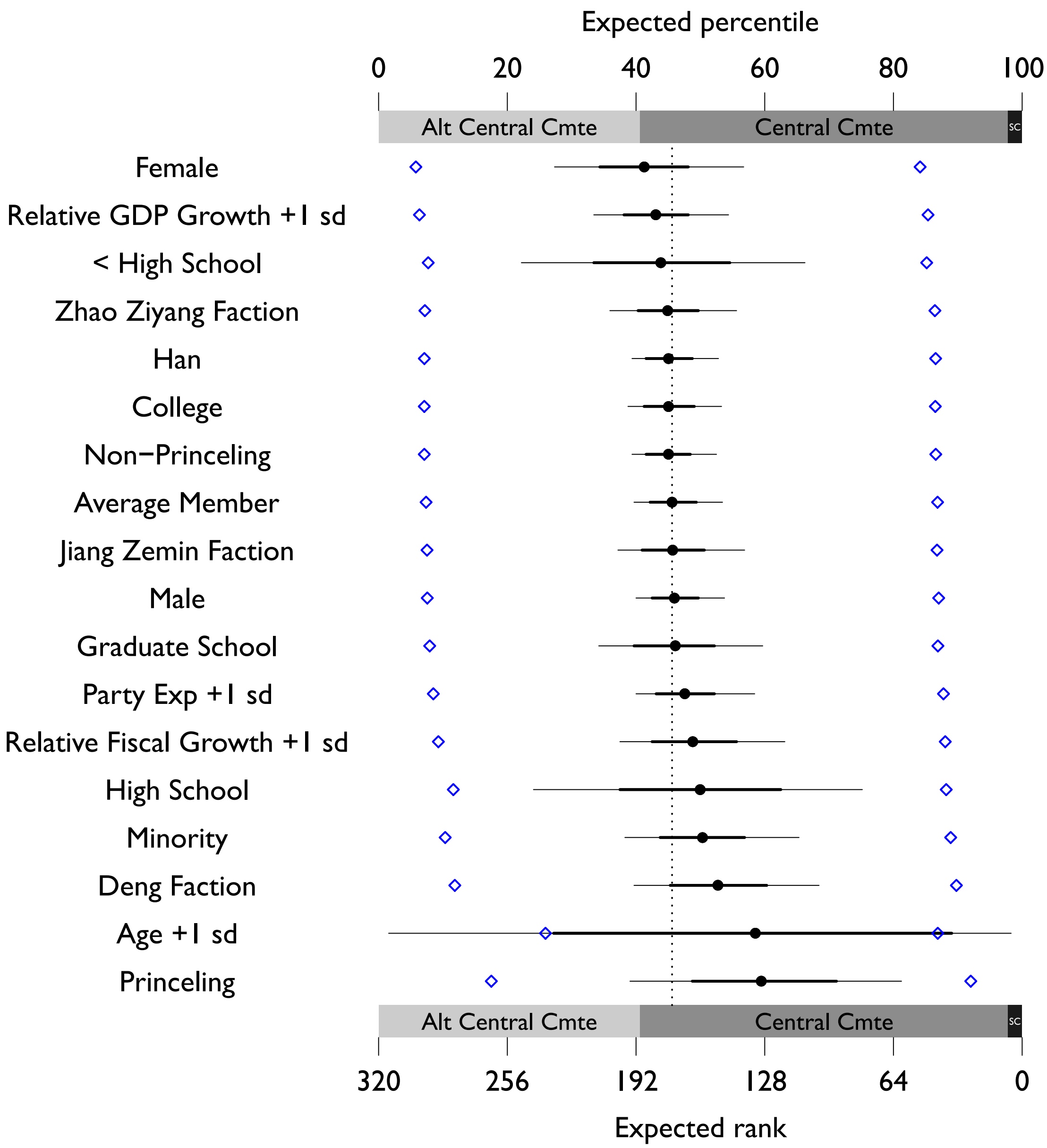

Figure A19: **Formal selection criteria and rank in the 14th Party Congress: Estimated relationships.** For each row of the plot, we set the indicated covariate at one or, for continuous covariates, one standard deviation above the mean, and hold all other variables at their means. The black circles indicate expected rank for an individual with these observed covariates and an average random effect, or degree of unmeasured political ability. Blue diamonds show expected ranks for members with random effects one standard deviation above or below the mean. Thick horizontal lines mark 67% Bayesian confidence intervals and thin horizontal lines indicate 95% Bayesian confidence intervals. The dotted vertical line shows the expected rank of a member with the average covariates. The light gray region indicates the actual ranks of ACC members, the medium gray region shows the Central Committee, and the dark gray region the ranks of the Standing Committee.

