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ORIGINAL RESEARCH

Bureaucratic science: a public choice analysis of gatekeeping during COVID-19

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Abstract

This paper examines scientific gatekeeping during the COVID-19 pandemic through two key episodes: the suppression of the Great Barrington Declaration's critique of non-pharmaceutical interventions (NPIs) and the pre-mature, prejudicial dismissal of the lab leak hypothesis regarding SARS-CoV-2's origins. Drawing on public choice theory, I argue that scientist-bureaucrats' gatekeeping behaviors were motivated not solely by epistemic goals or public good, but by three distinct incentives: enhancing public perception of their importance, increasing political influence, and protecting captured resources. Analysis of communications between key figures like Fauci, Collins, and Anderson, along with the history of early 21st century pandemic preparedness debates in the United States, reveals discrepancies between private uncertainties and public pronouncements. The paper proposes a "public choice philosophy of science" framework to understand how scientist-bureaucrats operate when their avowed goal is advancing public good rather than knowledge. This approach helps explain why scientific disagreement was suppressed during the pandemic despite its value for both scientific progress and public trust.

Keywords Covid-19 · Gatekeeping · Public choice · Incentives

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I feel they clearly misled me early on.

I'm disappointed, both in them and in myself, that I was so easily taken in. On the other hand, it's one thing to be lied to by a politician and fail to check it out. But on viral evolution, to whom do you go for a second opinion? At their level, there are precious few experts. If Albert Einstein assured you that nuclear fission is harmless, whom would you trust to quote saying, "Einstein's dead wrong? (Donald G. McNeil Jr, Science and Health reporter for The New York Times, and one of its most prominent reporters on Covid-19 (2024)).

1 Introduction

In keeping with the topic of this topical collection, this paper examines gatekeeping in science, specifically focusing on two episodes that occurred during the Covid-19 pandemic. While building upon previous work on gatekeeping in science—gatekeeping is boundary work that polices what counts as inside and outside of properly scientific activity in science—this paper takes a different approach from the formative work of Dormandy and Grimley (2024). Their paper conceptualizes gatekeeping in science from a normative perspective, considering an idealized scientific practice. They argue that ideal gatekeeping should preserve the "ideals of science." What they seem to mean is that it should facilitate various goals we ordinarily associate with science, among which they include "seeking understanding, striving for accuracy or empirical adequacy (or, for anti-realists, practical efficacy); substantiating results with arguments and other evidence; subjecting results and methods to critical feedback by peers, and engaging in ongoing epistemic self-reflection" (393). Dormandy and Grimley posit that effective gatekeeping in this idealized context should strike a balance between being neither too tight nor too loose.

In contrast, this paper examines gatekeeping as it occurs in actual scientific practice, where scientists' incentives and motivations may be influenced by factors beyond purely epistemic aims. While acknowledging the value of Dormandy and Grimley's normative approach, this study differs in two ways. First, it offers a more expansive normative ideal of scientific gatekeeping, so that it encompasses not only cognitive goals, but also goals aimed at the public good. That is, I want to allow that ideal gatekeeping might serve not only to maximize things like accuracy and understanding, but also to maximize more practical public interests. Good gatekeeping could be in the service of keeping scientists from wasting cognitive resources trying to understand fraudulent data, but it could also, in the eyes of the gatekeepers themselves, be in the service of keeping the public from adopting beliefs they regard as socially or practically dangerous.[1] Second, it also focuses on the descriptive aspects of gatekeeping in real-world scientific contexts. By analyzing two specific episodes from

[1] Of course, at the end of the day, we would hope that believing true things brings about good outcomes, and avoiding believing false things prevents bringing about bad outcomes. But we can still see these are separate goals, and sometimes we can see scientist-bureaucrats explicitly pulling them apart.

the Covid-19 pandemic, this paper aims to shed light on how gatekeeping functions in practice, potentially revealing discrepancies between the ideal and the reality of scientific gatekeeping. The analysis focuses on scientist-bureaucrats—scientists who, like Fauci, Collins, or Ferguson, operated at the intersection of epistemic authority and bureaucratic power.

This approach does not necessarily contradict Dormandy and Grimley's work but rather complements it by exploring the complexities and challenges of gatekeeping in science as it is actually conducted. By examining these real-world cases, we can gain insights into how gatekeeping practices may deviate from the ideal and consider the implications of these deviations for the broader scientific enterprise. By watching the ways in which scientists gatekept during the pandemic, we can learn a lot about some of the challenges philosophy of science might need to confront as it grows increasingly interested in policy-relevant science.

This paper should be read as an exploratory study rather than as a fully developed theoretical program. Its purpose is to sketch what a public choice philosophy of science might look like through the lens of two pandemic-era episodes, rather than to provide a complete framework or exhaustive defense. The focus is diagnostic: to show that the tools of public choice theory—attention to incentives, institutional self-interest, and bureaucratic dynamics—can illuminate familiar questions in philosophy of science when scientists act as policy advisers. A fuller treatment would need to engage related literatures, including Kitcher-inspired accounts of science and democracy, work on science communication and inductive risk (Douglas, 2009), and perspectives on "fast science" and crisis reasoning (Stegenga 2025). My more limited goal here is to motivate that larger research trajectory by demonstrating its plausibility and potential significance.

So first, what is gatekeeping? Brooks (2025) characterizes it nicely: "Gatekeeping is an action done by agents (the gatekeepers) in order to safeguard or maintain the perceived good-making features of the thing being gatekept. Importantly, good-making features can be domain-specific or more general, and gatekeeping tends to cover multiple rather than single good-making features. To a good first approximation, a gatekeeper is an individual or group with the authority—vested or otherwise—to regulate admittance or endorsements of a domain." The things being gatekept in our episodes are the epistemic products of scientific reasoning about two aspects of the Covid-19 pandemic. And as we have discussed, the good-making features we will be interested in fall into two categories: epistemic features and features related to the public good. And the gatekeepers themselves are of course scientists, but as we will see as we go, these are not scientists as philosophers of science normally conceptualize scientists. They are, mostly, what I will call scientist bureaucrats. Here. I am focusing on the extreme end of the spectrum of a somewhat blurry dichotomy drawn by Lefevre and Schliesser, following De Langhe, between "the individual roles of a scientist (such as doing research, writing articles, giving lectures), and the aggregate roles such as editing a journal, organizing conferences, teaching, refereeing, policy-advisor, regulator, media spokesperson, etc.)" (Lefevere & Schliesser, 2014, De, p.291; Langhe, 2009). So by scientific gatekeeping I mean the exercise of control over the circulation of scientific claims, evidence, or participants within the epistemic community—decisions about what gets published, funded, promoted, or publicly endorsed. Gatekeeping is

thus not limited to editorial or peer-review contexts; it also occurs when scientists or institutions determine which findings are amplified or marginalized in public communication and policy advice. What unites these activities is the capacity to open or close access to epistemic legitimacy (i.e. the received view that is capable and worthy of guiding action). In the pandemic episodes discussed below, gatekeeping took the form of coordinated efforts to restrict or promote certain scientific messages in the name of public health or institutional trust.

There are two particular episodes of scientific gatekeeping from the pandemic that are better understood as having been portrayed—by the gatekeepers themselves—as attempts to maximize the public good in these ways, rather than attempts to advance science itself. My argument for this claim will draw heavily on first-person quotes from gatekeepers themselves: I will aim to persuade you that, if asked to characterize their own ultimate values, gatekeepers would be more likely to cite the public good than the advancement of scientific knowledge. But this is not where the interesting action lies. I will not only be interested in the self-avowed goals of the central players in these episodes, but also in what we might hypothesize about their other drivers, which can be analyzed in terms of institutional incentives rather than inner motives.

The two episodes I will highlight have to do with the origins of the SARS-CoV-2 virus ("*Origins*"), and the efficacy and cost-benefit analysis of so-called "non-pharmaceutical interventions" ("*NPIs*"). When I argue that these episodes were episodes of gatekeeping gone wrong, I will be arguing that they failed to advance the public good, not that they failed to facilitate the epistemic goals of science. In the *Origins* case, many of the gatekeepers, in their own words, claimed to be working in "solidarity with all scientists and health professionals" trying to prevent people with heterodox views from "jeopardis[ing] our global collaboration in the fight against this virus" (Calisher et al., 2020). In the NPI case, the gatekeepers claimed to want to prevent criticisms of official public policy from "getting a lot of attention". I will argue that the best interpretation of the "a lot of attention" phrase, was not *too much attention from other scientists for the goals of science to be best pursued* but rather *too much attention from the public for the public good* or *too much attention in order for us to achieve the goal of protecting the health care system*.

Once we understand scientific gatekeeping as sometimes directed not at the epistemic goals of science but at the goals of *public good* more generally, we need, I will argue, a correspondingly richer conception of the *incentive structure* of science. Crucially, we need one richer than the one that philosophers usually offer when they assume scientific activity is directed solely at the 'goals of science'—that is, the traditional conception of the incentive structure of science called 'the credit economy of science.'[2] I will argue that scientists do not exclusively engage in scientific inquiry for the sake of knowledge. They also sometimes engage in inquiry for the purpose of generating policy-making advice and informing, even sometimes setting, public policy. And the 'scientist-bureaucrats' who seek to inform policy-makers do not just seek credit. In fact, in much of what scientists-bureaucrats do, credit is completely

[2] See, for example Lee (2022) and references therein, going back at least as far as Merton (1968). My interest in comparing episodes from the pandemic to the credit economy is partly inspired by a short piece by Peter Godfrey-Smith (2022).

beside the point, since they are not even prima facie engaged in making novel scientific discoveries.

