HOPOS 2022
Plenary Lecture
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[slide 1]

In the summer of 1992, thirty years ago, a small group of people formed the History of Philosophy of Science Working Group. Two years later, the first HOPOS conference took place, organized by Joseph Pitt and our current President, David Stump.
HOPOS conferences have gone on to provide me, and many of you, with our most important intellectual community.

[slide 1b]

If we want to transport ourselves back to 1992, all we have to do is read the opening paragraphs of the first HOPOS newsletter, which went out in January the next year, by email. It says:

> "Here is the first issue of the HOPOS Newsletter... HOPOS is also distributing a hardcopy version of the Newsletter to those who request it. But in order to minimize the cost of distribution, we would like to distribute it to as many people as possible in this electronic form. So please bear with us as we experiment with a new form of communication."

Looking back through the old conference programs is a lot of fun -- seeing the wide variety and range of topics covered, and the continuity in some of the themes. One of the instigators of HOPOS was Don Howard, and at the very first HOPOS conference he gave a talk about Einstein, and Einstein's distinction between constructive theories and principle theories. This is something I'm going to make use of later today.

[slide 1c]

If we go back another thirty years, we come to 1962, and to the publication of Kuhn's *Structure of Scientific Revolutions*. Love it or hate it, Kuhn's book continues to influence our field, for better and for worse. In my talk today I'm going to make an attempt to escape its clutches for the way we think about 18th century physics. And we'll see how that goes.

Going back another thirty years, and 1932 lands us in the middle of the activities of the Vienna Circle. Revisiting *that* work in its historical and political context has been an important strand of HOPOS activity ever since the formation of the Working Group.
One source for Einstein and for the Vienna Circle was Ernst Mach. And it's Mach that I'm going to start with today.

But before I do any of that, I'd like to invite you to join me in two big thank yous.
First, to our past president, Mary Domski, and her steering committee, program committee, and local organizing committees who attempted to put on our last conference in not one, but two, different locations. In the end we weren't able to meet because of the pandemic, but a big thank you to them for all the hard work they put in.

Second, to our current president, David Stump and his steering committee; also the program committees chaired by Gideon Manning and Flavia Padovani, and the local organizing committee here, chaired by Jeremy Heiss. It's a wonderful thing to be gathering together again, many of us in person, some of us via Zoom, and it takes an enormous amount of work to pull it off. So thank you.

Okay then. Now to my talk.

[slide 2]

I want to do three things:

- I want to present a different way of thinking about 18th century physics, philosophy and mechanics, and the relationships between them
- I want to support this picture with some details, including talking a little bit about my personal favorite philosopher from the time, Emilie Du Châtelet
- And along the way I want to highlight some methodological points that are important for doing HOPOS, and this is where Einstein and his constructive and principle theories come in

So let's make a start.

[slide 3]

According to Ernst Mach,

> "The principles of Newton suffice by themselves, without the introduction of any new laws, to explore thoroughly every mechanical phenomenon practically occurring, whether it belongs to statics or to dynamics. If difficulties arise in any such consideration, they are invariably of a mathematical, or formal, character, and in no respect concerned with questions of principle." (1883)

So according to Mach, once we have Newton's Principia, classical mechanics is complete as regards its principles. All that remains is the technical challenge of *using* these principles to treat more and more complex and difficult phenomena.

This view of post-Newtonian mechanics is perpetuated in Kuhn. For him, Newton's *Principia* is the culmination of a scientific revolution, after which "classical mechanics" is all about normal science within the Newtonian paradigm. The principles, methods, and basic ontological commitments are secure, all we need do now is solve puzzles ... until we reach another crisis point with Einstein in the early 20th century.

This way of framing things highlights *some* aspects of the history of physics, and makes others unimportant or invisible.
It focuses our philosophical attention on questions such as "What are the principles of Newton's Principia?", "How are they best understood?" and "What was revolutionary about Newton's work?"
And it invites us to skip over the 18th century – nothing philosophically interesting to see here – until we get to relativity and quantum mechanics. Then we can ask: "What unsolved puzzles led

to a crisis for the Newtonian paradigm?" "What was revolutionary in what Einstein did next?" And so on.

