Title
Spacetime, Symmetry, and Scientific Practice: Essays in History and Philosophy of Physics

ISBN
9798191654324

Author
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Publication Date
2026-09-23

Peer reviewed|Thesis/dissertation

UNIVERSITY OF CALIFORNIA SAN DIEGO

Spacetime, Symmetry, and Scientific Practice: Essays in History and Philosophy of Physics

A dissertation submitted in partial satisfaction of the
requirements for the degree Doctor of Philosophy

in

Philosophy

by

Shelly Yiran Shi

Committee in charge:

Professor Craig Callender, Chair
Professor Nancy Cartwright
Professor Eddy Keming Chen
Professor Raphael Flauger
Professor Dennis Lehmkuhl
Professor Kerry McKenzie

2026

The Dissertation of Shelly Yiran Shi is approved, and it is acceptable in quality and form for publication on microfilm and electronically.

University of California San Diego

2026

To my parents

致双亲

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ACKNOWLEDGEMENTS

I would like to thank Craig Callender for his support as my advisor and committee chair. Throughout graduate school, I often worried that people were simply too kind to tell me when a paper was not working. Craig perfected the art of delivering criticism through jokes, and I learned early on to take his jokes seriously. I also gradually learned that “Craig is always right,” even if it sometimes takes a while to realize it. He nudged me into writing something about C. S. Wu during the Covid year; I only took up that advice four years later, but every day I find the project more worth pursuing. Without his jokes, nudges, and encouragement, most of the chapters in this dissertation would not have been possible. Like many graduate students, I have spent Christmases and Thanksgivings at Craig’s house, a holiday refuge for international students. He treats his graduate students as an extension of his family, and I will always be grateful for his generosity. His work inspires me because it is so clearly driven by passion, curiosity, and intellectual joy; just as importantly, he has always encouraged me to work on the questions I care about most. That is perhaps the most important lesson I have learned in graduate school.

I would like to thank Nancy Cartwright for her generous feedback on many chapters of this dissertation. Nancy always spotted exactly the points I wanted to sweep under the carpet and made me clean them up. Her dedication to both her work and her students is humbling to all of us. I first encountered Nancy’s work as an undergraduate at LSE, where I came to think of her as one of the greatest living philosophers of science. Little did I know then that I would later have the privilege of knowing her both professionally and personally. I have also been lucky enough to join several of Nancy’s annual philosophy of science camping trips, which speak to the remarkable sense of community she has built in the field.

I would like to thank Eddy Keming Chen for many helpful discussions, spirited debates about the laws of nature and Humeanism, and generous feedback on many chapters of this dissertation. I often went into his office thinking he was wrong and came out convinced otherwise. Eddy has always been remarkably approachable whenever I have questions, and I learned a great deal from our conversations. Eddy and Linda also hosted me when I first arrived in San Diego, and like many other Chinese graduate students, I spent Chinese New Year at their house in the very best of company, including their cats.

I would like to thank Raphael Flauger, whose General Relativity I, II, and III courses I took almost religiously. It is fair to say that much of this dissertation would not have been possible without Raphael's classes. He welcomed my many questions, including those that might have struck others as annoying. One such question, which I must have asked at least five times in class, concerned energy conservation in general relativity; Chapter 2 is, in many ways, dedicated to that question. Chapter 3 would also not have been possible without Raphael spending ten classes on post-Newtonian expansions. I learned enormously from Raphael's Weinbergian approach, and from the patience and precision with which he explained things. I only wish I could stay at UCSD longer and have the chance to take QFT from him.

I would like to thank Dennis Lehmkuhl for his extensive and careful feedback on many chapters of this dissertation. I have learned enormously from his comments, especially from those passionate "No!"s when I had not been careful enough with interpreting historical sources. His standards for working with, referencing, and translating historical materials are ones I still aspire to meet more fully. Although we defend sharply opposed views in print—he supersubstantivalism, and I relationism—there is no one I would have been happier to have as my external committee member. This is not only because he is one of the leading Einstein scholars working today, but also because his work has long been a source of intellectual excitement for me. During the usual graduate-school existential crises, when I was trying to figure out what kind of philosopher—or HPS person—I aspired to be, I often thought of Dennis's work. When I visited Oxford, he introduced me to other philosophers and treated me as a colleague, which, coming from him, meant a great deal to me.