So, we need a conception of the incentive structure in science that is analogous to the credit economy conception, but that works for a class of scientists that is different from the traditional class. What I mean by 'analogous' is this: in the credit economy story, scientists' avowed ultimate goal is knowledge, but their incentive is credit; in the new conception that we need, scientist-bureaucrats avowed ultimate goal is to provide the 'best' policy advice for advancing the public good, but their incentive is *X*. To best spell out what falls under *X*, I will argue, we should pursue a program that, following Eric Schliesser (2022) and Gordon Tullock, I want to call 'public choice philosophy of science.' Although this phrase has certainly appeared in a number of places before, I don't think the program has ever been fully articulated other than in the context of economics (Tullock, 2005).

Public choice theory (of the ordinary variety) is designed to tell us (among other things) what sorts of personal incentives individuals pursue when they purport to be engaged in the collective goal of advancing the public good (which is, after all, supposed to be the goal of good government – the ordinary target of public choice theory.) And so, the goal of public choice philosophy of science, as I want to lay it out, is to outline what falls under X. What incentivizes scientist-bureaucrats?[3] A good public choice philosophy of science will not only explore goals beyond the narrowly epistemic, but will also explore incentives that reach far beyond credit.

In what follows, I will use the two episodes of gatekeeping I have called "Origins" and "NPIs" to argue that scientist-bureaucrats' incentives are best interpreted as ones closely analogous to those highlighted by ordinary public choice theory. Public choice theory often emphasizes that bureaucrats pursue such things as job security, budgets, prestige, power, or minimizing workload. In a similar spirit, the three incentives that I will emphasize in this paper are: *1) Public perception of scientist-bureaucrats' importance and virtue; 2) Scientist-bureaucrats' political influence; 3) Protection of the resources scientist-bureaucrats have worked hard to capture.*

Throughout this paper, I use the term scientist-bureaucrat to denote scientists who occupy positions where epistemic authority and administrative responsibility over-

[3] One could write an entire paper on the substance of this footnote, but in the limited space I have here, I want to highlight a difference between what I'm calling public choice philosophy of science and the kind of 'Sociology of Scientific Knowledge' (SSK) that thrived mostly in the 1990s. Because SSK, after all, was very interested in incentives. But SSK split into two camps, neither of which achieved what I hope PCPS will achieve. 'Interest modeling' tried to show how the content of well-credentialed scientific achievements was motivated by incentives. In this sense, interest modeling in SSK was profoundly relativist. So, for example, Donald MacKenzie (1981) argued that the development and acceptance of statistical methods was incentivized by the proponents of these theories wanting to advance the privileges of the rising professional middle class. Actor-Network Theory, like that of Bruno Latour (1987), on the other hand, focuses essentially on empire building, and in so doing mostly ignores the content of what the subjects it studies produce, emphasizing that it is all inscriptions. Indeed, Latour (1983) notes that "the way in which anything and everything was transformed into inscriptions was not my bias, as I first thought, but was what the laboratory was made for. What I think both overlooked is the interesting subject of how interests shape the transient acceptance of politically important claims in a particular policy context, like that of the pandemic. This allows a kind of interest-based account of science to avoid the relativism of someone like MacKenzie while also not doing what Latour did, which was to move away from really trying to understand how interests shape the content of scientific claims.

lap. The category includes not only career officials within agencies such as the CDC, FDA, or NIH, but also academic researchers whose funding, advisory roles, or institutional mandates tie them directly to policy processes. What distinguishes scientist-bureaucrats from scientists simpliciter is not their employment status but the dual nature of their incentives: they are simultaneously answerable to scientific norms of evidence and to bureaucratic imperatives of coordination, communication, and control. In this respect, the term partly overlaps with what Gil Eyal (2019) and David Demortain (2023) describe as regulatory science: scientific work that inhabits the middle lane between pure inquiry and policy execution. These are the actors whose choices I treat as shaped by public-choice dynamics—where incentives for political importance, institutional stability, and public trust can rival or displace strictly epistemic aims.

The remainder of the paper is structured as follows. In Sect. 2, I will introduce the two episodes of gatekeeping I want to analyze and give the reader just enough information to help me explain, in Sect. 3, what I mean by 'public choice philosophy of science', i.e., a program for understanding the incentive structure for scientist-bureaucrats. Sections 4, 5, and 6, respectively, will each give an account of one of three kinds of incentives that I propose to put in the toolkit of public choice philosophy of science. To put meat on the bones of these three sections, they will return to the gatekeeping episodes and provide new details of which the reader, at that point, will be equipped to see the relevance. Section 7 will offer some concluding remarks.

2 Two episodes of gatekeeping

Let me now introduce the two episodes of gatekeeping from the pandemic that motivate this paper. The first was the effort to keep out of the public domain criticisms of the dominant public health paradigm during the pandemic in most developed countries—the one that focused on so-called non-pharmaceutical interventions (NPIs). This gatekeeping effort was especially strongly directed at the so-called 'Great Barrington Declaration' (Kulldorff et al., 2020), which expressed "grave concerns about the damaging physical and mental health impacts of the prevailing COVID-19 policies" and which earned its signatories, infamously, the label of "fringe epidemiologists" in need of a "devastating published takedown" by the scientific establishment (Lepore, 2021). The second was the effort to keep out of the scientific literature, out of public debates, and out, broadly speaking, of the range of views that we were permitted to discuss in polite company, the hypothesis that SARS-CoV-2 originated in a lab as the result of scientific study of a kind known as 'gain of function research'.

2.1 NPIS

Starting in the spring of 2020 many countries adopted stringent lockdown measures, including school closures, business closures, stay-at-home orders, and restrictions on social gatherings. These measures were intended to slow the spread of the virus but had significant economic repercussions, leading to widespread job losses, business failures, and economic downturns. There were increasing reports of mental health

issues, including depression, anxiety, and increased domestic violence, attributed to the isolation and stress caused by lockdowns. Schools were closed or moved to online learning, which was prima facie detrimental to children's education, social development, and mental health. And non-Covid-19 healthcare services were deferred or canceled, leading to delays in treatments for other serious conditions like cancer, heart disease, and mental health issues. There was a noted polarization in public discourse regarding the handling of the pandemic, with media and political figures often discouraging the idea that there was any uncertainty about the wisdom of all these measures. Following all these policies, we were often told, was simply a matter of "following the science" (MacAulay et al., 2023).

The Great Barrington Declaration (GBD) (2020) aimed to shift the focus from broad, sweeping restrictions to a more targeted approach that would protect the vulnerable while minimizing societal disruption. The Declaration was authored by three epidemiologists, biostatisticians, and health economists (in fact, each of the three authors have disciplinary backgrounds in all these areas) and stated that:

> Current lockdown policies are producing devastating effects on short and long-term public health. The results (to name a few) include lower childhood vaccination rates, worsening cardiovascular disease outcomes, fewer cancer screenings and deteriorating mental health – leading to greater excess mortality in years to come, with the working class and younger members of society carrying the heaviest burden. Keeping students out of school is a grave injustice. Keeping these measures in place until a vaccine is available will cause irreparable damage, with the underprivileged disproportionately harmed.

However, this proposal was met with significant controversy and criticism from other public health experts, who argued that the strategy was impractical, ethically problematic, and could lead to higher mortality rates. Not only was it met with significant criticism, but strenuous gatekeeping attempts were also put into place. We now know that Francis Collins, director of the NIH, emailed the following words to Anthony Fauci, director of the NIAID and the face of Covid mitigation policy in the United States:

> This proposal from the three fringe epidemiologists who met with the Secretary seems to be getting a lot of attention – and even a co-signature from Nobel Prize winner Mike Leavitt at Stanford. There needs to be a quick and devastating published take down of its premises. I don't see anything like that online yet – is it underway?; (Bhattacharya, 2023, quoting Collins in email to Fauci)

And the take-down was, indeed, underway. Shortly after the release of the Great Barrington Declaration, some users reported that searching for it by name on Google did not yield the Declaration's official website at the top of the search results. Instead, what were often among the first results were articles critical of the Declaration, including pieces that smeared its authors. This was seen by some as Google's algorithmic manipulation or bias against the Declaration's message (Myers, 2020). On Reddit, posts linking to the Great Barrington Declaration were removed from popular

subreddits like r/COVID-19 and r/Coronavirus (ibid). These subreddits, with large memberships, classified the Declaration as 'spam' or deemed it not suitable under their content policies, which were interpreted by some as a form of censorship. The 'Twitter Files,' a series of internal documents from Twitter's corporate offices compiled by journalist David Zweig, revealed that Twitter had engaged in practices like shadow banning, reducing visibility, and blacklisting accounts associated with the Great Barrington Declaration, including co-author Jay Bhattacharya (Zweig, 2023). The Declaration was sometimes labeled as misinformation under corresponding policies by social media platforms or search engines, which could lead to reduced visibility or outright removal from platforms, as part of a broader effort to combat 'misinformation' during the health crisis (Economic Times, 2023, October 7).