This is a caricature, of course. But it highlights an important truth: The way we tell the history of science makes *some* of our philosophical inheritance very visible, and the rest completely invisible. It makes the work of some physicists, such as Newton and Einstein, philosophically important; and the work of others, such as Euler and d'Alembert, philosophically inert – after all, all they were doing was using Newton's principles to solve problems within the already-existing Newtonian paradigm.

Many of us in this room (and on Zoom) know that there is more to 18th century mechanics and physics than puzzle-solving within that paradigm. This is not a new point to make.
But for the significance of this to really shine through, for it to be something we can use in our research and teach in our classrooms, we need a different way of framing the history, one that's very different from Mach's or Kuhn's.

[slide 4]
I'm going to offer you one possibility for that today. My suggestion is that we think of the 18th century as a Golden Age for philosophical mechanics.

To make the case, I'm going to look at work by three people from the middle of the 18th century: Du Châtelet, d'Alembert and Boscovich. This will enable me to explain what I mean by "philosophical mechanics" and show why I think it gives us a powerful way of thinking about physics, philosophy and mechanics in the 18th century.

[slide 4b]

Most of what I'm going to talk about today comes from a joint project with Marius Stan, something that he and I have been working on for years but now at last it's almost done. Our book is called *Philosophical Mechanics in the Age of Reason*. Shameless advertising.
I do want to say that I count myself incredibly lucky to have worked on this book with Marius. It's been a truly collaborative project, so while I have the honor and the privilege of presenting some of our work today, almost everything that you'll hear comes from our joint efforts.

[slide 5]

So here's what I'm going to do.
First I'll say a little something about physics in the early 18th century.
This will give us an on-ramp into talking about the philosophical mechanics of Du Châtelet, d'Alembert, and Boscovich.
In the course of that, some points will come up about tools and methods for doing HOPOS. This is where Einstein's constructive/principle distinction comes in, as I said.

[slide 6]
Perhaps somewhat surprisingly, in the early 18th century, decades after Newton's *Principia*, physics and mechanics were still separate disciplines.

They were practiced by largely different groups of people, with different goals, different methods, and different criteria for success.

"Physics", as that term was then used, labelled a subdiscipline of philosophy, practiced by philosophers. It was responsible for the theory of bodies in general. It was largely qualitative, and its job was to provide an account of the nature, properties, behaviors, causes and effects of bodies, along with causal explanations for the behaviors of bodies, their changes and their motions and so on.

Mechanics was a branch of mathematics, practiced by mathematicians. It included statics along with mathematical theories of bodies in motion.
It was *about* bodies -- it took bodies as its objects -- but it presupposed them. It wasn't responsible for giving a theory of bodies. That was the job of physics.

[slide 7]
The central questions of physics were:
What is a body?
How do bodies act on one another?
And how can we know?

[slide 7b]
This is familiar from the debates over graviation after the publication of Newton's Principia. Philosophers argued over the *nature* of bodies: is gravity an essential property of bodies or not? And they argued over how bodies *act* on another: is contact action the only intelligible means of body-body interaction, or can bodies act on one another at a distance?

[slide 8]
On this latter point, here's one of my favorite quotes from Leibniz, where he's ranting against the intelligibility of gravitational action at a distance:

> But then what does he mean, when he will have the sun to attract the globe of the earth through an empty space? Is it God himself that performs it? But this would be a miracle, if ever there was any. …
> … That means of communication (says he) is invisible, intangible, not mechanical. He might as well have added, inexplicable, unintelligible, precarious, groundless, and unexampled.
> … If the means, which causes an attraction properly so called, be constant, and at the same time inexplicable by the powers of creatures, and yet be true; it must be a perpetual miracle: and if it is not miraculous, it is false. 'Tis a chimerical thing, a scholastic occult quality. (Leibniz, 1716)

The contrast here is with the contact action of the mechanical philosophy. For mechanical philosophers, the only intelligible means by which one body can act on another is through contact. This very intelligibility was supposed to be one of its key advantages.

But by the time Du Châtelet was writing, in 1740, people had begun to wonder whether contact action really is so intelligible.