I would like to thank Kerry McKenzie for her helpful comments on many drafts of my papers. I still remember receiving comments from her in my pigeonhole: a full page of overview remarks, followed by detailed comments throughout in different colors of pen. I was touched by the care she brought to my work. When I decided to pursue a book project on C. S. Wu,

she immediately connected me with experienced book authors. Kerry was also the first woman philosopher of physics I encountered. I first saw her give a talk at Columbia, on the eighth floor of Pupin Hall, arriving with a stroller (Murdo was in it, as I later learned), which she set aside before giving one of the best talks at the conference. I remember thinking: what a fierce woman. Her candor and intellectual fearlessness have remained inspiring to me.

Besides my committee, I would also like to thank many friends and colleagues. I am especially grateful to Guanhao Sun, from whom I learned a great deal of physics, both at Columbia and at UC San Diego. What a coincidence that he moved to San Diego and became Raphael's postdoc just as I decided to learn general relativity. In the years that followed, he helped me decipher much of what Raphael said. I would also like to thank Eugene Chua and Bosco Garcia for many wonderful conversations. I was lucky to be able to say that my officemates were also my closest friends. I am grateful to James Read, who gave me helpful suggestions on Chapter 3 during my time in Oxford. I would also like to thank the many friends who offered me shelter (and food) during conferences and archival trips in different parts of the world, making those travels affordable and delightful. They include Yifan Chen, Kang Liu, Yushu Liu, Yichen Lou, Yichen Luo, Yuwei Zhang, and Mei Zi.

Last but not least, I would like to thank my partner, Faron Ray, and my parents for their extraordinary support. Faron and I have laughed our way through much of graduate school together, and I cannot imagine these years without him. For a moral philosopher, he has also proofread more than enough of my philosophy of physics papers. My parents never really understood what I was doing, but my mum could always tell when a paper was going badly by judging from the amount of hair I had shed. She constantly told me to quit the PhD and live an easier life. Much as I know my mum is always right, on this point I rebelled. When I began working on Chapter 4 on C. S. Wu, my parents started spending hours on the phone asking me about it. That was when I realized how much they had wanted to understand what I was doing all along. I am glad that, in the end, I wrote at least one chapter through which they might make sense of some part of my work.

Chapter 1, in part, is based on the material as it appears in "Nothing Matters" published in *Philosophy of Science*, Volume 92, Issue 5, 2025. The dissertation author was the sole author of this paper.

Chapter 2, in part, is currently being prepared for submission for publication. The dissertation author was the sole author of this paper.

Chapter 3 is original dissertation material. Parts of this chapter may be developed into a collaborative paper in the future. The dissertation author is the sole author of the chapter as it appears here.

Chapter 4, in part, is currently being prepared for submission for publication. The dissertation author was the sole author of this paper.

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Shi, Shelly Yiran. “Are Symmetry Principles Meta-laws?.” *Synthese* 205, no. 6 (2025): 223. https://doi.org/10.1007/s11229-025-05064-w

Shi, Shelly Yiran. “Nothing Matters.” *Philosophy of Science* 92, no. 5 (2025): 1279–90. https://doi.org/10.1017/psa.2025.10144

ABSTRACT OF THE DISSERTATION

Spacetime, Symmetry, and Scientific Practice: Essays in History and Philosophy of Physics

by

Shelly Yiran Shi

Doctor of Philosophy in Philosophy

University of California San Diego, 2026

Professor Craig Callender, Chair

This dissertation examines philosophical problems in general relativity and historical questions about the production of physical knowledge. Part I focuses on three foundational problems in general relativity. Chapter 1 revisits the metaphysics of spacetime through Mach's principle and the vacuum challenge, arguing that the existence of gravitational degrees of freedom not fixed by the stress–energy tensor need not straightforwardly support substantivalism. Chapter 2 distinguishes energy conservation from energy balance in general relativity, arguing that even skeptics about gravitational energy should retain balance relations as physically indispensable constraints against gravitational perpetual motion machines. Chapter 3 argues that general relativity often achieves empirical success not through a single unified model applied uniformly,

but through a plurality of approximation schemes, boundary conditions, and matching procedures adapted to different regimes. Part II turns to the history and philosophy of twentieth-century physics through an intellectual biography of Chien-Shiung Wu. It situates Wu's work on parity violation within the broader histories of war, migration, racial exclusion, gender hierarchy, Cold War politics, and institutional reform. Taken together, the chapters show how our understanding of physical theories depends on their formal structure, their use in concrete modeling practices, and the historical and institutional conditions under which scientific knowledge is produced.

Introduction

This dissertation grows out of two connected research interests: philosophy of physics, especially foundational questions in general relativity, and the history and philosophy of twentieth-century physics. Part I concerns the conceptual and metaphysical implications of general relativity, especially questions about Mach's principle, energy conservation, and approximation methods. Part II concerns the historical, institutional, and political conditions under which physical knowledge is produced. In both parts, I am interested in how physics actually works: how abstract theory is applied through approximations and models, and how the contexts of research shape scientific practice.