Ironically enough, by the time the pandemic was over, Collins seemed to agree with many of the claims of the GBD:

> If you're a public health person and you're trying to make a decision, you have this very narrow view of what the right decision is. And that is something that will save a life; it doesn't matter what else happens. So you attach infinite value to stopping the disease and saving a life. You attach zero value to whether this actually totally disrupts people's lives, ruins the economy, and has many kids kept out of school in a way that they never quite recover from. So, yeah, collateral damage. This is a public health mindset and I think a lot of us involved in trying to make those recommendations had that mindset and that was really unfortunate. It's another mistake we made. (Schorr, 2023)

From this, we can observe two key points. First, the gatekeeping carried out by Collins and Fauci was not primarily aimed at facilitating the advancement of science. It was not about maintaining a firewall around legitimate internal scientific debate to optimize scientific progress. Instead, it appeared to focus on shielding the public from views they deemed "fringe." Fauci and Collins likely saw their actions—and here I'll attempt to portray their motives as they might have—as necessary to prevent behaviors they believed were not in the public's best interest.

Second, given Collins' 2023 admission, it is challenging to fully reconcile his behavior with a genuine pursuit of public good. Collins seemingly understood that the Great Barrington Declaration, which he wanted "devastatingly taken down," addressed issues with legitimate room for disagreement. This raises an important point: when one truly prioritizes public good, especially in scientific matters, and one sees room for legitimate disagreement, one can/should be expected to welcome rather than suppress disagreement.

The value of disagreement in pursuing public good, particularly in contexts of uncertainty, cannot be overstated. And it would be surprising if Collins were oblivious to this. Open debate and the clash of ideas allows for the identification and rectification of mistakes in reasoning or methodology. It helps challenge established ideas and often leads to novel solutions and breakthroughs. Perhaps most importantly, transparency about scientific disagreement fosters trust in the scientific process—something that came to be lacking in the pandemic as a direct result of Collins' gatekeeping.

By attempting to suppress the Great Barrington Declaration, Collins and Fauci had to know they would be hindering these benefits. Their actions thus seem to reveal that they were pursuing other incentives. It is hard to fully understand his behavior as directly incentivized by the pursuit of public good.

The value of open disagreement, particularly in conditions of uncertainty, is widely recognized in both democratic and scientific theory—even if the empirical relationship between transparency and public trust remains contested. Research in science communication shows mixed results: in politically polarized or crisis contexts, candid acknowledgment of uncertainty can sometimes erode trust in the short term, even while enhancing it among more reflective or epistemically sophisticated audiences (Jensen, 2008; van der Bles et al., 2020). From a public-choice perspective, hoever, the key point is not whether transparency *always* fosters trust, but that the relevant gatekeepers believed it did not—and therefore acted on an incentive to manage appearances rather than risk discord. Their suppression of debate thus fits the logic of bureaucratic self-protection: when institutional actors treat public trust as something maintained through unanimity, dissent becomes a liability rather than a resource.

It is also worth acknowledging that decisions made in the earliest months of 2020 occurred under conditions that, by any reasonable definition, constituted a public emergency. In such moments, decision-makers sometimes face collective-action problems where rapid, uniform behavior can be more valuable than further deliberation. Economists and political scientists describe these as "focal-point" situations, in which coordinated messaging serves to synchronize behavior when timing is critical and uncertainty high. From that perspective, temporary restriction of debate in March or April 2020 might be defended as a pragmatic—if ethically fraught—effort to preserve the effectiveness of collective action under duress.

That rationale, however, cannot easily explain the gatekeeping surrounding the Great Barrington Declaration. The Declaration appeared in October 2020, months after the initial crisis of uncertainty and hospital overload had passed. By then, multiple societies had demonstrated divergent yet relatively stable strategies, and the epidemiological situation allowed for reasoned reassessment. Most notably, Sweden's experience—having largely followed the targeted-protection approach advocated by the GBD—had not produced catastrophic outcomes. Yet Flaxman et al., (2020), which became the canonical modeling justification for continued lockdowns, deliberately treated Sweden's results as a validation of its single minor intervention, rather than as evidence that less coercive approaches could succeed. This rhetorical move papered over a clear empirical anomaly and helped sustain the impression that dissenting views were irresponsible rather than debatable. Whatever emergency logic might have existed in March, by October it had become a justification of convenience.

Flaxman et al., (2020) provides a telling example. This widely-cited modeling study – published in Nature and often invoked to justify lockdowns – estimated that strict interventions had saved over 3 million lives across Europe's first wave. However, to reconcile Sweden's relatively mild outcomes without a full lockdown, the Flaxman model made a dubious adjustment: it assigned an outsize effect to Sweden's one minor policy (a ban on large public events) as if that alone tamed the virus. In fact, the model estimated banning public events cut transmission by an implausible

~72% in Sweden (versus only ~1.6% in other countries). By attributing Sweden's success to this "magic" intervention − 45 times more potent in Sweden than elsewhere – the authors effectively erased Sweden as an anomaly. This rhetorical sleight-of-hand papered over conflicting evidence and helped reinforce the narrative that only hard lockdowns were responsible, casting more moderate pandemic strategies as irresponsible rather than legitimate alternatives (Lemoine, 2022).

The contrast with the *Origins* episode is thus even sharper. In that case, the rationale for closure was not coordination under duress but reputational and institutional self-protection. Recognizing this asymmetry strengthens the public-choice diagnosis: appeals to emergency coordination may begin as legitimate crisis management but can persist as bureaucratic habit, suppressing disagreement long after the emergency rationale expires.

2.2 Origins

On to the second episode of gatekeeping. In January and February of 2020, speculation emerged that the virus behind the Covid-19 pandemic, SARS-CoV-2, might have come from a lab accident at the Wuhan Institute of Virology (WIV), perhaps as a result of virologists there having experimented with finding existing SARS-type corona viruses and modifying them to make them more infectious in human beings. This hypothesis, let's call it "lab leak", was vigorously gatekept out of 'permissible' public discussion. At the same time, the very people who worked hardest to do this said privately that the hypothesis was "so friggin' likely," (Kristian Andersen), that "I literally swivel day by day thinking it is a lab escape or natural," (Andrew Rambault) and that "we are NEVER going to know what happened in that lab" (Edward Holmes).[4] Jeremy Farrar (2022: 2) recalled, regarding discussions of the virus's origins, "I would do things I had never done before: acquire a burner phone; hold clandestine meetings; keep difficult secrets." To be clear, I still do not think we know with any great confidence whether SARS-CoV-2 came from a lab or from nature. But I do think we know that the relevant experts, in early 2020, were privately expressing a position of, at best, profound uncertainty.

On February 19, 2020, *The Lancet* published a letter (Calisher et al., 2020: 42), signed by 27 scientists, which "strongly condemned" as a "conspiracy theory" the idea that COVID-19 might have emerged from a laboratory. To the signatories, that mere idea stood to "jeopardise our global collaboration in the fight against this virus". The letter not only emphasized that scientists overwhelmingly agreed that COVID-19 originated in wildlife, it also expressed "solidarity with Chinese scientists and health professionals" and urged "unity" in the fight against the virus—proclamations that seem to be about a lot more than setting the record straight on a factual matter for its own sake. This was a deeply moral case that transcended any purely epistemic goals and did not rely primarily on epistemic arguments. The epistemic case followed right after: in March 2020, "The Proximal Origins of SARS-CoV-2", now a widely cited paper, was published in *Nature Medicine* by Kristian Andersen, Robert

[4] These statements from Andersen, Rambault, and Holmes appear in private Slack messages, which are compiled here: <https://usrtk.org/covid-19-origins/visual-timeline-proximal-origin/>[accessed 24/02/25]

Garry, Edward Holmes, Andrew Rambaut, and W. Ian Lipkin (some of whose private comments I quoted above). The paper investigated the origins of SARS-CoV-2 and concluded that the virus most likely emerged through natural zoonotic spillover, rejecting the idea that it was deliberately engineered. They stated, "Our analyses clearly show that SARS-CoV-2 is not a laboratory construct or a purposefully manipulated virus" (Andersen et al., 2020: 450). Based on these two papers, both the media and official agencies (governmental and intergovernmental) worked hard to suppress discussion of the possibility that the virus emerged from a lab. On my interpretation, if you asked any of the participants in this suppression campaign why they did it, they would say it was for the public good. As Andersen, the lead author of "Proximal Origins", himself put it: "I hate when politics is injected into science, but it's impossible not to, especially given the circumstance."[5] Of course, Andersen might have meant that it's impossible not to inject politics *because that is the only way to preserve knowledge.* But the line comes right after Rambault had said "Given the shit show that would happen if anyone serious accused the Chinese of even accidental release, my feeling is we should say that given there is no evidence of a specifically engineered virus, we cannot possibly distinguish between natural evolution and escape so we are content with ascribing it to natural processes."

These same virologists worked very hard to keep discussion of a possible artificial origin of the virus out of the press. According to Donald McNeil Jr, who was the main science writer covering the Covid-19 pandemic at the New York Times, they successfully kept coverage of the issue out of that paper. As he suggests in the epigraph of this paper, Andersen and Rambault misled him both with the paper and in personal interviews with him. He then, according to his own reports, kept the NYT from reporting on the findings of the intelligence communities that the virus might have come from the WIV. Speaking about just a piece, he says:

> When I was allowed to see their draft, hours before it was to be published, I exploded. I wrote a note accusing them of downplaying my reporting because it had "too many big words." I was told off for my tone, but their article was held. I can only surmise that their sources then went to Fox News, which soon ran something similar.B
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> Had I known that the "Proximal Origins" authors initially suspected a lab leak, would my approach have been different? Of course. Did my ignorance—and my insistent advocacy—tip the balance of Times coverage away from the lab-leak theory? I don't know because I don't attend Page One meetings, but it probably contributed to what happened next: the Times essentially dropped the topic for a year. (McNeil Jr 2024: 208)B

But of course, it was not just *The Times*. The Lancet letter, "Proximal Origins," and the interviews given by these virologists had an enormous impact on, and played a central evidentiary role in, the reporting of major media outlets and on the proclamations of agency officials. Regarding agency officials, for example,

[5] Private Slack message, see Footnote 4.