[slide 9]
Here is Maupertuis making this point, in 1732:

> The common People are not at all surprised when they see a Body in motion communicate its Motion to others, for being used to this Sight they see nothing wonderful in it:
> but Philosophers who are resolute enough to decide a priori concerning what Properties are to be admitted in Bodies, and what excluded; such Philosophers I say **cannot conceive the impulsive Force more conceivable than the attractive**.
> What is this impulsive Force? How does it reside in Bodies? Who could have imagined it to have been resident therein, before he had seen the shock or congress [i.e. the collision] of Bodies?

[slide 10] So all these things were up for grabs, as live and pressing questions
[slide 10b] They are what Marius and I call the "problem of bodies"
And they are what Du Châtelet set out to deal with in her Foundations of Physics
[slide 11]

She worked on method – she has a famous chapter on hypotheses
And she worked on the nature of bodies and the means by which they act on one another, looking at both gravitation and contact action.

To give you an idea, here is the list of chapters in her book: **[slide 12]**
There's a lot of different topics in this book, she's covering a lot of different material, and if you look at the secondary literature you find that people have had trouble seeing how this is a single, unified text.

But if you come at the book from the perspective of the problem of bodies, then there's a strong line of argument that runs through the whole thing, from beginning to end, from the questions she raises in the early chapters about epistemology and the appropriate methodology for doing physics, to her account of bodies in the middle chapters, to her investigations into the two ways in which bodies were thought to act on one another: through contact action and Newtonian attraction.
[slide 12b] More shameless advertising. This is the book where I make the case that the problem of bodies is an important unifying theme of Du Châtelet's *Foundations*.

The details of this interpretation don't matter for my purposes today.

What matters is this. Given the topics that she covers here, it's going to be very hard to see the importance of her book if you think that the "Newtonian paradigm" was already established by the time she was writing. Here's Kuhn (*Structure*, pp. 4-5): **[slide 13]**

> "Effective research scarcely begins before a scientific community thinks it has acquired firm answers to questions like the following: What are the fundamental entities of which

> the universe is composed? How do these interact with each other and with the senses? What questions may legitimately be asked about such entities and what techniques employed in seeking solutions?"

These are exactly the questions about the foundations of physics that Du Châtelet was engaged with.
So if you think they've already been resolved by the time she was writing, in 1740, then what she's doing is going to be invisible to you.
But these questions were not resolved for physics at that time. They were very much alive. And Du Châtelet tackled them head-on.
[slide 14]
When we re-frame how we think about the 18th century, her work becomes visible.
It's dealing with the most important unsolved problems in the foundations of physics inherited by the generation of philosophers who came after Newton, Huygens and Leibniz.

[pause]

[slide 15]
Here is Du Châtelet's strategy for addressing the problem of bodies, where bodies act on one another via collision.
First, you ascertain the nature and properties of bodies.
Then, you reason from this to the general laws of motion.
Next, you use these resources to develop an account of collisions.
This will be both qualitative, drawing on the resources from physics, and quantitative, because when you develop your account of collisions you integrate the quantitative rules of collision into your theory.

And this is what Marius and I mean by a "philosophical mechanics"

You see this strategy not just in Du Châtelet but everywhere at the time.

- In the Malebrancheans in France
- The Leibnizians, such as Wolff and Jakob Hermann
- The Newtonians – such as John Keel (Keill) and Colin MacLaurin

There's a great deal of activity on this topic in the 1720s and 30s.

If we look back at all this activity from a Machian-Kuhnian perspective then what they're doing makes very little sense, but with the framework of philosophical mechanics available this shared project that they're all engaged with, of integrating physics and mechanics, has clear goals and methods

What's cool about Du Châtelet is that hers is the most developed, and the most promising, attempt at this.

She gives an account of body in terms of extension, active force and passive force
she has laws of motion that are modified versions of Newton's laws (she's not alone in modifying his laws)
and she has an explicit account of what happens when two bodies come into contact and press on one another. It's all there in her book.

But if, following Mach, you think that the principles or laws were already settled by Newton, decades earlier.
If, following Kuhn, you think that the basic entities of the Newtonian paradigm are already in place by the time Du Châtelet was writing,
then her work and her book become invisible.