Part I includes three chapters on foundational problems in general relativity. These chapters examine the relations between matter and spacetime structure (Chapter 1), between energy conservation and energy balance (Chapter 2), and between formal theory and modeling practice (Chapter 3). Much of the philosophical literature on general relativity proceeds through careful attention to differential geometry and the formal structure of gravitational theories. This work has been enormously fruitful, and my own work is indebted to it. But the approach I take in these chapters is somewhat different. I am often more interested in how the formalism is used in actual physical contexts: through approximations, idealizations, coordinate choices, and modeling practices.

The first three chapters also became increasingly historically informed as they developed. One of the recurring experiences of writing them was discovering, often after completing a first or second draft, that Einstein, Bondi, Fock, or others had already expressed closely related ideas, sometimes in strikingly similar terms. These discoveries were initially discouraging. There were moments when I thought: of course, if the idea is any good, someone much smarter must already have said it; and if no one has said it, perhaps it is just wrong. At times, it seemed nearly impossible to develop a good argument that was "purely original" in the conceptual space of the philosophical literature. But these discoveries were also a source of serendipitous joy. There is something wonderful about finding, buried in the writings of physicists from a century or half-century ago, a formulation or concern that resonates so closely with an argument one had arrived at independently. Over time, I came to see these discoveries not as reasons to give up the project because someone had already voiced a similar idea, but as signs that I was tracking something interesting about the theory itself and about the ways physicists have tried to understand it.

For that reason, the historical material in Part I is incorporated into the development of the arguments, both to situate the debates and to clarify the philosophical lessons at stake. These episodes show that the problems I discuss arose within the development and application of the theory itself. Although I did not begin these chapters with a fully formed history-and-philosophy-of-science methodology, the process of writing them gradually changed my approach. I came to see that a more historically informed philosophy of physics can make the philosophical arguments more precise, and often more interesting.

Part II turns to an intellectual biography of Chien-Shiung Wu, whose 1957 experiment provided the decisive experimental demonstration of parity violation, an important episode in the history of modern physics. By focusing on Wu's life and work, this part asks how scientific practice is shaped by migration, war, exclusion, and institutional power, and how those conditions enter into the making of modern physics. As part of the requirements of the Science Studies Program, I completed an archival trip to the Chien-Shiung Wu Papers at Columbia's Rare Book and Manuscript Library in winter 2024. In spring 2025, I visited the archives of the Bancroft Library at UC Berkeley and conducted interviews with several of Wu's former students and colleagues. In summer 2025, I carried out research in Taicang, Wu's hometown, and in Nanjing, where she attended college, on her early life and education. With Grants-in-Aid support from the

American Institute of Physics, the research in Part II will also become part of a broader book project on the twentieth-century Chinese scientific diaspora.

The rest of this introduction provides a brief explanation of the motivations for the chapters, followed by a chapter-by-chapter summary.

Chapter 1 "Nothing Matters" focuses on the metaphysics of spacetime. The debate over the nature of spacetime is often framed as a dispute between substantivalism and relationism. Very roughly, substantivalism holds that spacetime exists as a fundamental entity or substance in addition to the material bodies, whereas relationism holds that only matter is fundamental, with spatiotemporal structure derived from the relational distribution of matter. The core ideas of this debate are inherited from early modern philosophy, and this has led some to wonder whether the dispute survives in any substantive form in the context of general relativity. In particular, Robert Rynasiewicz suggested that it was a mistake to impose seventeenth-century metaphysical categories on a theory that has long since outgrown them (Rynasiewicz, 1996, 2000). There is something right with this worry. In the context of modern field theory, disputes about whether to call the metric a "field," an "ether," a property of spacetime, or spacetime itself can easily collapse into verbal or terminological maneuvering. Yet I think that the conclusion that nothing substantive remains at stake is too quick. The real issue is not what we choose to call the metric, but whether and in what sense the metric can be understood as *determined* by matter. That is where the metaphysical pressure lies.

This question has a particularly clear historical source in Einstein's engagement with Mach's principle. From special relativity to general relativity, Einstein was deeply concerned with eliminating what he regarded as the substantivalist residue in earlier physics. His hope was that inertia and spacetime structure could ultimately be explained by the material distribution of the universe. That ambition informed his long and complicated engagement with Machian ideas, especially in the period surrounding the formulation of general relativity and his exchanges with de Sitter. Later, however, Einstein distanced himself from Mach's principle, remarking in a 1954 letter to Felix Pirani that one should no longer speak of it, since the stress–energy tensor itself *depends* on the metric field. In this paper, I choose to leave aside the *dependence* question, which has been addressed by Lehmkuhl (2011), and instead focus on the *determination* question, since these are two separate metaphysical relations. The issue remains whether the metric in general relativity is underdetermined by matter in a way that supports substantivalism, or whether a more sophisticated relationist reading remains available. Chapter 1 reopens that question.