- "A new study debunks such (lab origin) claims by providing scientific evidence that this novel coronavirus arose naturally" – Francis Collins (2020)B
- "SARS-CoV-2 is introduced through a laboratory incident, reflecting an accidental infection of staff from laboratory activities involving the relevant viruses. We did not consider the hypothesis of deliberate release or deliberate bioengineering of SARS-CoV-2 for release, the latter has been ruled out by other scientists following analyses of the genome" – Joint WHO-China Study Team (2021: 118)
- "On 17 March, five prominent scholars from the US, the UK and Australia pointed out on Nature Medicine that the evidence shows that SARS-CoV-2 is not a laboratory construct or a purposefully manipulated virus" – Ministry of Foreign Affairs, PR, (2024, June 6)
- "The rumours of a leak from the laboratory were refuted categorically by the laboratory director for the following reasons: "(2) … a paper by leading virologists in Nature rebutted the idea of a bioengineered source." – Joint WHO-China Study annexes virus (2021, March 30).
- "Let's put conspiracy theories about the origin of #SARSCoV2 to rest and help to stop spread of misinformation – great work by Kristian Andersen" – Chief Editor to Nature Medicine, João Monteiro (Monteiro, 2020)

In major media outlets, we find the following sorts of reporting, all relying on these two publications:

- Fox News: "The coronavirus did not escape from a lab: Here's how we know." (Live Science. (2020, March 24)
- Forbes: "No, COVID-19 Coronavirus Was Not Bioengineered. Here's The Research That Debunks That Idea." (March 17th, 2020)
- ABC News: "Sorry, conspiracy theorists. Study concludes COVID-19 'is not a laboratory construct'. Some have suggested the virus is actually an engineered biological attack" – Kate Holland (March 27th, 2020)
- ScienceNews: "No, the coronavirus wasn't made in a lab. A genetic analysis shows it's from nature. Scientists took conspiracy theories about SARS-CoV-2's origins seriously, and debunked them." (March 26th, 2020).
- Vice: "Once and for All, the New Coronavirus Was Not Made in a Lab. A new paper explains exactly what makes it different than other coronaviruses and how we know that it wasn't created in a lab." – Love, S. (March 20th, 2020).
- MedicalNewsToday: "The new coronavirus was not man-made, study shows" –(March 20th, 2020).

3 What is public choice philosophy of science?

Public choice theories of government often highlight the ways that public servants are motivated by factors such as job security, budgets, prestige, power, or minimizing workload (Stigler, 1971). For example, a bureaucrat might advocate for policies that increase their department's funding or expand its responsibilities, even if these policies are not the most efficient solutions for society. Public servants often work

within hierarchical systems where advancement and influence are tied to specific metrics, such as the size of their agency or the scope of their projects. This can lead to behavior like 'empire-building,' where departments seek to expand their influence regardless of actual public demand.

Advocates of public choice theories of democratic government will point to the following sorts of considerations: a regulatory agency might impose rules that create greater demand for its oversight functions. Public servants might resist reforms or innovations that threaten their traditional roles or power structures. Policy decisions made by public servants may be swayed by considerations such as future job prospects that a public servant will enjoy in the private sector as a result of the decisions they make (the 'revolving door' phenomenon) (cf. Blanes i Vidal et al., 2012). In short, public servants are incentivized by more than the prospect of votes.

Public choice theory, when applied to the philosophy of science, would do something similar. Just as public choice theories of government highlight how public servants seek more than votes, public choice philosophy of science will highlight the ways in which scientists pursue many things other than credit. Here, I want to focus on three aspects of self-interest that public choice philosophy of science might successfully highlight, and which might, inter alia, explain some features of the two episodes of gatekeeping we have discussed above.

It is worth emphasizing that the analysis offered here departs from the standard Tullock- or Stigler-style model of *homo economicus* as a purely utility-maximizing agent. The "self-interest" at stake in the behavior of scientist-bureaucrats, the ones that institutional incentives appeal to, is often heterogeneous and socially embedded: it includes moral self-conception, professional identity, and group allegiance as well as career or material rewards. In that respect, the framework I develop is compatible with public-choice insights about incentives and institutional structure, but it relaxes the assumption that all such motives can be reduced to expected-utility maximization. I am also relaxing the goals of public choice. The goal is not to build Tullock-style models of rent seeking and their impact on consumer surplus. Rather than assuming simple MaxU behavior, the aim is to trace how a variety of commitments and pressures—status within epistemic communities, perceived virtue, institutional solidarity—shape scientists' choices when they act under bureaucratic or crisis conditions. While this differs from the way the concept of public choice is sometimes framed, I think it is consistent with the way the concept is employed more broadly in the analysis of bureaucratic institutional behavior.

To start, I want to highlight one overarching feature of a kind of self-interest that differs substantially from what is ordinarily discussed under the rubric of "the credit economy of science" (Hull, 1988, 1997; Strevens, 2003). To be clear, the framework of the credit economy of science is already, to some extent, a model based on self-interest. Scientists, on this account, want credit out of self-interest. They pursue credit directly, rather than knowledge directly. The canonical case of this is James Watson's (1976) admission that he pursued the hypothesis that DNA, rather than protein in the cell nucleus, was the molecular basis of genetic inheritance, not because he thought his hypothesis was more likely to be true than its rival. Quite the opposite. He pursued the DNA hypothesis because he thought that he would be much more likely

to get there first, and hence acquire credit, if it turned out to be the case that the DNA hypothesis was true and he pursued it.

But we need a different picture when scientist-bureaucrats, unlike Watson, are pursuing the public good rather than knowledge. In the credit economy picture of ordinary scientists, the naive view of scientists being motivated by pure curiosity is replaced by a picture where scientists pursue credit. Is a similar 'looking behind the curtain' required for the view that scientist-bureaucrats pursue the public good? If so, what incentives would stand in for credit? In a crisis, scientist-bureaucrats don't care as much whether the ideas that they put forward will, say, end up in text-books or encyclopedias as, for example, James Watson did. They care whether their ideas are followed for the duration of the crisis. We can see this fairly clearly by comparing what Francis Collins says in his private email to Fauci with what he said to a public audience after the crisis was mostly over. The question is: what explains our cases of gatekeeping? When we look behind the curtain of ordinary scientists, arguably, we find credit as a primary incentive. What do we find when we look behind the curtain of scientist-bureaucrats?

Looking at the behavior of experts and scientist-bureaucrats during the pandemic, there are three other kinds of incentives that I believe do a better job of explaining their behavior: *1) Attention and a public perception of scientist-bureaucrats' importance; 2) Political influence; 3) Protection of the resources they have worked hard to capture.*

4 Gatekeeping the science of NPIs

We should admit right away that there is a major challenge in analyzing scientists' incentives and that is that we do not have omniscient access to what motivates them. We can be reasonably confident that James Watson pursued the DNA hypothesis despite thinking it was less likely to be true, in the hope of maximizing his likelihood of getting credit, because he tells us this in *The Double Helix.* How can we find similar evidence that scientist-bureaucrats were responding to incentives rather than being motivated purely by promoting the public good? Again, we have to avoid pretending that we know how to read minds and instead look at the details of people's behavior and merely suggest incentive structures that give a plausible background explanation of that behavior, at best noting that the explanation is more plausible than the explanation based on the pursuit of public good. We should also be very clear, here, that the behavior I want to analyze is not the advocacy of the use of models and the NPIs that those models purport to recommend. The behavior I want to analyze is the gatekeeping. The question is not just 'why did Ferguson, Fauci and Collins advocate NPIs based on modeling?'. The question is why Ferguson, Fauci and Collins wanted to create the impression that questioning their judgment on these matters was "fringe," "in need of a devastating takedown," worthy of censorship and deplatforming, and generally outside the bounds of reasonable disagreement. And why, moreover, they did this when barely more than a decade earlier, this was a huge, open public debate with many important and influential people and groups (including

the ACLU[6]) vigorously advocating against most NPIs and against over-reliance on models (even in a context of a pandemic that, at the time, was assumed to be capable of killing 2 million Americans.)

To begin to fully understand this, it helps to go back to that (roughly) decade-old history. That is because the very same models, the very same methods, and the very same recommendations got a very different reception in the middle 00s than they did in 2020. So, it is extremely instructive to look at the role that Neil Ferguson and his fellow travellers played in developing some of the pandemic preparedness documents (which were primarily focused on influenza epidemics) that emerged in the USA in the period between 2005 and 2008, and the reception that they received in the debates that occurred in that period. In the earlier period, Ferguson and his allies were mostly rebuffed. So, then, it's even more instructive to compare that reception and role to the reception that Neil Ferguson and his team at the Scientific Advisory Group for Emergencies (SAGE), along with their model, sometimes referred to as "Covidsim" and sometimes referred to as the "Imperial College London" (ICL) model received in 2020, and to look at the role that it played in the Covid-19 pandemic.