If instead we

- recognize that at the time Du Châtelet was writing, physics and mechanics were still separate disciplines
- notice that there were widespread and extensive efforts going on to integrate the two, to create a philosophical mechanics
- approach the 18th century as a century of philosophical mechanics

then suddenly everything that she's doing in her book comes into view as interesting and important

[slide 16]
Reframing our history using the lens of philosophical mechanics makes DC's Foundations of Physics an extremely important book, and this is reflected in the prominence and prestige it received at the time.

So this is great. We change our framework, and suddenly we can make sense of this book. Except that it's not so easy. Obviously. As any of you who have worked on "non-canonical" authors will know.

- one obstacle: had not been translated into English
  - problem for those of us with questionable language skills
  - and more importantly: in English-speaking countries the lack of an English translation is a huge practical obstacle to bringing the materials into the classroom

- Here's another related, but even more important, obstacle: when materials haven't already been worked on in detail by previous generations, there's a lot of preliminary work and ground-clearing to do, and scholarly infrastructure to build. And that takes time. And it takes a team. So a shout-out to all the people who worked with me on the translation of the Institutions, and to everyone who has labored to create that scholarly foundation that makes the next level of research on Du Châtelet's Foundations of Physics possible. And also, gratitude to everyone who has done this, and is doing it, for other figures. Thank you.

But back to my story. As I said, Du Châtelet gives us the most systematic attempt of the time to provide a philosophical mechanics of collisions. Which is great. But it didn't work. There are interesting reasons why not that are internal to her project,
but there's also a sense in which it was doomed to fail for external reasons
and this is because of developments in rational mechanics at the time

[slide 17]

First, it had become clear to those working at the forefront of mechanics that Newton's laws were not adequate for treating extended bodies: new principles were needed.

Second, it had become clear to these same people that, from the perspective of mechanics, collisions are a really complex problem, one that needs to be situated in a more general theory of the motions of bodies.

Third, it was clear that a general theory of the motions of bodies requires a treatment of constrained motion.
One reason for this is that the parts of an extended body don't move freely under the influence of forces; they are constrained to move together.

The upshot is this: A philosophical mechanics needs to integrate more than just the rules of collision from mechanics; it needs a complete mechanics of the motions of extended bodies.
And this means that the theory of constrained motion becomes the locus of research in mechanics that's most relevant to the problem of bodies.

[slide 18]
And this brings us to d'Alembert. Because the most important work on constrained motion mid century is d'Alembert's 1743 *Treatise on Dynamics*.

And here we run into similar obstacles that we faced with Du Châtelet's Foundations.
There's no English translation.

There is some scholarly infrastructure in the philosophy of science literature, but nothing like what you find for Newton's *Principia* or Einstein's 1905 special relativity paper. Not even close.

But there's also a third obstacle. We've arrived at d'Alembert's book because of our interest in the problem of bodies, and we want to know what it has to say about it. But here's the issue: the "problem of bodies" was not d'Alembert's primary concern in this book. What should we do?

This is a challenge we often face as HOPOS scholars, where scientists are doing work that is important for the philosophical questions that we have, but the scientists themselves are not trying to address those questions, at least not explicitly.
So we have to pause, and think about our methodology. Whereas in Du Châtelet's case the problem *I'm* interested in, the problem of bodies, is also the problem *she's* interested in.
But that's not true for d'Alembert. So how should we approach reading his book for its relevance to the problem of bodies?

This is tricky, so let's talk a little bit about methodology.

[slide 19]

When Marius and I were writing our book, we chose to deploy three methodological heuristics, all of which are relevant, the last one most obviously so. Here they are.

First, we try not to read more meaning into a term than is supported by the use that our author makes of that term. So, when d'Alembert uses the term "body", we try to recover meaning from use, in his philosophical argumentation and in his theoretical problem-solving.

Second, when we're working through his arguments we try to follow George Smith's recommendation of using only those resources that someone who was fully up to speed with the state of the art at that time, could have understood.
This is easier said than done. Words change their meanings and concepts evolve over time, and we have different conceptual and technical resources available to us now than d'Alembert had then. But this second heuristic is a useful methodological guide for trying to avoid problematic anachronism.

