Sortition and Cognitive Ability

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Abstact: There is a growing sense that representative democracy is in crisis, leading to renewed interest in alternative institutional designs. One popular proposal—what I call *legislative sortition*—says we should replace elected legislators with randomly selected citizens. While legislative sortition has drawn both numerous supporters and critics, one objection has received little attention: that ordinary citizens' lower cognitive abilities, relative to elected officials, will diminish the quality of governance. This paper articulates and evaluates this concern, distinguishing between several versions of it. I argue that some forms of the objection are implausibly strong, but that a suitably qualified version can be defended. Although this does not provide a decisive reason to reject legislative sortition, it meaningfully shapes how we should assess its promise.

Keywords: sortition; lottocracy; representative democracy; elections; political selection; adverse selection.

1. Introduction

There is a growing sense that representative democracy is in crisis. In response, political philosophers have begun exploring new institutional possibilities to supplement—and in some cases supplant—it. Finding increasing support is *sortition*, or the use of lotteries as a tool of political governance. It is an old idea, dating back to the ancient Greeks (Manin 1997: ch. 1;

Sintomer 2023). Contemporary political philosophers are reviving and reimagining the practice (Umbers 2021: 313). Some advocate using randomly selected citizens primarily in advisory or oversight roles. They might, for instance, be convened to deliberate on a specific policy question; their findings are then presented to the public through a referendum or perhaps submitted as advice to the elected legislature (Farrell and Stone 2020). Alternatively, randomly selected citizens might form "citizen oversight juries" charged with exercising scrutiny over bureaucracies (Bagg 2024).

I am not concerned with these proposed uses of sortition in the current paper. I focus on those who want to replace *legislators* with randomly selected citizens (Barnett and Carty 2008; Zakaras 2010; Guerrero 2014; Van Reybrouck 2016; Vandamme and Verret-Hamelin 2017; Gastil and Wright 2019; Landemore 2020; Abizadeh 2021; Kogelmann 2023; Guerrero 2024; Shoemaker 2024). Instead of electing legislators, the idea is that we select (at least some of) them randomly from the general population. To distinguish it from the more general use of lotteries in politics, call this *legislative sortition*.

Legislative sortition has many advocates, but concrete proposals differ in important ways. For example, some believe we should replace *all* legislators with randomly selected citizens (Guerrero 2014; 2024). More popular is replacing only one chamber of a bicameral legislature with randomly selected citizens, the other chamber constituted by elections (Zakaras 2010; Van Reybrouck 2016; Vandamme and Verret-Hamelin 2017; Gastil and Wright 2019; Abizadeh 2021; Kogelmann 2023). Those who favor a bicameral legislature disagree over the power of the randomly selected chamber: some say it should be limited to a review function (Zakaras 2010: 457), while others say the exact opposite, that its main function should be to initiate new legislation (Vandamme and Verret-Hamelin 2017: 2). Others split the difference and say its primary function should be review, but it can initiate some (i.e., non-financial) types of legislation (Abizadeh 2021: 799). Proponents of legislative sortition also disagree over how long terms should be. Some say three years (Guerrero 2024: 157), while others say four (Abizadeh 2021: 799). One proposal says two years with the option of a second two-year term (Gastil and Wright 2019: 33).

Proposals also differ in terms of surrounding complementary institutions: there is talk of learning phases (Guerrero 2024: 160-161), consultation phases (Guerrero 2024: 164-166), plenary and sub-group deliberative sessions (Vandamme and Verret-Hamelin 2017: 2), "firewalls" to insulate citizen-legislators from elite capture (Abizadeh 2021: 802), secret deliberation and voting (Gastil and Wright 2019: 26-28), and much more. In one of the more ambitious proposals, Alexander Guerrero (2024: 151-153) suggests we replace a single, generalist legislature with multiple specialized ones (e.g., one legislature for foreign affairs, another for health and medicine, and so on). My focus in this paper is on the idea of legislative sortition broadly construed. This, however, does not mean that the differences between the various proposals are irrelevant. Indeed, these differences play a crucial role in my argument below (in §2).

Legislative sortition faces many objections (Lafont 2020; Landa and Pevnick 2021; Umbers 2021; Lafont and Urbinati 2024). And yet, I have found no sustained scholarly investigation of what I believe is the most common reaction to it among those who encounter it for the first time. Guerrero describes it:

> One worry that some have—usually expressed only delicately, in side-conversation after the talk—is that randomly chosen citizens would simply be of inadequate intellectual capacity to make epistemically responsible policy. Expressed bluntly: ordinary people

> aren't smart enough. There is a concern—felt more powerfully by some than by others—that entrusting policy decisions to a randomly selected body of citizens would be a disaster, much worse than using elected representatives, and that it would be a disaster because of the intellectual (cognitive, general intelligence) limitations of ordinary citizens (Guerrero 2024: 275).

The concern touches on sensitive issues—whether meaningful differences in intelligence exist, whether political elites are more intelligent than ordinary citizens, and whether intelligence affects real-world outcomes—and perhaps that is why no one in the scholarly literature has seriously pressed it. My goal in this paper is to move this discussion out of whispered side conversations and into the open arena of scholarly debate over the merits of legislative sortition. Defenders of legislative sortition (and defenders of sortition more generally) deny the concern, or at the very least think it has little force (Zakaras 2010: 465-466; Abizadeh 2021: 801; Bagg 2024: 96; Benson 2024: 212; Guerrero 2024: 275-279). I shall argue that an appropriately qualified version of the concern has merit, and that it should influence our overall assessment of legislative sortition.

Here is the structure of the paper. In the next section, I explore different ways of expressing the worry that ordinary citizens' relatively lower cognitive abilities may undermine effective governance (§2). A strong version of the concern cannot be sustained, I argue, but a weaker version can. This weaker version of the concern can itself be divided into two claims: what I call the *cognitive gap thesis* and the *cognitive impact thesis*. I then present both empirical and theoretical reasons to believe the cognitive gap thesis (§§3–4), as well as empirical and theoretical reasons to believe the cognitive impact thesis (§5). With both claims established, the weaker version of the intelligence-based objection to legislative sortition follows. I then consider an objection to my argument—namely, that the real-world success of *mini-publics* serves as a counterexample (§6). I show that this is not the case, although there are some intelligence-based concerns about legislative sortition for which mini-publics do function as a counterexample. There is a concluding section.

The arguments of this paper should inform our overall assessment of legislative sortition, but not decide it outright, for a few reasons. First, intelligence may not be the only quality we value in legislators. Jeremy Bentham argued that politicians should possess three forms of aptitude: intellectual, moral, and active (Elster 2013: 141). Even if my core claim is correct—that elections are better than random selection at selecting intelligent leaders, and that this matters for good governance—legislative sortition might be better at selecting for moral or active aptitude. These qualities may be more important than intellectual aptitude. Second, there are additional values at stake when comparing political regimes beyond the kinds of leaders they select, including equality, social peace, epistemic performance, and more (Landa and Pevnick 2025b: 36–45). Even if I am right, one could still reasonably favor legislative sortition on the grounds that it better realizes other important values.