The ICL model was at the foundation of 'Report 9' (Ferguson et al., 2020) that was released by SAGE on March 16th, 2020. The report, which greatly influenced pandemic policy in the UK, the United States, and many other countries around the world,[7] was used to justify most of the NPIs that were enacted in these two countries and elsewhere. If we compare these two periods, what we see is that the period between 2005 and 2008 is one in which there was a very contentious battle between a group who favored both NPIs and the use of modeling to manage pandemics, and a group who opposed them both, with the opposing group emerging from the 2005–2008 period having mostly prevailed. But by the spring of 2020, as the Covid-19 pandemic raged, the tables had turned. I hope to show that the details of these episodes, combined with some telling remarks made by some of the participants, support an interpretation according to which some of the gatekeeping of the pandemic period can be seen as an effort by those who favored the use of models and NPIs to secure their political influence.

5 Pandemic preparedness–the early history[8]

To fully understand the story, we need to go back to pre-Covid efforts in pandemic preparedness, particularly the American 2005 National Strategy for Pandemic Influenza (NSPI). The work of Carter (Hatchett et al., 2007) was pivotal in formulating this strategy, emphasizing the importance of NPIs as a key tool in managing pan-

[6] "American history contains vivid reminders that grafting the values of law enforcement and national security onto public health is both ineffective and dangerous. Too often, fears aroused by disease and epidemics have justified abuses of state power". https://www.aclu.org/sites/default/files/pdfs/privacy/pemic_report.pdf

[7] Report 9 made projections of deaths and ICU bed demand for the UK and US under various policy options, and a later report, Report 12, did so for many other countries.

[8] I am highly indebted Macedo and Lee (2025) for drawing my attention to much of the history of pandemic preparedness in the first decade of the century.

demics. Their research, which was widely discussed in the context of the NSPI, was grounded in historical data from the 1918 influenza pandemic, and their advocacy for NPIs was further reinforced by a series of influential papers published in the mid-2000s based on their models.

Perhaps more importantly for our story, Mecher and Hatchett's views were strongly supported by a collaborative effort among three groups of scholars, who analyzed the effectiveness of NPIs during the 1918 pandemic *using models*. One key paper, authored by Howard Markel, Martin Cetron, and colleagues (2007: 644), found "a strong association between early, sustained, and layered application of non-pharmaceutical interventions and mitigating the consequences of the 1918–19 influenza pandemic in the United States."

Mecher and Hatchett's work had a direct influence on two key pandemic planning documents: the *2006 National Strategy for Pandemic Influenza: Implementation Plan* (commonly referred to as the "Implementation Plan") (Homeland Security Council, 2006) and the *2007 Interim Pre-pandemic Planning Guidance: Community Strategy for Pandemic Influenza Mitigation* (known as the "Community Strategy") (Centers for Disease Control and Prevention, 2007).

The 2006 *Implementation Plan* declared that "models are powerful tools that can be used to inform policy decisions by highlighting the impact of various interventions on the spread of disease" (Homeland Security Council, 2006, cited in; Macedo & Lee, 2025). This marked a significant step in ensuring that local, state, and federal authorities could base their decisions on scientific models predicting the outcomes of various interventions. The plan emphasized expanding infectious disease modeling capabilities and sharing model findings with state and local authorities to inform decision-making regarding transportation, social distancing, and public health interventions. It recommended several NPIs, including social distancing, working from home, and "cancelling non-essential public gatherings" (in Macedo & Lee, 2025: 108).

Notably, the 2006 *Implementation Plan* was most emphatic about the importance of **school closures**. Citing *modeling* evidence, it concluded that "school closure is an effective means of reducing overall attack rates within communities" (in Macedo & Lee, 2025: 108), and that this intervention was most valuable when implemented early in a pandemic. The plan also advocated for social isolation measures within households, recommending that families limit their contacts to reduce community transmission rates.

The 2007 *Community Strategy*, published by the U.S. Department of Health and Human Services (HHS) and the Centers for Disease Control and Prevention (CDC), expanded upon the guidance of the Implementation Plan. Designed specifically for state and local governments, the Community Strategy was subtitled "Early, Targeted, Layered Use of Nonpharmaceutical Interventions", underscoring the importance of multiple simultaneous interventions to manage a pandemic effectively.

5.1 Pandemic preparedness–the role of models in the early history

An especially important part of this story, for our purposes, was the role played by respiratory disease modeling of public health interventions conducted by Martin

Bootsma and Neil Ferguson in 2007.[9] Their paper, "The Effect of Public Health Measures on the 1918 Influenza Pandemic in U.S. Cities," applied a model to analyze how NPIs influenced the course of the pandemic in various cities. That's because the case for NPIs was always and ever entwined with the case for the evidentiary power of these models. People who lined up behind the NPIs almost always did so on the basis of models, and those who were skeptical of the models almost always were skeptical of the NPIs.

We will see this most clearly when we get to the findings of the *National Institute of Medicine* (NIM) in what follows. But to preview: One of the NIM report's strongest recommendations was the need for transparency about the *limitations of model predictions* and the *potential costs—social, ethical, and economic—of implementing NPIs*. They warned that governments might feel pressured by models to deploy public health measures, even without solid evidence of their benefits, while overlooking potential secondary effects. So we can see quite clearly that, in 2005–2008, the battle lines were clear with favor and opposition to modeling and NPIs rising and falling together. These two issues were very much joined at the hip.

Returning to our story: Bootsma and Ferguson's (2007: 7591) model purported to provide critical insights into how NPIs, such as school closures, quarantines, and public gathering bans, altered epidemic dynamics. By fitting their model to weekly mortality data from 16 U.S. cities during the 1918 pandemic, they "demonstrated" that the timing and duration of NPIs significantly affected transmission rates. Bootsma and Ferguson's work further argued that NPIs needed to be timed carefully and applied with sufficient stringency to have the desired impact. Ferguson's later modeling of COVID-19 (in the simulations known as "covidsim" and reflected in Report 9 of SAGE) reflected these findings, using a similar modeling framework (reusing some of the same code) to predict that early, widespread interventions would be critical to controlling the pandemic and preventing healthcare systems from being overwhelmed.

Despite the growing emphasis on modeling and NPIs in pandemic preparedness, not all epidemiological experts were convinced that pandemics could be effectively managed or mitigated by these strategies. Perhaps as importantly, Pre-COVID, many professionals raised concerns about the limitations of *models* to assess the otherwise uncertain benefits of NPIs.

A notable voice of dissent came from Dr. David A. Henderson. Along with three colleagues from the Center for Biosecurity at the University of Pittsburgh Medical Center, Henderson co-authored a comprehensive response criticizing the widespread reliance on NPIs, particularly measures like stay-at-home orders. The team argued that such interventions failed to account for the large number of essential workers required to maintain basic services, such as food supply and public transportation,

[9] This reconstruction of events and attitudes surrounding pandemic preparedness and modeling draws primarily on publicly available sources, including official U.S. pandemic planning documents (Homeland Security Council, 2006; CDC 2007), retrospective interviews and reporting on Neil Ferguson and the Imperial College COVID-19 Response Team (e.g., Whipple, 2020; MacAulay et al., 2023), and secondary syntheses such as Macedo and Lee (2025). Where quotations appear, they are taken directly from the cited materials. The goal here is not to provide an exhaustive history but to reconstruct the received narrative that guided policymaking and scientific self-understanding.

which couldn't simply be halted. They also pointed out that schools serve as critical day care and meal providers for low-income families, who would be disproportionately affected by closures and stay-at-home mandates. Their overriding principle was clear: communities respond best to crises when daily life and social functions are least disrupted (Inglesby et al., 2006).

In 2006, further criticism came from a group convened by the (NIM), led by infectious disease expert Dr. Adel Mahmoud. This group of public health scholars, epidemiologists, and biostatisticians gathered to assess the strengths and limitations of *mathematical models* used to guide pandemic responses. The NIM group's findings were cautious. They acknowledged that models could be useful tools for organizing data and promoting dialogue among scientists and policymakers, *but they warned against over-reliance on models for decision-making*. In their view, *models simplify reality by offering idealized representations of complex systems like virus transmission in communities. While useful for generating insights, models should not be seen as substitutes for real-world decision-making*.

Both the Henderson and NIM groups raised fundamental concerns about the limitations of our knowledge when dealing with novel viruses, particularly when crucial knowledge needed to drive models is lacking, and particularly when the case for NPI was so routed in modeling. They cautioned that the models used to guide pandemic responses often rely on estimated or uncertain inputs, which can foster an illusion of control—suggesting that if we implement a specific intervention (X), we will achieve a desired outcome (Y). The critics emphasized that models should not be represented as providing the "right" answer, and they called for a more honest acknowledgment of the uncertainties inherent in pandemic planning.

Ultimately, the critics highlighted the need for pandemic response strategies that *acknowledge the limitations of models, avoid overstating the certainty of their predictions, and consider the full range of social, economic, and ethical consequences of NPIs*. While the scientific community debated the merits and limitations of pandemic modeling and NPIs, civil liberties groups also raised significant concerns about the ethical and social costs of these measures. As pandemic plans circulated during the George W. Bush administration, groups like the *American Civil Liberties Union* (ACLU) emphasized the potential dangers of embedding national security and law enforcement values into public health responses.