Finally, we try to make sure that whenever we're talking about a problem, we're clear about whose problem it is. We never treat the problem of bodies as "floating free", with a life of its own (as "disembodied", so to speak). So when we're reading d'Alembert, whose primary motivation was *not* the problem of bodies, we want to avoid suggesting that he was somehow mistaken about his own project, engaged in working on the problem of bodies without knowing it; his is not a sleepwalker.
We need to be clear that what we're doing is bringing a problem that *we* care about, and that we see in *other* philosophers of the period, to his work. And we're looking to see how the moves that d'Alembert makes, in pursuit of his own project, are relevant to that problem, as it stood at the time.

We can use our own labels for concepts and distinctions that are supported by these heuristics, but in the places where we go beyond what these heuristics allow, we aim to say so explicitly. We aim to make careful and deliberate use of anachronism. We'll see an example of this when we come to Einstein's constructive/principle distinction.

There are obviously different methodological choices that one can make. I think the important thing is that we are explicit about which ones we are adopting. This is something that I haven't been very good at in the past, and working on this book with Marius has forced me to think about it a lot more.

With these methodological provisos in place, we can go back to reading d'Alembert's book.

[slide 20]

(1) We can start by looking at what he says.
Since rational mechanics at the time claimed to be offering a science of bodies in motion, we can look at d'Alembert's explicit statements about bodies, and think about how far we can get in solving the problem of bodies using this content that he provides. We'll try that in a minute.

(2) We can also look at what he does.
We can try to uncover and analyze the philosophical significance of the moves d'Alembert makes for *our* problem, *independent* of his own motivations and purposes. We can look at the problems *he* was trying to solve, the presuppositions and assumptions he makes in order to do so, and the means by which he arrived at solutions, to see how these developments bear on the problem of bodies.

In both cases, we are all the while recognizing that it is *we* who are bringing the problem of bodies to d'Alembert's work.

[slide 21]
So let's take a look at what he says. His very first definition concerns body. He says:

> "If two portions of space, equal in size and shape to one another, cannot be imagined to be unified and combined in such as way as to make one portion of extension whose total size is less than that of the sum of the original two portions, then these two portions of space are impenetrable to one another, and are what we call bodies"

And if we read through the rest of the Definitions and Preliminary Notions we find that bodies are: extended, impenetrable, mobile, and follow continuous trajectories in space.

[slide 21b] So then we can ask: Does this provide a concept of body sufficient for the purposes of d'Alembert's rational mechanics?

To decide that, we need to read on a bit in the book.

[slide 22]

Here's something else he says, when he's introducing the first principle or axiom of his theory. He says,

> the force of inertia is ”the property bodies have for remaining in the same state of rest of or motion in a straight line”

So he's mentioning another property of bodies.
And then if we carry on, and work through the rest of the book, and through all the problems that he solves in his mechanics, his bodies also seem to have mass, some kind of resistance, and to be hard. So it seems as though the original definition that he gave us is incomplete, and we need to add more to our concept of body the further we get into d'Alembert's theory of constrained motion.

So this looks like kind of a mess philosophically, but I think it would be premature to be that dismissive. I think here's where some judicious anachronism is useful.

[slide 23: constructive and principle theories]

So now let's turn our attention to Einstein.
In 1919, in a piece on relativity that he wrote for the London Times, Einstein distinguished between constructive theories and principle theories.

I think there's general agreement in the literature that theories are not themselves either constructive or principle. It's more the approach to theorizing, or to the intepretation of a given theory, that can be viewed as being constructive or principle
And that's how Marius and I use this terminology.
With that caveat in mind, we can think of Einstein's distinction like this:

Constructive theorizing begins with an account of the material constitution of the objects of interest (bodies, systems, fluids, whatever).
Once you have the material constitution, you then use that to develop an account of the rules and laws that such objects or systems satisfy.
This is a constructive approach to theorizing in physics.

We've seen constructive theorizing already in Du Châtelet.
This was her strategy for solving the problem of bodies, and the strategy of everyone else at the time too.
They were adopting a constructive approach to theorizing in physics.