During a discussion session at the 1993 Tübingen conference "Mach's Principle: From Newton's Bucket to Quantum Gravity," Julian Barbour posed an important question about *what*, in general relativity, is supposed to *determine* the inertial structure: "In the context of general relativity, must the local inertial frames of reference be determined completely by the energy-momentum tensor of matter in the narrow sense, or can the gravitational degrees of freedom themselves contribute to the determination of the local frames of reference?" (Barbour and Pfister, 1995, p. 92) Hermann Bondi immediately responded that the question has an undisputed and "obvious" answer: Since two Cauchy surfaces with the same stress-energy tensor but different gravitational wave situations can develop differently, gravitational degrees of freedom must play a role. Bondi then added, "The hidden agenda which has been brought out is: Is a theory in which the gravitational degrees of freedom play a role thereby non-Machian?" (Barbour and Pfister, 1995, p. 94)

I discovered Bondi's exchange with Barbour only after I had finished the first few drafts of this chapter. His question struck me because it captured, almost exactly, the question I had been trying to answer. I argue that the role of gravitational degrees of freedom in general relativity does not by itself disqualify the theory from being Machian, at least in one interesting and defensible sense of that term.

The chapter begins with one especially influential formulation of Mach's principle: the requirement that the inertial structure, i.e. the metric field, be fully determined, up to gauge, by the stress–energy tensor. I call this Mach's Principle-T. General relativity does not satisfy this principle. The Einstein field equations do not uniquely fix the metric once the stress–energy tensor is given, even after one sets aside the issue with gauge freedom. This is vividly illustrated by the existence of distinct vacuum solutions: solutions with vanishing stress–energy tensor but different metrical structures. From the point of view of a relationist who hopes that spacetime geometry is in some sense sourced by matter, this looks like bad news. Chapter 1 reformulates this familiar underdetermination problem as what I call the vacuum challenge.

The advantage of the vacuum challenge is that it sharpens what is really at issue. The problem is that in general relativity there appear to be genuine gravitational degrees of freedom that remain even where the stress–energy tensor vanishes. The chapter shows that these degrees of freedom are associated with the Weyl tensor, which captures the "free" gravitational field in a way that the Ricci tensor does not. The Ricci curvature is directly related to matter through the Einstein field equations, but the Weyl curvature is not fixed in the same way by local stress–energy. This is what allows different vacuum solutions to exist. The metaphysical question then becomes: how should those degrees of freedom be interpreted? Do they force us toward substantivalism, or can a relationist say something more?

My answer is that the existence of Weyl curvature in vacuum does not by itself vindicate substantivalism. There is nothing intrinsically substantival about the Weyl tensor. What it represents, I argue, is the possibility that something outside a system exerts gravitational influence on that system. In that respect, the relevant degrees of freedom need not be understood as evidence of substantivalism; they may instead reflect the omitted sources or modeling assumptions.

The chapter develops this proposal through two case studies. The first is Schwarzschild spacetime, historically central to Einstein's struggle with boundary conditions. In this case, I argue that boundary conditions can be viewed as stand-ins for a source, either a distant star or a singularity. The second and more difficult case is the Ozsváth–Schücking metric, introduced as an "anti-Mach" spacetime. This solution describes a pp-wave spacetime that contains only gravitational radiation. Here I suggest two available responses. First, I show that the dimensional parameters of the model can be viewed as echoes of forgotten matter. Second, I show that one can pose a dilemma based on the Ehlers–Kundt conjecture and a theorem by Penrose to argue that the spacetime is either incomplete or a mere idealization. If one of these responses is successful, then even the most hostile vacuum solution can be made compatible with relationism.

The chapter accepts that general relativity does not satisfy Mach's Principle-T. What it denies is that this failure straightforwardly settles the metaphysics of spacetime in substantivalist terms. The vacuum challenge arises because the metric contains gravitational degrees of freedom not captured by the stress–energy tensor alone. But once those degrees of freedom are understood as reflecting external influence via idealization, the inference to substantival spacetime is blocked. In that sense, Chapter 1 attempts not to prove relationism, but to reopen its viability within general relativity by showing that the underdetermination of metric by matter need not be read in substantivalist terms.