5.1.1 In 2008, an ACLU report included a stark warning:

"American history contains vivid reminders that grafting the values of law enforcement and national security onto public health is both ineffective and dangerous. Too often, fears aroused by disease and epidemics have justified abuses of state power. Highly discriminatory and forcible vaccination and quarantine measures adopted in response to outbreaks of the plague and smallpox over the past century have consistently accelerated rather than slowed the spread of disease, while fomenting public distrust and, in some cases, riots. The lessons from history should be kept in mind whenever we are told by government officials that 'tough,' liberty-limiting actions are needed to protect us from dangerous diseases (Annas et al., 2008: 5–6).

We end the 2005–2008 period, therefore, with an emerging consensus that NPIs, and the models that support them, are suspect, and might even pose a danger to the public good. But, other than the perhaps slightly heavy-handed rhetoric of the ACLU quotation, there was mostly healthy debate and dissent around all of these issues.

5.2 The role of models in the COVID-19 pandemic

So what changed? How did this situation that existed circa 2008, in which one group of experts advocated NPIs supported by modeling, while another criticized them and questioned the trustworthiness of models for uncovering these facts—and perhaps more importantly, one in which there was healthy debate—turn into one in which the models were 'universally' held in high regard, and criticism of NPIs was regarded as "fringe"? The answer to this question unfolds historically, in the winter of 2020, as the virus spreads from China, to Italy, and finally to the UK and US.

As Macedo and Lee (2025) observe, much of the transformation lay not in the science itself but in the institutional embedding of modeling as a governance tool. By 2020, the infrastructures created during the earlier preparedness era—simulation platforms, advisory committees, and model-based decision protocols—had become routinized instruments of crisis management. Their authority was therefore less an epistemic achievement than an administrative one, reflecting precisely the sort of bureaucratic entrenchment that a public-choice analysis is meant to diagnose.

The first widespread implementation of NPIs during the COVID-19 pandemic took place in China, where the outbreak began in late 2019. As the novel coronavirus, SARS-CoV-2, spread rapidly from its epicenter in Wuhan, Chinese authorities responded with an unprecedented array of NPIs to contain the virus and prevent it from overwhelming the healthcare system. These measures would later serve as a paradigm—though sometimes a controversial one—for many other countries as they faced their own outbreaks.

After witnessing China's rapid and aggressive response to the COVID-19 outbreak in early 2020, Italy became the first Western country to experience a severe outbreak and implement large-scale NPIs. Initially unprepared for the scale of the pandemic, Italy quickly shifted to adopt strict measures similar to those used in China, as the virus spread uncontrollably through its northern regions, particularly Lombardy and Veneto.

As the COVID-19 pandemic unfolded in early 2020, Neil Ferguson, the epidemiologist whose models shaped much of the global response, observed with surprise how NPIs implemented in China began to change the way the world viewed pandemic control. Initially, Ferguson and other members of the *Scientific Advisory Group for Emergencies* (SAGE) doubted that the harsh measures enacted in China—particularly the lockdown of entire cities and regions—would ever be viable in Western democracies. In an interview with Tom Whipple for *The Times* on December 25, 2020, Ferguson reflected on how quickly that perception changed in just a few months.

> I think people's sense of what is possible in terms of control changed quite dramatically between January and March," Ferguson remarked. In January

> 2020, China had deployed an "innovative intervention" by locking down entire communities and preventing people from leaving their homes, an unprecedented move in modern public health. At the time, Ferguson and his colleagues assumed that such an approach was unique to China's authoritarian governance system and could not be replicated in more liberal, democratic societies. As Ferguson explained, "It's a communist one-party state, we said. We couldn't get away with it in Europe, we thought.

China seemed to Ferguson to be controlling the virus with aggressive NPIs, leading to rapid reductions in transmission, but in Ferguson's view, this level of state control was unimaginable for countries like the UK, Italy, or Spain. However, everything changed for Ferguson when Italy, the first Western country to face a major outbreak, implemented its own national lockdown. The Italian government's decision to shut down entire regions and later the entire country, closely mirroring China's actions, was a turning point for Ferguson and his colleagues.[10]

5.3 Analyzing the history of pandemic modeling in terms of the incentive of political importance

How should a public choice philosophy of science analyze this episode? What we see in the 2005–2007 period is a fairly straight-forward turf war over who should have influence over pandemic preparedness and mitigation, and what methods ought to be considered reliable. Of considerable contention are two central questions: should NPIs, especially school closures, be used in a pandemic, and are models like Covidsim reliable tools for evaluating the effectiveness and likely outcomes of instituting various measures. Once again, in trying to understand the incentives that underpin science, we are at a major disadvantage in that we cannot read people's minds, but when Neil Ferguson said, in late 2020 "It's a communist one-party state, we said. We couldn't get away with it in Europe, we thought," he seems to be revealing (particularly with the phrase "we couldn't get away with it") information that we can use in conjunction with historical records to help interpret his motives. From the historical records reviewed above, we know that Ferguson had effectively had this fight in 2005–2007, a fight over which people and which forms of expertise (model-based or not) would have political power in planning for and mitigating a pandemic. And we know, that, as of, 2008, it seemed that Ferguson had lost this fight. Indeed, we can read Ferguson's *The Times* interview as an admission that he thought he had lost the fight, but when he saw what happened in Italy, he seems to be revealing that he and his allies had an opportunity to regain their influence, and perhaps even surpass the influence that they had before. If this is right, and it seems right to me, then the arguments between Ferguson and Boozma, on the one hand, and the Henderson and NIM groups on the other–and the directive to "devastatingly take down" the GBD by Collins and Fauci–were not only arguments about what was in the public good (as

[10] This entire episode reveals an interesting contrast between an epistemic and administrative achievement as sources of authority in science (I thank an anonymous referee for pointing this out.) This is of course a very Foucaultian point, about which another entire paper could probably be written.

Collins eventually admitted they were not). They were also arguments about which people, and which tools, would have political influence; and perhaps more importantly, which kinds of people (those adept in mathematical modeling or not) would have political influence.

We've seen a pretty full interpretation of what might have incentivized Ferguson, but this might still leave readers wondering why Fauci and Collins became so invested in supporting him. To understand this, we might appeal to a common theme of ordinary public choice theory, visible, for example, in the work of William Ascher (1983). According to Ascher, politicians rely on research communities to outsource responsibility for their decisions. Indeed, research communities need not even reach consensus, or eliminate uncertainty, in order to create a moral order. Ascher (1983: 417) argues that "once it is established that the international economic regime is not a straightforwardly determinable vector of nations' interests and power … uncertainty gives greater power to those (whether individuals or subunits) who 'absorb uncertainty.'" In such circumstances, politicians and bureaucrats, in other words, can protect themselves from future criticism by deferring to experts. Being able to point to experts and say 'we just did what they said' is the best way for policy makers to minimax. This leads, Ascher argues, to situations in which expert bodies, regardless of if they have in fact reduced uncertainty, get a life of their own, and an attendant set of powers. Even though Donald Trump is mostly famous for opposing Fauci and Deborah Birx's recommendations to follow the example laid by Ferguson, this was not entirely true early on. Early on, Trump acted just as Ascher suggests politicians will do. He said:

> I'll be listening to Deborah, who you just spoke to. I'll be listening to other experts. We have a lot of people that are very good at this. And, ultimately, it's a balancing act. … I'm a student. I've learned a lot from Deborah. I've learned a lot from Tony. … I've learned a lot (in Brady, 2020).

Thus, Fauci, Collins and Birx were able to exploit Ascher's mechanism to gain quite a bit of influence in the pandemic, and maintaining that influence, as Ascher suggests, requires scientists to be able to 'absorb' uncertainty. And you can only absorb uncertainty if you create the perception that uncertainty is not part of the scientific community, or exists only at its 'fringe.'

One might be tempted to treat "political influence" as a straightforward motive for scientist-bureaucrats, but in practice the relation between expertise and politics is ambivalent. In contested policy arenas, visibility can confer both authority and vulnerability: it secures access to decision-makers but also exposes scientists to blame when policies fail. Many pandemic-era experts tried to manage this tension by asserting both authority and deniability—presenting their advice as neutral "guidance" while emphasizing that elected officials made the actual decisions. In Ascher's terms, politicians relied on scientists to absorb uncertainty, but scientists also relied on politicians to absorb accountability. Their incentives therefore included not only the pursuit of influence but also the avoidance of responsibility: to shape outcomes without being seen as political actors. This dynamic helps explain the careful boundary-work

and public performances of neutrality that characterized figures such as Fauci and Collins throughout the pandemic.[11]

6 Origins

The role of protecting resources as an incentive for scientist-bureaucrats is nicely illustrated by the Origins episode. To see this, we need to look at three important documents that were heavily relied on to enforce gatekeeping claims about "lab leak" from being taken as anything other than a conspiracy theory-two from very early in 2020, and one from 2022. We will see three very prominent features of this episode that support this interpretation. The first is that domain experts in virology and virological research published a very influential paper that seemed to strongly conflict with the views that they shared with each privately. The second is that they seemed to have done this under strong pressure from high level scientific bureaucrats who had an interest in shielding from criticism the kind of scientific research that some suspected might have been responsible for a possible lab leak. A third prominent feature of this episode is that many members of the scientific community fell in line behind the narrative that the lab leak hypothesis was a conspiracy theory. This again supports the interpretation that scientists are sometimes incentivized by a desire to be *perceived* as advancing the public good.

The three important documents we need to examine are the letter to *The Lancet* that we mentioned above, the paper published in *Nature Medicine* called "The Proximal Origins of SARS-CoV-2", and a paper published in *Science* in July 2022 entitled "The Huanan Seafood Wholesale Market in Wuhan was the early epicenter of the COVID-19 pandemic."