Einstein contrasted the constructive approach with a different way of going about theorizing, where instead we begin from principles that are held to be very general.
These principles are independent of the particular material constitution of the target objects or systems (and I'll give you an example in a minute).
And then we derive the consequences of these principles. These consequences are conditions that any material system, no matter its constitution, must satisfy.
And I think this is going to be a useful way of thinking about d'Alembert's theory.

Now of course, the use of this terminology in an 18th century context is anachronistic, but as a tool of analysis Marius and I think its directly relevant.

Einstein did not create his constructive/principle distinction from nowhere. Don Howard, in his talk back at the first HOPOS conference, traced it back into the mid 19th century, and Marius and I think it has its origins in philosophical mechanics: in the amalgam of physics (as it was once understood, with its constructive approach) and rational mechanics (with its search for principles that can serve as axioms), this amalgam that went on to become the physics of the 19th century.
If this is right, then using the distinction to talk about Du Châtelet's strategy, and -- more importantly -- using it to analyze d'Alembert's *Treatise* -- might turn out to be a helpful anachonism, productive of new insights. In our book, Marius and I argue that it is.

As a first step, let's have a closer look at the principle approach to Einstein's special theory of relativity **[slide 24]**

This will be very familiar to many of you.
The two principles of Einstein's theory are the light postulate and the relativity principle.
These are intended to be general -- that is, to apply to all material systems -- and to be independent of the particular material constitution of any given system.

In Einstein's case, the justification for the choice of principles was inductive: they were consistent with all known phenomena at the time.

Famously, what Einstein does in his 1905 paper, is to derive the consequences of adopting these two principles.
This gives him the Lorentz transformations. These are a condition that any material system, no matter its constitution, must satisfy.
In that sense, this condition brackets dynamics into kinematics.
We don't need to know about the material constitution of a rod, or about the forces holding it together, in order to know that it will contract.

[slide 25]
If we read d'Alembert's Treatise through the lens of a principle approach to theorizing, we get a helpful parallel.

D'Alembert has three principles:

- force of inertia
- composition of motions
- equilibrium

and from these he arrives at "d'Alembert's principle", which is his "general solution" to all problems in mechanics, it's a general condition that all relevant systems must satisfy.
So you can see the parallel.

D'Alembert's three principles, as well as "d'Alembert's principle" itself, have all the key features that you would expect of a principle theory: **[slide 26]**

They are intended to be general
They are intended to be independent of the material constitution of the sytem in question
And d'Alembert's Principle brackets dynamics into kinematics.
Remember that in Einstein's case, we don't need to know anything about the material constitution of a rod or the forces holding it together in order to know that it will contract in accordance with the Lorentz transformations
Similarly, in d'Alembert's case we don't need to know anything about the the material constitution of the constrained systems, or the forces implementing the constraints, in order to know that the system will move in accordance with d'Alembert's Principle.

So it's not implausible, I think, to use a principle approach as a way of investigating the content and structure of d'Alembert's theory.

Suppose we do this, and take a principle instead of a constructive approach to the problem of bodies in d'Alembert's Treatise.
Then instead of looking for a constructive material account of bodies, and looking for him to have that in place prior to theorizing about the motions of these bodies, instead we ask a different question:
[slide 27: bodies]

> To what extent are d'Alembert's definitions, axioms and principles sufficient to determine a concept of body adequate for the purposes of his theorizing?

This is one way to look for meaning from use, and it's one that's suggested by adopting a principle reading of d'Alembert's theory.

Of course there's no guarantee that any single concept of body is going to emerge from this as the object of d'Alembert's mechanics.

My point is that the constructive/principle distinction allows us to take this alternative route, the principle approach, to probing and interpreting d'Alembert's theory.
And that doing so is fruitful of important insights into the structure and content of his theory, one that gives us an alternative to the constructive approach to the problem of bodies.
So it opens up an avenue of enquiry.

But rather than pursuing that any further here, I want to end now by turning our attention back to the constructive approach, and to someone who deliberately tried to give a constructive account of bodies consistent with the latest developments in mechanics: Boscovich.
Boscovich explicitly tried to develop what Marius and I call a philosophical mechanics: an integration of philosophical physics with rational mechanics. Let's take a quick look.