The work most closely related to the current paper is Dimitri Landa's and Ryan Pevnick's recent book *Representative Democracy: A Justification*. They argue that one (of many) reasons to favor representative democracy over legislative sortition is that the former "leads to incumbency of individuals who are well suited to exercise political power and, in doing so, helps bring about good policy outcomes" (Landa and Pevnick 2025b: 173). The current paper is consistent with this thesis, but its arguments differ in important ways. First, while Landa and Pevnick (2025b: 170) focus on the "competence and aptitude" of political leaders, I focus narrowly on intelligence. Second, though they "are dubious about the claim that differences in capacity are irrelevant to quality of governance," they set it to the side, whereas I directly argue that differences in the intellectual capacities of politicians do matter for the quality of governance (Landa and Pevnick 2025b: 171). Finally, the selection mechanisms I posit differ from those they posit. They argue that "when properly institutionalized, elections create a quality-tracking property for electoral representation" (Landa and Pevnick 2025b: 173). The various mechanisms I suggest in §§3–5 are distinct from this.

2. Refining the Concern

Would the relatively lower cognitive abilities of randomly selected citizens diminish the quality of governance? This is a common concern among lay audiences—though it is never raised in academic journals—when they first encounter legislative sortition. The concern may seem straightforward, but it is anything but. It contains ambiguous terminology. Moreover, the concern can be stated in different ways, some more plausible than others. The goal of this section is to refine this general worry before I assess its verity.

First, what is meant by terms like "intelligence" and "cognitive ability"? I follow psychologists and understand them in terms of what is called "general factor intelligence," or simply "*g*" (Deary et al. 2010: 201). The idea of general factor intelligence was introduced by Charles Spearman (1904) to explain what is sometimes called "positive manifold," or the fact that aptitude in one cognitive ability predicts aptitude in other cognitive abilities (Neisser et al. 1996: 78; Deary et al. 2010: 201; Jones 2016: 15-16; Kovaks and Conway 2016: 151). For example, those who do well on math standardized tests also tend to do well on exams that test reading, writing, verbal skills, spatial reasoning, mental rotation, and so on. Positive manifold is "perhaps the most replicated result in all of psychology" (Kovaks and Conway 2016: 151). It compels us to posit a single, underlying cognitive capacity—general factor intelligence—to account for these pervasive correlations among all cognitive abilities.

Positive manifold gives us reason to think a general factor of intelligence exists. But what is it? Psychologist Linda S. Gottfredson (1997: 13) defines it as "a very general mental capability that, among other things, involves the ability to reason, plan, solve problems, think abstractly, comprehend complex ideas, learn quickly and learn from experience." Others adopt this definition (Zax and Rhees 2002: 613; Deary et al. 2010: 202; Nisbett et al. 2012: 131). General factor intelligence is measured through a variety of tests, such as Raven's Progressive Matrices, Wechsler tests, and Stanford-Binet (Neisser et al. 1996: 78; Deary et al. 2010: 201; Jones 2016: 20-21). Not only does general factor intelligence explain positive manifold, but it predicts academic achievement, social mobility, occupational performance, (modestly) income, and even health (Jensen 1998: ch. 9; Strenze 2007; Deary et al. 2010: 201; Nisbett et al. 2012: 131; Jones 2016: 28-32). There are good reasons to think general factor intelligence exists. When I speak of "intelligence" or "cognitive ability," this is what I refer to.

The worry is that legislators chosen by lot will have limited cognitive abilities, thereby impairing the quality of governance. Technically, one could level the same charge against legislators selected by election—they too will possess limited cognitive abilities that adversely impact governance quality. Indeed, this seems to be what motivates some of the Confucian meritocrats, who believe we should select political leaders with exalted cognitive abilities via competitive exams (Bell 2006: 168; Bai 2020: 75).[1] I, however, will assume that those who worry about the intelligence of randomly selected legislators do not have similar concerns about the intelligence of elected ones. The concern under investigation thus assumes a meaningful intelligence gap between randomly selected and elected legislatures.

There are a few ways to conceptualize this gap, according to Guerrero. One approach holds that "even the least intelligent elected official must be above some relatively high threshold of intelligence or intellectual capacity, and this is not the case with those elected via random selection" (Guerrero 2024: 276). In other words, there is some threshold of intelligence, *t*. While some members of randomly selected chambers might meet *t*, many will not. By contrast, all members of elected legislatures will meet *t*. This is implausible, argues Guerrero. One need not spend too much time observing Congress to conclude that, if the threshold is defined with any level of rigor, at least some Congressmen don't meet it.

A second way of conceiving the gap holds that "the *average* or *total* intelligence level of the group of political representatives will be notably lower if they are chosen via sortition rather than election" (Guerrero 2024: 276). In other words, if we compare the mean intelligence of elected legislatures to that of randomly selected ones, elected bodies will score higher. This interpretation is more plausible: it does not demand that every elected legislator be highly intelligent. The presence of a few seemingly unintelligent members of Congress does not undercut the claim that, on average, elected legislators are more intelligent than those chosen at random. We can state this claim as follows:

> *Cognitive gap thesis*: Elected legislatures will have higher average intelligence than randomly selected ones.

The cognitive gap thesis is controversial. Guerrero (we shall see in the next section) denies it. Yet even if the thesis were true, that fact alone would not undermine the case for legislative sortition.

To constitute an objection to legislative sortition, one must further show that a legislature's average level of intelligence impacts governance outcomes. If intelligence has no impact on performance, then the truth of the cognitive gap thesis is irrelevant.

More specifically, to make the cognitive gap thesis relevant and constitute an objection to legislative sortition, one must also establish:

> *Cognitive impact thesis*: Higher average intelligence in a legislature has a positive impact on governance outcomes; conversely, lower average intelligence in a legislature has a negative impact on governance outcomes.

While the cognitive gap thesis is a purely descriptive claim, the cognitive impact thesis contains both descriptive and normative elements. It is descriptive insofar as it claims that higher (lower) average intelligence in a legislature has certain downstream policy effects. It is normative insofar as it asserts that these effects are positive (negative). Implicit in the cognitive impact thesis, then, is a procedure-independent normative standard for evaluating governance outcomes. I will have more to say about this in §5.

My aim in this paper is to show that we have strong reasons to believe that both the cognitive gap thesis and the cognitive impact thesis are true. Suppose I am right about this. What would that mean for legislative sortition? One might think that, taken together, these two claims imply the following:

> *Strong intelligence objection*: The lower average intelligence of randomly selected legislatures will lead to worse governance outcomes when compared to elected legislatures.

As its name suggests, this is a strong claim. It asserts that randomly selected legislatures *will* perform worse than elected ones *because* randomly selected legislatures are less intelligent on average.