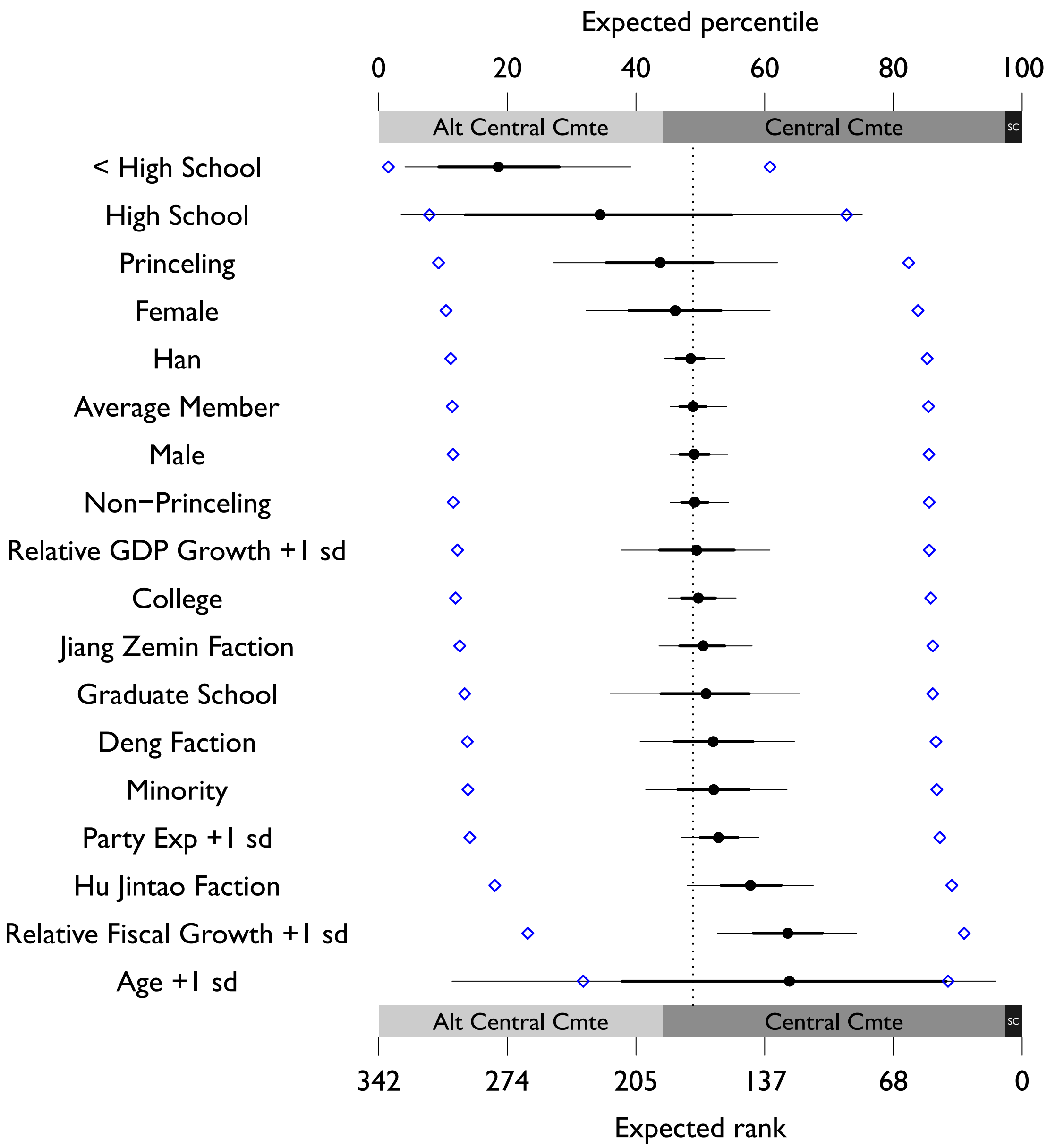

Figure A20: **Formal selection criteria and rank in the 15th Party Congress: Estimated relationships.** For each row of the plot, we set the indicated covariate at one or, for continuous covariates, one standard deviation above the mean, and hold all other variables at their means. The black circles indicate expected rank for an individual with these observed covariates and an average random effect, or degree of unmeasured political ability. Blue diamonds show expected ranks for members with random effects one standard deviation above or below the mean. Thick horizontal lines mark 67% Bayesian confidence intervals and thin horizontal lines indicate 95% Bayesian confidence intervals. The dotted vertical line shows the expected rank of a member with the average covariates. The light gray region indicates the actual ranks of ACC members, the medium gray region shows the Central Committee, and the dark gray region the ranks of the Standing Committee.