[slide 28]
His theory consists of three parts:

Part I is an outline of the theory as a whole, the "philosophical mechanics", consisting of a mechanics and a physics.

In Part II, Boscovich gives us his mechanics, by which he means the mathematical treatment of motions under forces. The task is to find the motions given the forces and the forces given the motions -- this is Newton's conception of rational mechanics.

In Part III, we get Boscovich's physics: an account of the general properties of bodies, their different species, and the changes, alterations and transformations that they undergo.

Famously, Boscovich attempts to build extended bodies from point particles interacting by means of a force that oscillates with distance between being attractive and repulsive, and bodies are extended stable configurations of these particles.

[slide 29]

It's very natural to interpret Boscovich's theory constructively. He starts with his point particles and force function and from there tries to build up to an integrated physics and mechanics of extended bodies.

For our purposes, the main advantage of his theory is that we get an explicit and detailed attempt to construct bodies.
We also know what the underying ontology of the theory is -- the point particles and the force function -- so the theory has ontic unity.
It's also a very interesting theory because it appeals to the same principles as d'Alembert -- "force of inertia" (more like d'Alembert's than Newton's); composition of motions or forces; and equilibrium.

This is a philosophical mechanics that goes beyond the early 18th century and its attempts to integrate the rules of collision into physics, and instead tries to incorporate the very latest developments in mechanics; specifically, d'Alembert's theory of the motions of bodies subject to constraints.

It's a remarkable theory.

However, its main advantage, viewed from this perspective -- of providing the constructive account of bodies that was "missing" from d'Alembert -- turns out to be a double-edged sword.

One of the powerful things about d'Alembert's theory of constrained motion is that he can successfully treat the behaviors of a variety of systems *without* having a constructive account in hand, and this is important because very often we have no such account, it's epistemically inaccessible.

So all Boscovich can do is wave his hands towards what he claims is going on constructively, in terms of particles and forces, but he can't actually recover d'Alembert's results. Pretending otherwise obscures the epistemic achievement of constrained motion mechanics, and the beautiful and powerful way in which it brackets dynamics into kinematics.

One final point: as mechanics and physics develop into the early 19th century, one of the things that drives theorizing forward is a lack of ontic unity, the freedom to theorize without prior commitment to a single, unified ontology.

This is not a criticism of Boscovich. My point is that by viewing his book as a contribution to philosophical mechanics, we can get a new perspective on his theory; on how hard the task was that he set himself, and where the strengths and limitations of it lie.

[slide 30] So to conclude. It's simply not true that 18th century physics is "normal science" or that 18th century mechanics has settled foundations and is philosophically uninteresting.
But, if we want to say more, then we need a new framework for reading and analyzing the books and materials of the time. The framework of "philosophical mechanics" is built on evidence from those books and materials themselves, and it enables us to do all sorts of new and interesting things.

[slide 30a] If with think of the 18th century as a Golden Age of philosophical mechanics, then:

[slide 30b] Du Châtelet's *Foundations*, d'Alembert's *Treatise*, and Boscovich's *Theory* all find a natural place in the story, and in dialogue with one another

[slide 30c] Works whose philosophical importance is largely invisible under old framings (such as Du Châtelet's *Foundations* and d'Alembert's *Treatise*) become highly visible and prominent

[slide 30d] New questions arise about more familiar works (such as about Boscovich's *Theory* and its relation to constrained mechanics)

[slide 31]

There are obstacles to developing this picture, such as the challenges of working on understudied materials, including translation and building scholarly infrastructure for working on a text. But good progress is being made.

There are also important methodological decisions as we see the locus of our philosophical questions move from philosophers who treat them explicitly to the new "physicists" who often do not

But this shift is something that was a challenge *in* the 18th century too, and was very much a part of the Golden Age of philosophical mechanics

So I recommend this framework to you.

Finally, more generally, my talk is intended to support something we all care about: the importance of telling and re-telling the history of the philosophy of science, keeping it alive over and again with every new generation of students and scholars. Different tellings make different things important and unimportant. So if you have a book or some letters or some other materials that you think are interesting and important, but they haven't been worked on and they don't fit in the canonical framework for what counts as important, don't be afraid to reframe the history. That's a part of what we do.

Thank you.