The claim might be true, but it does not follow from the cognitive gap thesis and the cognitive impact thesis. This is because those who advocate legislative sortition *also* advocate we pair legislature by lot with complementary institutions, such as elected legislatures, single-issue legislatures, learning phases, community consultation phases, plenary and sub-group deliberative sessions, firewalls, secret voting, and more. These complementary institutions may offset the effects of lower average intelligence, allowing randomly selected legislatures to outperform elected ones. An analogy: earning a college degree may tend to increase intelligence, and greater intelligence may tend to increase income. Yet from this it does not immediately follow that people with degrees earn more than those without. Those without degrees may have other traits—such as work ethic, persistence, charisma, etc.—that may offset the advantages of a college degree.

To establish the strong intelligence objection, one must establish a third claim alongside the cognitive gap thesis and cognitive impact thesis. It is:

> *Cognitive supremacy thesis*: The average intelligence of a legislature is a decisive factor in its performance; other institutional features of the legislature cannot offset it.

The cognitive supremacy thesis may be true, but establishing it is difficult, for a few reasons. First, as already noted, advocates of legislative sortition propose we pair randomly selected legislatures with complementary institutions—elected legislatures, single-issue legislatures, learning phases, community consultation phases, plenary and sub-group deliberative sessions, firewalls, secret voting, etc.—that one must demonstrate have no countervailing effect to offset the lower average intelligence of randomly selected chambers. That's a lot of complementary institutions to examine, a time-consuming task.

Not only would one have to show that *all* other proposed complementary institutions have no countervailing effects, but such demonstrations would be highly speculative. Nothing remotely close to legislative sortition has ever been tried. While empirical evidence can help us evaluate the cognitive gap thesis and cognitive impact thesis (as I show in the sections below), no comparable evidence exists to evaluate the cognitive supremacy thesis. This does not mean the thesis is beyond scrutiny, but it can only be explored theoretically—for instance, by constructing formal or informal models of how various forms of legislative sortition might work in practice and then analyzing the outputs of these models (Chung and Kogelmann 2024: §4). Because this kind of inquiry is highly speculative, we should place only low credence in the results of such thought experiments.

For this reason, it is also difficult to show that the cognitive supremacy thesis is false. Establishing its falsity requires showing that at least one of the proposed complementary institutions offsets the effects of lower average intelligence in the legislature; this avoids the numerosity problem. We still, however, run into the speculation problem. In the absence of empirical evidence, all we can do is imagine how specific versions of legislative sortition might work in practice. We can certainly do this, but we should have low confidence in our judgments. Accordingly, the appropriate epistemic stance toward the cognitive supremacy thesis is agnosticism. Since the strong intelligence objection depends on the cognitive supremacy thesis, we should also be agnostic about it.

No convincing argument can be made in defense of the strong intelligence objection, but a weaker version of it can be sustained, or at least I shall argue. In particular:

> *Weak intelligence objection*: The lower average intelligence of randomly selected legislatures will hinder their performance when compared to elected legislatures; other institutional features of randomly selected legislatures may (but need not) offset this deficit.

The weak intelligence objection differs from the strong version in that it does not claim randomly selected legislatures' lower average intelligence will necessarily lead to poor governance outcomes. Instead, it holds that lower average intelligence contributes to poorer performance, while leaving open the possibility that complementary institutions offset this effect, allowing legislatures by lot to outperform elected ones.

Unlike the strong intelligence objection, the weak intelligence objection *does* follow from the cognitive gap thesis and cognitive impact thesis. For all the objection really says is that randomly selected legislatures will have lower average intelligence compared to elected ones (i.e., the cognitive gap thesis), and this will have a negative (but not necessarily decisive) impact on their performance (i.e., the cognitive impact thesis). Thus, if I can establish the cognitive gap thesis and cognitive impact thesis, I establish the weak intelligence objection. That is my focus for the rest of the paper.

The weak intelligence objection is not as forceful as the strong version, but it is far from trivial. For one thing, as we shall see, some advocates of legislative sortition deny it. Beyond this, if the objection is true, it meaningfully affects how we assess the choice between legislative sortition and elected democracy. That choice involves multiple criteria, performance being one

(Landa and Pevnick 2025b: 36-45). If the weak intelligence objection is true, performance-based reasons for favoring legislative sortition are less compelling than they otherwise would be. This does not resolve the debate, but it moves the needle.

3. The Cognitive Gap Thesis, Part I

The cognitive gap thesis says that an elected legislature will have higher average intelligence than a randomly selected one. Guerrero denies it. He writes:

> I am never sure exactly how to respond to this worry. We should be wary of the idea of domain general intelligence at work in the background, but my initial response is even less concessive. Have you seen the people who are elected? On the other side, have you interacted with ordinary people? It is hard to know what standard of intelligence or competence is being deployed such that those elected typically do well by the measure relative to the general population, whether on average, or (somehow) in total (Guerrero 2024: 276).

On the other side of the ledger, there is a longstanding tradition in the history of political thought that supports the cognitive gap thesis. In his history of the concept of representative government, Bernard Manin (1997: 94) shows that elections were not initially regarded as democratic. Their initial purpose was to select individuals with exalted capabilities, such as wisdom (but also wealth and virtue).[2] In this sense they were regarded as aristocratic.

In this section and the next, I argue that the cognitive gap thesis is true. This section considers both empirical and theoretical reasons to believe that elections tend to select leaders who are, on average, more intelligent than the populations they represent. The following section argues that legislative sortition tends to select leaders who are, on average, less intelligent than those they serve. In principle, either claim alone would suffice to establish the cognitive gap thesis. Taken together, however, they suggest that the intelligence gap between randomly selected and elected legislatures is significant.

The cognitive gap thesis is fundamentally an empirical claim. Is there any empirical evidence we can look at? There isn't much, mostly because it's not easy to widely measure intelligence, especially among politicians—what politician would consent to an IQ test? But there are some exceptions.

One revealing study takes advantage of a distinctive feature of Swedish data collection. Since 1901, Sweden has required mandatory military service for men, and the Swedish Defense Recruitment Agency has compiled an extensive database covering nearly all male citizens, including measures of cognitive ability. Drawing on this rich dataset, a group of economists find that politicians are markedly more intelligent than the general population. On a nine-point scale, the average Swedish citizen has a cognitive score of 5.1, while the average member of parliament scores 6.7 (Dal Bó et al. 2017: Table 1). Politicians' cognitive scores align closely with those in other elite professions such as CEOs, lawyers, and academics (Dal Bó et al. 2017: Table 2). Further evidence from Finland, which has a similar system of mandatory military service and also cognitive scores for all men, points in the same direction: "Selection on intelligence in Finland is very similar to selection in Sweden, despite the crucially different electoral systems" (Jokela et al. 2025: 1925).