6.1 The lancet letter

The *Lancet* letter was organized and drafted by **Peter Daszak**, the president of EcoHealth Alliance, an organization that had collaborated with and funded research at the WIV. One thing noteworthy about the letter is that it was signed by 27 prominent scientists, and later received over 20,000 additional signatures via a petition on Change.org. But as we have noted, at the time, many of the most knowledgeable experts were "literally swivel[ing] day by day thinking it is a lab escape or natural." So, it's somewhat natural to wonder what incentivized this mass behavior, and it's natural to suspect that neither the ordinary aims of science, nor a credit economy, can do the required work.

Peter Daszak's behavior in particular is noteworthy. The WIV was central to concerns about a potential lab-related origin of the virus. Critics argued that Daszak's connection to the WIV represented a significant conflict of interest that was not dis-

[11] A similar dynamic was identified in the aftermath of the 1976 swine flu episode, where CDC officials sought to preserve both public esteem and bureaucratic authority. As Neustadt and Fineberg (1978) observe, their self-conception as heroic protectors of the public good contributed to overconfidence and poor risk communication—a more hubristic version of the quest for public virtue analyzed here.

closed in the letter, which presented itself as an independent statement from scientists (Thacker, 2021). The letter's dismissal of the lab-origin hypothesis as a "conspiracy theory" clearly stifled debate and scientific inquiry (we can see this in the quotations from news outlets and agency officials that cited the papers). This framing, coupled with the reputational influence of *The Lancet*, discouraged open discussion and investigation into the lab-origin hypothesis during the early stages of the pandemic.

By 2021, reports highlighted Daszak's role in drafting the letter and his organization's ties to the WIV, sparking scrutiny. Some argued that the letter had been orchestrated to shield EcoHealth Alliance and the WIV from accountability. Public records obtained through Freedom of Information Act (FOIA) requests revealed communications showing the coordinated effort to craft the letter, further fueling allegations of a lack of transparency. Daszak signed a statement declaring he had no conflict of interest. This led The Lancet's editors to take the unusual step of publishing a 2021 "Addendum: competing interests and the origins of SARS-CoV-2," (Editors of the Lancet, 2021) in which they discussed Daszak's competing interests. Since that time, we have learned a great deal about Peter Daszak, and his organization's role in sponsoring the exact kind of research that very easily could have led to the creation of SARS-CoV-2.

The strongest evidence for this last claim comes from a 2018 grant proposal to DARPA called "Project Defuse: Defusing the threat of bat-borne coronaviruses." Authors of the proposal include leader Daszak, as well as UNC scientist Ralph Baric, and Wuhan Institute of Virology senior scientist Zhengli Shi. The proposal was to synthesize spike proteins with a Furin Cleavage Site (FCS) and insert them into coronaviruses from the same clade as SARS-CoV-1.

To be absolutely clear about this, DARPA rejected the application. But many suspect the work was carried out anyway. We know that Daszak had the ability to carry out the work even without DARPA funding. We know this because of an email he sent on April 28, 2020. The email concerned a different project called "Understanding the Risk of Bat Coronavirus Emergence" another collaboration between EcoHealth, Baric at UNC, and the WIV. It was funded by the NIH but suspended in April 2020. Daszak wrote "My plan is to continue this work, unfunded for now …" (in Corin, 2023). So, it's not at all unlikely that DEFUSE was carried out despite the DARPA rejection.

Everything we know about DEFUSE is alarming. Formally, the grant application proposes to do all the spike protein work at UNC, but a draft comment on a MS word document reads "Ralph, Zhengli. If we win this contract, I do not propose that all of this work will necessarily be conducted by Ralph, but I do want to stress the US side of this proposal so that DARPA are comfortable with our team. Once we get the funds, we can then allocate who does what exact work, and I believe that a lot of these assays can be done in Wuhan as well …" (in Select Subcommittee on the Coronavirus Pandemic, 2024: 2).

Daszak publicly tried to refute the claim that DEFUSE might have been at the root of the Covid-19 pandemic by reminding people that the work was to be done at UNC, not in Wuhan (Cohen, 2023). But that was before he knew that his draft comments would be FOIA-ed and that we would come to know that was not in fact the plan. The alarming thing is that part of the reason Daszak wanted the work to be done at WIV

was that he knew it could be done there more cheaply under BSV-2 level biosafety, whereas at UNC it would be expected to be at least BSV-3. (It's a four-level system.) In comments, Baric wrote that US researchers would "freak out" about this kind of work being done under BSV-2 (in Kopp, 2023a).

6.2 Proximal origins

The Lancet letter appeared just before the "Proximal Origins" paper appeared in *Nature Medicine.* Recall that the paper investigated the origins of SARS-CoV-2 and concluded that the virus most likely emerged through natural zoonotic spillover, rejecting the idea that it was deliberately engineered. They stated "Our analyses clearly show that SARS-CoV-2 is not a laboratory construct or a purposefully manipulated virus" (Andersen et al., 2020: 450). But evidence from Slack messages (and inter-agency communication channel) and other documents related to the authors of "Proximal Origin of SARS-CoV-2" suggests skepticism among the authors about their public conclusion that the virus could not have originated from a lab: Slack messages reveal they were privately concerned about a lab origin. For instance, Kristian Andersen expressed that lab escape was "so friggin' likely" (in Kopp, 2023b) due to the molecular data's consistency with such a scenario. Other messages show discussions on how the lab leak hypothesis could not be fully dismissed, which contrasts with their published paper's dismissal of this possibility. Notably, Robert Garry commented regarding the genetic engineering of the virus that "asking your graduate student to insert a furin site … would get you there" (in Kopp, 2023b) indicating skepticism about the complexity of lab manipulation being beyond possibility. Emails show that the authors, including Ian Lipkin, were considering lab origin possibilities before the paper's publication (Select Subcommittee on the Coronavirus Pandemic, 2023: 28–9). There are discussions about the virus potentially being engineered, with Andersen mentioning the difficulty in ruling out engineering completely due to modern virology techniques. There's documentation of a significant shift in the authors' public opinions after a teleconference with key figures like Anthony Fauci and Francis Collins. Before this call, some authors leaned towards a lab origin, but afterwards, they moved to a more definitive public stance against it, suggesting that they were in fact responding to non-epistemic incentives shaped by bureaucratic and political environments in reaching their final published conclusions. One email from Eddie Holmes alludes to "pressure from on high" (in Kopp, 2023b), indicating that there might have been external pressure to downplay the lab leak theory. Early drafts of the paper discussed lab-based scenarios like serial passage, which were later omitted or downplayed in the final version. This suggests a deliberate choice to alter the narrative presented in the paper. Finally, many have highlighted the contrast between the authors' private doubts and their public statements in 'Proximal Origins.' For example, Andersen's later statements about not being fully convinced about ruling out cell culture involvement contradict the paper's conclusion.

In sum, a large body of evidence indicates that the authors might not have fully believed their public conclusion, showing a discrepancy between their private deliberations and the published narrative, which is especially noteworthy given the apparent political pressure the authors were under: the 'pressure from on high' identified

by Holmes referred to a February 1st 2020 conference call between the authors of 'Proximal Origins' and Farrar, Collins, and Fauci (among others), with the pressure originating from Collins and Fauci, as later identified by Robert Garry (in Select Subcommittee on the Coronavirus Pandemic, 2023: 25). Indeed, it seems very likely that Farrar was involved in drafting some of the key language of the paper that suppressed uncertainty about the origin of the virus, and violated scientific ethical norms (sometimes called 'ghostwriting') by having drafted some of the paper and not including his name among the authors (Select Subcommittee on the Coronavirus Pandemic, 2023: 26). Publicly, Andersen and Holmes claimed that they changed their minds because of new data regarding Pangolin viruses, but privately, and in testimony they aligned themselves with Garry's view: that the pangolin data "are interesting, but they, you know, by themselves, don't tell you that, the virus was natural or from a lab" (in Select Subcommittee on the Coronavirus Pandemic, 2023: 33).

6.3 The huanan seafood market

Finally, we should look carefully at the third of the three central documents regarding Origins, the July 2022 paper in *Science*. Though the lead author of this paper is Michael Worobey, four other authors comprise four of the five authors of 'Proximal Origins': Kristian G. Andersen, Andrew Rambaut, Edward C. Holmes and Robert F. Garry. In addition to the fact that we know the private opinions of these authors from their Slack messages and emails, there are two problems with this paper. The first is the model, the second is the data the model relies on.

We'll start with the data. First of all, we know that the number of identified cases is much smaller than the total number of people who would have been infected. The authors of The Huanan Seafood … insist that the data they have is representative. They deny, in other words, that there was 'ascertainment bias' in collecting the cases. Ascertainment bias is what you would have if you looked for discarded bottlecaps in the dark, and concluded that they were primarily discarded in the vicinity of lamp posts. But recently, George Gao, director of the Chinese Center for Disease Control, admitted that there almost certainly was ascertainment bias in the collection of cases. He says there was special focus on the Seafood Market and people associated with it in the early search for cases (in Ridley, 2024). In fact, Worobey et al. (2022) claim to use their model to show that there is no ascertainment bias. So, given that we now know there was ascertainment bias (George Gao was responsible for collecting the data, and he says so), what does this show about their model? Finally, Demaneuf (2022) has conclusively shown that the data Worobey et al. had access to were only a subset of the cases the Chinese authorities are aware of. And we have no idea what selection criteria they used in making that subset available (Duenwald et al., 2022).

What about the model? In January 2024, Stoyan and Chiu (2024a), published "Statistics did not prove that the Huanan Seafood Wholesale Market was the early epicentre of the COVID-19 pandemic" in the *Journal of the Royal Statistical Society Series A*. Even taking the data at face value, they argued that the "statistical conclusion is invalid on two grounds:" (a) The assumption that a centroid of early case locations or another simply constructed point is the origin of an epidemic is unproved. (b) A Monte Carlo test used to conclude that no other location than the seafood market

can be the origin is flawed. Hence, the question of the origin of the pandemic has not been answered by their statistical analysis." (Stoyan & Chiu 2024a: 710).

Perhaps more alarmingly, in a subsequent blog post on the website of the Institute of Mathematical Statistics, they wrote: "We are pleased to have published our critique. However, we are astonished that no other colleagues have reported similar findings. This situation raises doubts about the current state of the system of modern science and the general understanding of basic principles of statistics in modern society, not to speak about the reviewing process of Science" (Stoyan & Chiu 2024b).

6.4 Analyzing origins in terms of the incentive of protecting resources

So, what is going on in all of this? This is a very difficult case to untangle. One plausible interpretation of the behavior of the authors of "Proximal origins" is that they they faced strong institutional incentives to remain in the good graces of powerful agency bureaucratsres like Fauci and Collins. This is a plausible way to interpret both the discordance between their private views and the public statement that "Our analyses clearly show that SARS-CoV-2 is not a laboratory construct or a purposefully manipulated virus," as well as the private statement that there was "pressure from on high"–not to mention the degree to which the manuscript changed over the course of just a few days: going from a balanced portrayal of the evidence to an unequivocal denial of the possibility of lab leak. The huge number of signatories of the Lancet letter, along with what the authors of the Royal Society paper say in their blog post, strongly suggest that another piece ought to be added to the incentive structure of scientists, and that's the desire to be seen as part of a consensus that has been successfully portrayed as morally virtuous. Who does not want to endorse a scientific claim that demonstrates "solidarity with all scientists and health professionals" and prevents "jeopardis[ing] our global collaboration in the fight against this virus"? What else explains 27,000 signatures behind a hypothesis which in reality was highly uncertain, or the 'doubts about the current state of the system of modern science' that Stoyan and Chiu highlight. Neither truth seeking nor credit seeking explain this behavior. The fact that the Lancet letter received 27 signatures, and 20,000 more, in support of the idea that the lab leak hypothesis was a conspiracy theory worthy of public condemnation, at a time when the epistemic situation was, in the minds of the mostly knowledgeable people, extremely uncertain, suggests that in scientific episodes of significant public policy significance, many scientists are incentivized by a desire to be *perceived* as on the side of public good, even when there are not in a particularly good position to assess what side that is.

The dynamics described above can be illuminated by work in behavioral economics on herd behavior and reputational signaling. In their classic model, Scharfstein and Stein (1990) show that managers facing uncertainty may rationally imitate the investment decisions of their peers, even when private information counsels otherwise, because conformity minimizes reputational risk. The same logic applies to scientist-bureaucrats operating in high-stakes policy environments. When the reputational rewards of being aligned with a perceived moral or epistemic consensus outweigh the potential gains from dissent, mimicry becomes an individually rational but collectively distortive strategy.

The pandemic offered textbook conditions for such herding. The Great Barrington Declaration, though broadly consistent with pre-COVID public-health orthodoxy on targeted protection, was rapidly stigmatized as "fringe." For scientist-bureaucrats, the reputational cost of even partial sympathy with its authors vastly exceeded any epistemic benefit from open debate. Aligning with the dominant consensus—signing letters, amplifying official narratives, or simply declining to question them—served as a low-risk way to signal virtue, loyalty, and prudence. Much as corporate managers imitate each other to safeguard their careers, scientist-bureaucrats mimic the cues of institutional peers and superiors to safeguard access to funding, media standing, and public esteem.

Historical analogues reinforce this interpretation. Neustadt and Fineberg's (1978) account of the "influenza fraternity" during the 1976 swine-flu campaign depicts similar groupthink and overconfidence born of intra-elite emulation. In both crises, reputational mimicry substituted for independent judgment: appearing decisive and morally upright outweighed the risks of error. A public-choice philosophy of science thus requires attention not only to explicit incentives like budgets or influence, but also to positional incentives that reward conformity and punish epistemic independence. This dynamic is compounded by the asymmetry of reputational risk. As one reviewer aptly noted, aligning with the majority insulates decision-makers from personal blame: when prevailing consensus proves wrong, the error is collectively absorbed and socially diffused. In contrast[12], those who dissent from orthodoxy and are later shown to be mistaken incur the full reputational burden alone. This imbalance encourages conformity not just for material gain, but also as a rational hedge against reputational exposure. A public-choice philosophy of science must therefore account for this subtle but powerful incentive structure, where the safety of shared error outweighs the risks of solitary dissent—even in moments that call for independent judgment.

Finally, what incentives lay at the root of the behavior of people like Daszak, Collins, Fauci, Drosten and Fouchier? To get to the root of this, we may have to wait and see if more evidence emerges. But we can certainly put forward a hypothesis, and suggest that it ought to be part of the toolkit of public choice philosophy of science. And that's that science often involves large research programs that depend on large allocations of resources. And it stands to reason that scientist-bureaucrats are often incentivized to protect these resources from public criticism. When scientists understand that senior administrators and program directors can shape which research areas and methods are viewed as fundable, they internalize those priorities. At agencies like the NIH, this influence operates less through direct control of individual grants than through agenda-setting—defining "high-priority" areas, issuing targeted funding calls, and exercising limited discretionary authority over allocations and renewals. As multiple commentators have noted, figures such as Anthony Fauci and Francis Collins possessed substantial, though indirect, leverage over scientific careers and research trajectories. Again, we will need to wait for more evidence to know if this motivated the people 'on high.' That said, one can reasonably infer the structural stakes involved. Had SARS-CoV-2 been widely accepted as the result of a

[12] Thanks to Monte Cairns for making this point.

laboratory spillover, the political consequences for virological research would likely have been severe. Congressional and international scrutiny of biosafety practices would have intensified; gain-of-function and high-containment work might have faced funding freezes or statutory restriction; and the autonomy of major biomedical agencies would almost certainly have contracted under new oversight regimes. For senior scientist-bureaucrats whose careers and institutional influence depended on steady research support and minimal external regulation, such a scenario would have represented an existential threat to their resource base and professional independence. Even without direct evidence of conscious self-protection, it is therefore plausible that bureaucratic caution over the lab-leak debate reflected these underlying structural incentives rather than mere epistemic judgment.

The incentive patterns identified here are not unique to pandemic science, or even to gatekeeping; they reveal structural features of contemporary expert institutions that a public-choice philosophy of science could investigate across fields and across other activities.

7 Conclusion

Looking at episodes of gatekeeping from the Covid-19 pandemic reveals that a certain class of scientists behave more like bureaucrats than the traditional picture of science that philosophers normally offer. If this is right, then a popular framework for analyzing the behavior of bureaucrats, public choice theory, might be fruitfully applied to the analysis of gatekeeping in science. In public choice theory, it is often emphasized that while we traditionally view public servants as pursuing public support, usually in the form of votes, by pursuing the public good, they often pursue other goods, such as job security, budgets, prestige, power, or minimizing workload. This is closely analogous to the claim, made in the philosophy of science, that while we traditionally view scientists as pursuing truth, they often actually pursue credit. Combining these insights, we asked: 'when scientists are more like bureaucrats than inquirers, purporting to pursue the public good more directly than they purport to pursue knowledge, can a similar analysis be offered?' The two episodes of gatekeeping that we looked at suggested that the answer is yes. When scientist-bureaucrats gatekeeping, they might very well be best understood to be pursuing such incentives as *1) Attention and a public perception of scientist-bureaucrats' importance; 2) Political influence; 3) Protection of the resources they have worked hard to capture* as they are to be best understood as pursuing the public good.

Finally, while this paper has focused on gatekeeping as the most visible arena in which scientist-bureaucrats act, the incentives identified here are not confined to gatekeeping alone. Similar dynamics likely shape how scientific agencies set research agendas, allocate funding, and communicate uncertainty. Future work could extend this public-choice analysis beyond gatekeeping to the broader governance of science itself, tracing how institutional incentives shape epistemic norms long before decisions about inclusion or exclusion arise.

The two pandemic episodes discussed here serve primarily as illustrations of a broader phenomenon: how scientists who operate at the interface of research and

administration respond to incentive structures that reward coordination, clarity, and conformity over epistemic adequacy. A public-choice philosophy of science reframes these behaviors not as moral failings but as predictable responses to institutional conditions. When epistemic agents are also bureaucratic agents, their self-interest becomes multidimensional—encompassing professional reputation, institutional stability, policy relevance, and perceived public virtue alongside the pursuit of truth. Recognizing these heterogeneous motives allows philosophers of science to analyze crisis- and policy-driven research using the same tools economists use to study bureaucratic behavior: by asking which incentive structures produce which epistemic outcomes. The hope is that future work will extend this framework to other domains—such as climate modeling[13], biomedical research, or risk regulation—where similar tensions between bureaucratic incentives and epistemic norms shape what counts as good science.

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[13] In fact I do just this in (Winsberg, 2025)

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