Perhaps we should not extrapolate from two Nordic democracies to make general claims about the extent to which elections select intelligent leaders.[3] Because it is not easy to collect data on politicians' intelligence, an alternative way to assess the cognitive gap thesis is to identify a proxy for intelligence and then ask whether elections select for it. A natural candidate is education (Gulzar 2021: 266-267; Olav Dahlgaard and Pedersen 2025: 568). Many will be uneasy about equating intelligence with educational achievement, but the relationship between the two is empirically supported (Heckman et al. 2006: 434; Strenze 2007: 415). There's a debate over the direction of causality: whether smarter people get more education, or whether more education makes people smarter (Ritchie and Tucker-Drob 2018: 1358). For our purposes, this is irrelevant. So long as education achievement and intelligence are correlated, we can use the former as a proxy for the latter.

So, do elections select leaders more educated than the populations they serve? Yes. This has been repeatedly empirically confirmed. Looking at 20,000 parliamentarians in 97 countries between 2015 and 2017, 78 percent had a college degree and 40 percent a postgraduate degree. The average for all adults in rich countries is 35 percent and 15 percent in poor countries (*The Economist* 2024). Focusing just on Western European democracies at the beginning of the 21st century, the share of parliamentarians with at least a college degree was very high: 94 percent in Belgium, 74.9 percent in Denmark, 82 percent in France, 82.6 percent in Germany, and over 90 percent in both the Netherlands and the United Kingdom (Bovens and Wille 2017: 114–117). In every case, these figures well exceed the educational attainment of the general population. In the European Parliament, more than 80 percent of representatives hold college degrees, and 26.5 percent have doctorates—again, far surpassing the educational levels of the European public (Bovens and Wille 2017: 118). Speaking generally, "democratic systems are around 20% more likely to select highly educated leaders" when compared to non-democratic political systems (Besley and Reynal-Querol 2011: 552).

The totality of empirical evidence suggests that elections do a reasonably good job of selecting intelligent leaders. Why is this true? There are a few plausible mechanisms. Note first that those who run for office—which includes the winners *and* the losers of elections—are more intelligent and educated than the populations they serve (Olav Dahlgaard and Pedersen 2025: 575; Jokela et al. 2025: 1923). This could be because running for office costs money, and intelligent and educated people are more likely to be wealthier and thus able to pay these costs (Gulzar 2021: 267).

Another explanation is that running for office is cognitively demanding, thereby creating a filter that excludes individuals who lack sufficient cognitive ability. Joseph Schumpeter advanced this view. Although he worried that elections select for traits irrelevant to good governance—such as the ability to win elections (Piano 2025: 137)—he also argued they select for positive attributes as well. “There are,” he observes, “many rocks in the stream that carries politicians to national office which are not entirely ineffective in barring the progress of the moron or the windbag” (Schumpeter 2008: 289). The underlying idea seems to be that mounting a successful campaign requires intelligence, such that only intelligent candidates advance to the stage where voters evaluate them on election day (Kogelmann 2025: 44). Running a campaign requires fundraising, navigating complex legal rules, managing a large team, crafting a coherent policy platform, winning debates, answering tough questions from the media, and cultivating relationships with one’s political party. These are complex and demanding activities that many would struggle to perform effectively. Success in these endeavors requires at least some cognitive ability.

But the fact that democracies tend to select intelligent leaders is not explained solely by the fact that intelligent people run for office with greater frequency. The data suggest that, although both electoral winners and losers tend to be more intelligent and educated than the general population they serve, the winners are, on average, more intelligent and educated than the losers (Arceneaux and Vander Wielen 2023: 2; Olav Dahlgaard and Pedersen 2025: 575; Jokela et al. 2025: 1923). Voters thus play a role. One explanation for how they do so says that voters prefer intelligent leaders and can reliably identify intelligent leaders by observing costly signals of intelligence (Kogelmann 2025: 43-44). A costly signal of trait *x* is one that is difficult for individuals lacking *x* to convincingly fake. In the case of intelligence, such signals might include educational credentials, success in the private sector, strong debate performance, answering tough questions from journalists, and so on.

There are compelling empirical and theoretical reasons to believe elections select political leaders who possess higher average intelligence than the populations they serve. Since a legislature chosen by lot would be a random sample of the broader population, it follows that an elected legislature is likely to display a higher average level of intelligence than one selected at random. This is what the cognitive gap thesis asserts.

4. The Cognitive Gap Thesis, Part II

In the previous section, I argued that there are good empirical and theoretical reasons to accept the cognitive gap thesis. I did so by examining evidence and mechanisms suggesting that elections tend to select leaders who are more intelligent than the populations they serve. I assumed the average level of intelligence in a randomly selected legislature would mirror that of the broader population. This may have been too generous. In this section, I present a theoretical argument for why legislative sortition is likely to face an adverse selection problem. Specifically, we should expect randomly selected legislatures to exhibit levels of intelligence lower than the population average. In terms of intelligence, there are reasons to think randomly selected legislatures will be *worse than average*. The upshot is that we now have a second reason to endorse the cognitive gap thesis: not only do elections positively select for intelligence, but legislature by lot negatively selects for it.

To set the stage, I begin with a class of political economy models that examine how incentives shape who enters politics. These models suggest that certain incentive structures can discourage the kinds of individuals we hope to see in office from seeking it, while attracting those we might prefer to stay out (e.g., Besley 2004; Messner and Polborn 2004; Mattozzi and Merlo 2008). Scholars propose several mechanisms explaining this adverse selection problem. For the remainder of this section, I focus on the model developed by Francesco Caselli and Massimo Morelli (2004), which highlights how *compensation* influences who chooses to run for political office.

We begin with two assumptions. First, we want intelligent people to serve in public office, because they have a positive impact on governance outcomes. This is just a restatement of the cognitive impact thesis. I have yet to defend the thesis—that is reserved for the next section—but assume for now that such a defense can successfully be made. Second, the intelligent individuals we would like to serve in public office have attractive outside options in the private sector; they command high private-sector wages. Why believe this assumption? Empirical evidence indicates that both intelligence (Strenze 2007: 415) and education (Autor 2014: 844) are correlated with higher earnings in the labor market. Hence, those we would most like to see serve in public office—namely, smart people—often have appealing vocational options elsewhere.

Because of this, intelligent people will be hesitant to pursue public office unless the compensation is sufficiently high to offset their opportunity costs. If compensation for serving in office is low, then it will be relatively less intelligent individuals attracted to politics, because their opportunity costs in the private sector are not as high. As Caselli and Morelli (2004: 760) state the logic: “bad politicians win because rewards from office are too low to induce potentially good politicians to run.” Empirical evidence supports this dynamic. Raising compensation induces more educated individuals to run for office (Ferraz and Finan 2009; Gagliarducci and Nannicini 2013).

Political compensation is more complex than compensation in the private sector, as it takes multiple forms beyond salary. Specifically:

- Politicians receive formal compensation (Besley 2005: 53). This includes salary and also benefits like travel allowances. Members of the U.S. Congress (in both the House and Senate) make $174,000 a year. Their estimated benefits package is quite high, an additional $200,000 in the case of senators and between $20,000 and $35,000 for members of the House (Diermeier et al. 2005: 365).
- Politicians receive post-office rents (Besley 2005: 53). They often have lucrative career opportunities after their time in office is done. It is a cliché that many politicians become lobbyists after they serve, and this is often true (Lazarus et al. 2016: 85). Politicians also frequently join corporate boards after they retire from office (Eggers and Hainmueller 2009; Palmer and Schneer 2016; 2019).
- Politicians receive esteem (Caselli and Morelli 2004: 760; Brennan and Pettit 2005). They are famous. In many cases, they are admired. We name streets, buildings, and airports after them. The pursuit of esteem can be a major factor for why some enter

politics (Adair 1998: ch. 1). Alexander Hamilton wrote in *The Federalist Papers* that the "love of fame" is "the ruling passion of the noblest minds, which would prompt a man to plan and undertake extensive and arduous enterprises for the public benefit" (Hamilton et al. 2001: 375).[4] Or, as Louis IV put it to justify his invasion of the Dutch Republic: "A king need never be ashamed of seeking fame, for it is a good that must be ceaselessly and avidly desired" (Blanning 2007: 540).

Taken together, these three forms of remuneration constitute what I shall call a politician's *total compensation*. The lesson from the Caselli and Morelli paper is that to attract intelligent people to politics, the total compensation package must be sufficiently high to compete with their opportunities in the private sector.

The same logic applies to legislative sortition. Most of its defenders believe participation should be voluntary (Zakaras 2010: 457; Vandamme and Verret-Hamelin 2017: 11; Abizadeh 2021: 799; Guerrero 2024: 159). If someone is selected but doesn't want to serve, they can turn down the invitation. To attract smart people away from their lucrative careers, compensation must be high. But it will not be, I now argue. As a result, the intelligent will opt out of service, while less intelligent individuals with fewer outside options will opt in. Because the intelligent select out, the average intelligence of a randomly selected legislature will fall below that of the population it serves.

One component of total compensation disappears completely under legislative sortition. First, randomly selected legislators will not collect post-office rents. Elected officials often secure lucrative roles after leaving office—such as positions in lobbying firms or on corporate boards—because of the networks they build while in office (Vidal et al. 2012; McCrain 2018). These networks are valuable. Firms want access to sitting politicians to both influence them and learn about their thought processes. Legislature by lot undermines this. Terms are too short to build meaningful relationships; robust networks form over decades in office. Moreover, even if relationships develop, they offer little long-term value, for one's fellow citizen-legislators will exit office around the same time. A few years after your service is complete, you will know no one currently serving. Legislative sortition does not generate the social capital necessary to produce post-office rents.[5]

A second component of total compensation—esteem—will be greatly attenuated. Those who accept the invitation to serve will probably be thought well of by their friends and family for taking on burdensome civil service. But they will not achieve the fame elected politicians often achieve, for a few reasons. First, their terms are too short to become household names. Try this: write down ten currently serving members of Congress. My guess is that most of the people you wrote down have been serving a decade or more (e.g., Nancy Pelosi, Mitch McConnell), not a few years. To become famous, one must serve in politics for a long time. Second, legislative sortition offers less opportunities to gain notoriety. Randomly selected legislators may still debate on the floor of the legislature,[6] but they won't run election campaigns. But campaigning is where politicians build public recognition, through rallies, televised debates, appearances at county fairs, and constant media exposure. Without this performative dimension, randomly selected legislators won't attract significant public attention. Esteem will thus be a very small component of randomly selected legislators' total compensation.[7]

So, one component of total compensation is eliminated completely, and another is significantly weakened. To ensure intelligent individuals don't select out of service to pursue lucrative private-sector opportunities, the third component of total compensation—salary—must be exceptional. Will it be? Advocates of legislative sortition argue salaries should be high for precisely this reason (Van Reybrouck 2016: 138; Abizadeh 2021: 800; Guerrero 2024: 159). Guerrero (2024: 159), for instance, says we should pay citizen-legislators 110 percent of their current salary.[8] If we do, then doctors, lawyers, scientists, entrepreneurs, and other highly intelligent people will not have to take a pay cut to serve. Will this be enough to convince individuals with high private-sector opportunity costs to serve?

I have doubts, for a few reasons. First, I am skeptical that a randomly selected legislature would actually adopt Guerrero's proposed pay scheme. This raises the issue of whether compensation falls within the authority of randomly selected legislatures or whether it belongs to the fundamental constitutional order and is thus beyond revision. Following John Rawls's (2005: 231) notion of "higher law," we might assume that certain constitutional principles—such as the impermissibility of abolishing legislative sortition itself—should remain outside the reach of citizen-legislators. Yet the democratic ethos underlying legislative sortition suggests that very few matters should be off-limits. Many elected legislatures determine their own salaries, though some rely on independent commissions or link compensation to external benchmarks.[9] It would be odd if a randomly selected legislature were less democratic than the modal elected one. If we take the democratic aspirations of legislative sortition seriously, it is difficult to see why compensation should be determined *ex ante* by philosophers rather than determined by those selected by lot.

If randomly selected legislatures set their own pay, I am skeptical they would adopt Guerrero's proposed scheme, for it will result in vastly unequal pay for equal work. A janitor making $40,000 a year before being selected will make $44,000 in the chamber. A doctor making $400,000 a year before being selected will make $440,000 in the chamber for doing the exact same job as the janitor. There are good reasons for this inequality—the doctor would probably not serve if she were not offered the exorbitant salary—but a randomly selected group of individuals may not understand, and may not be moved by, this logic. Empirical work shows that normal people are less tolerant of pay inequality when compared to professional economists (Sapienza and Zingales 2013: 638; Kiatpongsan and Norton 2014). This is probably because professional economists think in terms of efficiency and incentives, while normal people think in terms of fairness (Haferkamp et al. 2009). The rationale for Guerrero's proposed pay scheme is incentive-based, not fairness-based. We should not be surprised if a randomly selected group of individuals rejected it.

Salaries will probably be egalitarian. If they are too low, then smart people will refuse service because their opportunity costs are too high. Perhaps, then, citizen-legislators will set a very high but nonetheless egalitarian wage for service—*every* citizen-legislator makes $440,000 a year, not just the doctor. This proposal is unrealistic. Citizen-legislators will probably be reluctant to assign themselves exorbitantly high salaries. In many elected legislatures, politicians' salaries are quite low relative to their opportunity costs. This is because many elected legislatures control their own salaries, and politicians raising their own salaries is profoundly unpopular (Cullen 2011). In 1816 Congress passed an act abandoning the $6 daily per diem in favor of an annual salary of $1,500; roughly two-thirds of the House of Representatives lost their seat in the next election as a result (United States Senate, n.d.). Randomly selected legislators don't face voters, but they probably care at least somewhat about what the public thinks of them. For this reason, granting themselves a salary high enough to ensure smart people choose to serve—which is likely orders of magnitude above the median income of the population they are selected from—is unlikely.

If randomly selected citizens oversee compensation—and recall, this is consistent with the democratic ethos of legislative sortition—then we should expect an egalitarian pay structure that is too low to entice intelligent individuals to serve. Suppose, however, that the compensation scheme cannot be altered by randomly selected legislatures. It is part of the fundamental constitutional framework that citizen-legislators receive 110 percent of their current salary while serving, as Guerrero proposes. Yet even here there are reasons to doubt this will be sufficient to attract highly intelligent individuals.

To begin, there is evidence that the impact of intelligence on earnings increases with age (Zax and Rhees 2002: 609; Jones 2016: 31; Lin et al. 2018: 122). For example, if *A* is more intelligent than *B*, we should expect, all else equal, that *A* will earn more than *B* at age 30, and that the gap will be even larger at age 50. Why this occurs is not entirely clear, but one plausible explanation is that more intelligent individuals have greater opportunities for career advancement than less intelligent individuals. For instance, *A* and *B* might start at the same firm in identical positions. Because *A* is more intelligent, she might eventually become a vice president; because *B* is less intelligent, his ceiling may be mid-level management.

If this is correct, then 110 percent of one's current salary might still be insufficient to entice highly intelligent individuals to serve, since this compensation fails to offset the opportunity cost of forgoing career advancement. When *A* steps away from her career to serve, she reduces her chances of eventually becoming vice president, since such promotions depend on her performance at the firm. By contrast, because *B*'s advancement opportunities are more limited, he sacrifices comparatively less by stepping away for three years. In this way, even compensating individuals at 110 percent of their current salary can produce adverse selection, because the payment offsets only *present* earnings, not *future* earnings, and more intelligent people have greater future earnings.

To summarize: legislative sortition suffers from an adverse selection problem. Intelligent individuals will opt out of serving in a randomly selected legislature. Why? Because post-office rents are eliminated, and esteem significantly weakened. Salaries alone are unlikely to be high enough to convince smart people to serve. A randomly selected chamber would probably not adopt Guerrero's proposed pay scheme. Even if it were constitutionally mandated, the pay scheme may still fail to persuade highly intelligent individuals to forgo career advancement. Taken together, these factors suggest that legislatures chosen by lot will, on average, be less intelligent than the populations they serve.[10] This gives us a second reason to believe the cognitive gap thesis is true. When coupled with the argument from the section above, we should expect the intelligence gap between randomly selected legislatures and elected ones to be quite significant.

Elected legislatures avoid this adverse selection in a few ways.[11] First, though elected legislatures do not offer terribly high salaries, they reward service with post-office rents and esteem. The total compensation package is thus quite high, attracting smart people. Moreover, elected legislatures handle the career advancement problem by being *outlets* for it. As Schumpeter (2008: 285) notes, "we must recognize, in modern democracies … politics will unavoidably be a career. This in turn spells recognition of a distinct professional interest in the individual politician." Highly intelligent individuals don't have to give up career advancement to serve in politics. Serving in politics is precisely how some of them pursue career advancement (Schlesinger 1966; Black 1972).

5. The Cognitive Impact Thesis

In the last two sections, I presented arguments in defense of the cognitive gap thesis. Even if these arguments succeed, they are not yet sufficient to establish the weak intelligence objection. To do so, I must also establish the cognitive impact thesis, which holds that higher (lower) average intelligence within a legislature has a positive (negative) effect on governance outcomes. That is the goal of the current section.

The cognitive impact thesis contains both descriptive and normative elements. It is descriptive in that it claims higher (lower) average intelligence within a legislature has certain downstream effects on policy. It is normative in that it maintains these effects are positive (negative). Consequently, the cognitive impact thesis cannot be established without also identifying a procedure-independent normative standard by which to evaluate governance outcomes.

There are two ways to do this. The first is to define a procedure-independent normative standard and then seek empirical and theoretical evidence showing that higher (lower) intelligence within a legislature is more likely to produce policies that succeed (fail) according to it. The disadvantage of this approach is that it prevents us from drawing on existing evidence that already demonstrates a relationship between legislative intelligence and governance outcomes. I want to use this literature, so I adopt a second approach: first examine the existing evidence and then argue that the ways in which intelligence has been shown to influence governance outcomes are consistent with plausible procedure-independent normative standards. In other words, first identify the demonstrated effects of higher (lower) average intelligence on governance, then argue they are positive (negative).

Let's turn to the empirical evidence showing that political leaders' intelligence affects governance outcomes, setting aside for the moment whether these impacts are normatively good or bad. Consider the following:[12]

- Looking at a sample of over 1,000 national-level political leaders across the globe between 1875 and 2004, economic "growth in general is higher when leaders are more highly educated" (Besley et al. 2011: F205).
- In the United States, more educated presidents produce better economic outcomes (Congleton and Zhang 2013).
- In Indian state legislatures, "election of graduate legislators result in around 0.3 standard deviation increase in access to both roads and power supply. Similarly, the percentage of households that have access to electricity as the main source of lighting is 5.3 percentage points higher if a graduate candidate is elected" (Jain et al. 2023: 1069-1070).
- In Indian state legislatures, "in the most developed states, having more educated leaders results in significantly higher learning levels (reading, maths and English scores) among children in the district" (Lahoti and Sahoo 2020: 44).
- In India, "electing a Postgraduate candidate through a close election leads to a reduction in the prevalence of anemia among women by 8 percentage points, greater access to clean cooking fuel by 7.7 percentage points and growth in internet users by 12 percentage points" (Jaiswal 2025: 1-2).
- In Indonesia, more educated politicians "led to important increases in the provision of key public goods: number of doctors, presence of primary health care centers, and access to basic services, such as safe drinking water and garbage disposal" (Martinez-Bravo 2017: 257).

- In Italy, mayors "with higher education (defined as holding a university or technical degree or above) increase public investment, especially in the education sector," in a manner that "does not compromise fiscal stability, as the fiscal deficit remains statistically indistinguishable from zero" (Mitra 2025: 3).
- At the local government level in Norway, "larger shares of highly educated politicians generate a higher level of efficiency" in terms of public service provision (Sørensen 2023: 164).
- At the local government level in Finland, "an increase in university-educated politician's representation leads to an increase in the size of the public sector. Nevertheless, there do not seem to be any negative impacts on fiscal health" (Meriläinen 2022: 3021)
- In European states between the 10th and 18th centuries, monarchs with "a one-std increase in ruler ability leads to a 0.8 std higher broad *State Performance*, to an expansion in territory by 16%, and to an increase in urban population by 14%" (Ottinger and Voigtlander 2025: 97).[13]

Importantly, these studies do not merely document correlations but employ strategies to identify causal effects. To do this, most focus on very close elections, in which an educated candidate narrowly wins or loses against a less educated opponent. The logic is that when the margin is extremely narrow, the election outcome likely hinges on an essentially random shock, such as slight fluctuations in turnout, local events, or last-minute news exposure. The method is common among social scientists (Cuesta and Imai 2016). Treating these razor-thin contests as "as good as random" enables researchers to compare just-winners to just-losers in a way that approximates a natural experiment.

There are, then, good reasons to think the descriptive component of the cognitive impact thesis is true: political leaders' intelligence impacts governance outcomes. But what is the normative status of these effects?

Let's first look at the studies by Besley et al. and Congleton and Zhang, which show that more educated leaders result in more economic growth and better economic performance generally. There are compelling normative reasons to think growth is good (Kogelmann 2022). Although once questioned by the "Easterlin paradox," evidence now shows that rising income reliably increases happiness with no clear satiation point (Stevenson and Wolfers 2013). Even if one is skeptical of subjective accounts of well-being, growth produces goods typically featured on objective list theories of well-being—longer lifespans, better health, more literacy, more education, etc. (Moller 2011: 187). Growth also strengthens political institutions: it can spur demands for democracy (Moore 1993: 418) and makes democratic backsliding less likely (Przeworski 2019: 33). Prosperity may also foster tolerance, as wealthier societies are less inclined toward zero-sum thinking (Friedman 2005: ch. 4).

Another tranche of studies—those by Jain et al., Lahoti and Sahoo, Jaiswal, Martinez-Bravo, and Mitra—show that educated leaders increase access to basic goods such as food, roads, electricity, internet, healthcare, and education. Most normative theories of distributive justice will regard such outcomes as good. For instance, a Rawlsian egalitarian would consider these as examples of *primary goods*—the resources every individual needs to pursue their conception of a good life (Rawls 1971: 92). A defender of the capabilities approach would argue that these basic goods are essential capabilities all individuals need to flourish (Nussbaum 2011: ch. 2). In fact, the only distributive theories of justice that would *not* regard increased access to these goods as normatively desirable are historical theories, which focus not on what people have, but on how they acquired it (Nozick 1974: 151).

Continuing, some studies—those by Sørensen and Meriläinen—show that educated leaders enhance the efficiency of public goods provision, meaning they deliver more services at lower cost. In general, economic efficiency is a relatively anodyne normative standard (Heath 2020: 9-12); it is difficult to object to getting more for less. Of course, efficiency alone does not exhaust the normative landscape, but that should not stop us from claiming it is nonetheless part of it (Rawls 1971: 70-71). The final study—by Ottinger and Voigtlander—measures the impact of political leaders' intelligence on territory acquisition and urbanization. While I don't think territorial acquisition is necessarily normatively good (but neither is it necessarily normatively bad, a lot depends on the justification for acquisition), a case can be made for urbanization: cities make us richer, smarter, greener, happier, and healthier (Glaeser 2012). These all seem to be good things.

So, not only does higher average intelligence impact governance outcomes, but there are reasons to think these impacts are good. This is exactly what the cognitive impact thesis says. Hence, we have good reason to believe the cognitive impact thesis. Coupled with the cognitive gap thesis, it implies the weak intelligence objection.

To close, it's worth asking: why does higher average intelligence have such a positive impact on governance outcomes? There is no clear answer, but several hypotheses are available. One possibility is that more intelligent individuals simply have a better grasp of policy issues than their less intelligent counterparts. They understand problems more deeply, see a wider range of solutions, better anticipate unintended consequences, are more adept at thinking about how various policies fit together to form a coherent whole, etc. Beyond this, substantial empirical evidence shows that more intelligent individuals have lower discount rates—they prioritize the future over the present to a greater extent than less intelligent individuals (Dohmen et al. 2010; Benjamin et al. 2013; Jones 2016: ch. 4). This is so even after controlling for education, income, and the like. This could help explain some of the observed links between intelligence in legislatures and fiscal sustainability.

There is also strong empirical evidence that smarter people are more cooperative: they are more likely to cooperate in repeated prisoners' dilemmas, public goods games, and similar contexts (Burks et al. 2009; Burnham et al. 2009; Jones 2016: ch. 5). Greater cooperation within a legislature—whether among members of the same coalition or across the partisan aisle—could plausibly lead to better legislation. Finally, it may be that smarter individuals hold preferences that, when they influence policy, produce good governance outcomes. For example, intelligent people may simply prefer more education. More education investment may lead to less crime, less poverty, more economic growth, and so on.

6. Are Mini-Publics a Counterexample?

A natural worry about legislative sortition is that the relatively lower cognitive abilities of randomly selected citizens would diminish the quality of governance. There are good reasons, I have argued, to believe a suitably qualified version of this objection succeeds, though strong versions of it do not. How might the defender of legislative sortition respond? They could argue that I have overlooked an entire body of evidence. Although legislative sortition has never been tried, there has been growing experimentation with *mini-publics*. One might reasonably think these experiments demonstrate that ordinary citizens' cognitive abilities do not adversely impact governance quality. Jonathan Benson articulates this line of reasoning in his recent book, *Intelligent Democracy*. In his words:

> Perhaps the greatest achievement of experiments with mini-publics has been to reply to the objection that a random group of lay citizens cannot intelligently and competently discuss issues of public policy. Many of those who are skeptical of sortition question whether the assessment of policy is a too cognitively burdensome task for the average citizen … The evidence coming from mini-publics, however, rejects these concerns (Benson 2024: 212).[14]

The idea, then, is that the performance of real-world mini-publics allows us to dismiss intelligence objections to legislative sortition. The current section responds to this line of argument.

First, what are mini-publics? They are "small groups of randomly selected citizens, operating according to deliberative principles (including facilitated small-group discussions) and tasked with considering one or a number of important policy, institutional, or constitutional reform issues" (Farrell and Stone 2020: 229). Examples include the British Columbia Citizens' Assembly, which met in 2004 to investigate possible changes to the provincial electoral system; the Netherlands' *BurgerForum*, which met in 2006 to examine the country's electoral system; and the Irish Citizens' Assembly, which met from 2016 to 2018 to deliberate on several political questions, one of which was abortion (Farrell and Stone 2020: Table 11.2). Most mini-publics focus on a single policy question, though there are exceptions (Benson 2024: 211). Their decisions are not binding. Outcomes are typically presented either as recommendations to the public through referendums or as reports submitted to an elected legislature (Farrell and Stone 2020: Table 11.2).

Some are deeply impressed by mini-publics. To be impressed by them implies that we are judging their outputs by a procedure-independent normative standard. I grant whatever procedure-independent standard the proponent of this objection embraces, and I accept that mini-publics, on the whole, have performed well according to it. Even so, the success of mini-publics does not falsify this paper's core arguments.

The success of mini-publics does not undermine the cognitive gap thesis, for it is a comparative claim about the traits of individuals who constitute elected versus randomly selected legislatures. The thesis says nothing about governance outcomes at all. Nor does it falsify the cognitive impact thesis, which asserts only that higher (lower) average intelligence positively (negatively) influences governance outcomes. The thesis does not imply that good governance is impossible without high average intelligence, nor does it say that low average intelligence necessarily results in poor performance. Because mini-publics do not act as a counterexample to either the cognitive gap thesis or the cognitive impact thesis, and because the weak intelligence objection follows from these two claims, mini-publics do not act as a counterexample to the weak intelligence objection.

In fact, the success of mini-publics does not even act as a counterexample to the strong intelligence objection—which, recall, I argued we should be agnostic about. The strong intelligence objection holds that the lower average intelligence of randomly selected legislatures will lead to worse governance outcomes compared to elected legislatures. Successful mini-publics cannot refute this claim because it is comparative in nature. It does not assert that randomly selected legislatures will perform poorly, only that they will perform worse than elected ones. No matter how well mini-publics perform, we simply do not know how equivalent bodies—identical in every respect except that they are constituted through election—would perform. Elected legislatures are not a fair comparison, for they are tasked with very different functions than mini-publics. For mini-publics to contradict the strong intelligence objection, we would need to compare their performance directly with that of bodies identical in every respect except they are constituted by elections.

What kind of intelligence-based objection to legislative sortition do mini-publics act as a counterexample to? Consider the following:

> *Cognitive collapse thesis*: The lower average intelligence of randomly selected legislatures will lead to extremely poor governance outcomes.

The cognitive collapse thesis might be true, but I will not defend it, as I can think of no theoretical or empirical considerations that support it. Does the success of mini-publics at least act as a counterexample to this claim?

Mini-publics offer *some* evidence that the cognitive collapse thesis is false, but this evidence is inconclusive. As noted above, mini-publics and legislatures differ in many important ways. Most mini-publics are tasked with addressing a single, narrow policy question (Benson 2024: 211). Their recommendations are advisory rather than binding (Farrell and Stone 2020: Table 11.2), which reduces the incentives for outside actors to influence their deliberations and decisions. Once citizen-legislators are required to consider multiple policy issues at once, and once there are stronger incentives to influence them, the performance of ordinary citizens might deteriorate significantly. We simply do not know. Nonetheless, I grant that mini-publics provide some reason to believe the cognitive collapse thesis is false.

Mini-publics, then, act as counterexamples to very few intelligence-based objections to legislative sortition. They do not challenge the core claim I defended in this paper (the weak intelligence objection). They do not even touch a claim I regarded as too strong to defend (the strong intelligence objection). At most, they give us some reason to doubt an extraordinarily strong claim (the cognitive collapse thesis).

7. Conclusion

Many political philosophers have argued that legislators should be selected by lot rather than by elections—a practice I refer to as legislative sortition. A natural concern is that the relatively lower cognitive abilities of randomly selected citizens may impair the quality of governance. Defenders of legislative sortition dismiss this concern, but I have shown there is something to it. The concern can be stated in many ways. While its stronger versions are untenable, a suitably qualified version succeeds.

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[1] For an overview and critique of the new meritocrats, see Kogelmann (2025); Landa and Pevnick (2025a).

[2] See also Herreros (2006).

[3] This is Guerrero's (2024: 277) response, who is aware of the Dal Bó et al. (2017) study, but not the Jokela et al. (2025) one.

[4] In general, the Founding Fathers thought the desire for esteem was an important part of human nature. John Adams (2004: 347) writes in *Discourses on Davila* that "the same ardent desire of the *congratulations* of others in our joys, is the great incentive to the pursuit of honors. This might be exemplified in the career of civil and political life."

[5] One might respond: participants in a randomly selected chamber could still secure post-office rents. Indeed, Landa and Pevnick raise this as a criticism of legislative sortition, although in terms of selecting for intelligence it is probably a benefit. They write: "there is reason to worry that lottocratic systems would be afflicted by a particularly pernicious version of the revolving door problem. If a wealthy trade group forges a reputation for handsomely rewarding former legislators (say, by offering them lucrative positions after they leave office), this is very likely to affect the behavior of future legislators who wish to capitalize on the good fortune of being selected for public office" (Landa and Pevnick 2021: 55-56). The idea, then, is that post-office rents would persist even in randomly selected legislatures.

I am skeptical. Landa and Pevnick, I think, misunderstand the nature of the revolving door. They seem to treat it as a form of delayed bribery: individuals receive lucrative jobs after leaving office because they served the interests of their future employers while in office. Yet the best empirical evidence suggests otherwise. The revolving door is not about bribery, but social capital. Specifically, both the likelihood of securing a lobbying position and the salary one commands as a lobbyist are strongly correlated with the size of one's professional network (Vidal et al. 2012; McCrain 2018). If the revolving door were just a system of delayed bribery, the size of one's current network would be irrelevant. That it is so strongly predictive instead indicates that the revolving door operates through social capital—something participants in randomly selected chambers would possess almost none of.

[6] Although Gastil and Wright (2019: 26-28) argue that much of this deliberation should be secret, making the esteem problem even worse for their specific proposal.

[7] Things will be even worse under Guerrero's proposed scheme of many singe-issue legislatures. This is because there are too many of them for anyone to stand out and gain notoriety. He recommends 450 people for each single-issue legislature (Guerrero 2024: 157). He recommends 20-30 permanent legislative bodies (Guerrero 2024: 238). Assume there are 25. That's 11,250 randomly selected legislators at a time, which is far too many for anyone to stand out and gain recognition.

[8] Zakaras (2010: 458) and Gastil and Wright (2019: 19) suggest a salary of double the median household income. This would probably be insufficient to get doctors, lawyers, scientists, entrepreneurs, and other highly intelligent people to serve.

[9] See the National Conference of State Legislatures (2022).

[10] To be clear, I do not mean to suggest that civic duty plays no motivational role at all. Undoubtedly, some highly intelligent individuals would serve even when doing so offers little personal benefit. For my argument to succeed, I need only assume that civic duty does not *always* override considerations of compensation. If this assumption were false, then everything I have said would be irrelevant, since no one would ever refuse service for compensatory reasons.

Yet it seems entirely plausible that considerations of compensation prevail at least some of the time, which is all my argument requires.

To see this, suppose that only ten percent of the population—randomly distributed across the intelligence distribution—regards compensation as more important than civic duty. Not everyone in this ten percent would refuse service if asked, because service might not require them to sacrifice present or future earnings. Only those within this small group who have high opportunity costs would refuse. This, I have argued, is more likely to be smart people, leading to their underrepresentation.

[11] Note that I am claiming elected legislatures do not face an adverse selection problem with respect to *intelligence* specifically. They may, however, face such a problem regarding other traits—such as Bentham's moral or active aptitude. This paper, however, focuses solely on the extent to which different regime types select for intelligence, and how this impacts governance.

[12] Not everyone finds a positive relationship between intelligence and governance outcomes. See Carnes and Lupu (2016).

[13] "Ruler ability" refers to general cognitive ability. Cognitive ability for monarchs is estimated by looking at rates of consanguinity, because consanguinity is negatively correlated with IQ. Thus, leaders with less consanguinity in their family tree probably had higher IQ's and thus higher cognitive ability

[14] For similar remarks, see also Zakaras (2010: 466); Van Reybrouck (2016: 121); Abizadeh (2021: 801).

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