Chapter 2 "Why Not Perpetual Motion Machines?" turns to the status of conservation in general relativity. In general relativity, spacetime is dynamical rather than fixed, generic spacetimes lack the symmetries associated with Noether's First Theorem type of conservation laws, and the isolation conditions presupposed by ordinary conservation reasoning are difficult to satisfy. These features make energy conservation in general relativity technically and conceptually difficult.

The chapter begins from the observation that the standard background conditions for conservation do not carry over smoothly from classical mechanics to general relativity. The central claim of the chapter is that much of the debate on energy in general relativity has been obscured by a failure to distinguish energy conservation from energy balance. These notions are often run together, but they should be kept apart. Energy conservation applies only to closed systems and presupposes an isolation condition. Energy balance is a weaker and more general notion: it concerns open systems and relates changes in the energy of one subsystem to balancing terms, such as flux through the boundary or exchange with other interacting subsystems. In this respect, conservation appears as a special case of balance that is obtained only when an appropriate isolation condition is met.

In this chapter, I try to make the distinction between energy balance and energy conservation salient, for instance by drawing on toy examples from classical mechanics. A helpful conversation with Craig Callender pressed me to ask whether this distinction really carves nature at its joints. During a conversation in Oxford, James Read asked me whether anyone else had drawn the distinction, and I vaguely recalled that Einstein might have used the term "Energiebilanz" in some relevant contexts. After some digging, I was delighted to find historical sources that helped support this thought. The relevant source is Einstein's *Manuscript on Special Relativity*, which, as Dennis Lehmkuhl informed me, remained unpublished. Even so, its examples and parallels with electromagnetism make it especially useful for my purposes here.

A substantial part of the chapter is devoted to clarifying the conditions under which different forms of conservation or balance can still be recovered. I argue that matter energy can be treated as locally conserved when one works in a suitably local region. On that scale, spacetime may be treated as approximately Minkowskian, and familiar special-relativistic conservation reasoning applies. Global conservation can also be recovered in special cases where matter is independently isolated, as in stationary spacetimes admitting a timelike Killing vector field. Beyond that, one may still obtain a balance statement for the combined matter-plus-gravitational system when the system is treated as collectively isolated, typically under asymptotic flatness.

The clarification is then brought to bear on one of the most lasting debates in the foundations of general relativity: whether gravitational waves carry energy. The chapter reconstructs the familiar opposition between gravitational-energy realists and eliminativists. Realists take gravitational waves to carry genuine energy and often appeal to the Bondi–Feynman sticky-bead argument: if passing gravitational waves can heat sticky beads through friction, then they must be doing work, and something that does work seems to carry energy. Eliminativists, by contrast, emphasize the notorious problems with gravitational energy: it is typically represented by pseudotensors that are non-covariant, non-unique, and coordinate-dependent. On this view, there is no need to posit any genuine gravitational energy at all; one can instead say simply that energy conservation fails in general relativity. Chapter 2 does not attempt to settle that realism dispute. Instead, it shifts attention to a different question: even if one remains skeptical of gravitational energy, should one still retain an energy-balance story?

My answer is yes. The core argument of the chapter is a reductio directed at the eliminativist rejection of energy balance. If one denies not only gravitational energy realism but also any genuine balance relation between matter and gravitation, then one loses an essential physical constraint: there would be no principled upper bound on the amount of energy that could be extracted from a gravitational system. That would open the door to what the chapter calls a gravitational perpetual motion machine. In asymptotically flat settings, the asymptotic energy-balance relation between ADM energy, Bondi energy, and radiated energy implies that the total energy emitted through gravitational radiation cannot exceed the initial total energy of the system. Energy balance therefore is an important physical constraint: it rules out pathological scenarios in which gravitational systems yield unlimited work.

Much of the literature focuses on whether gravitational energy is "real," and thus on whether the usual objections to pseudotensors undermine gravitational-energy realism. Chapter 2 instead argues that even a skeptic about gravitational energy can reject the inference from "gravitational energy is not real" to "there is no energy balance between matter and gravitation." I explore this possibility in detail. One option is to treat balance relations as tracking the maximal work gravitational waves can do, without reifying localized gravitational energy.

The chapter also emphasizes the role of approximation in making sense of energy in general relativity. Just as Chapter 1 argued that the metaphysics of spacetime cannot be read straight off exact vacuum solutions without attending to modeling practices, Chapter 2 argues that conservation and balance in general relativity must often be understood through approximation schemes. The broader lesson is that the notion of energy in general relativity is often approximate, context-sensitive, and scale-dependent. But that contextuality is not a defect. It is a consequence of the fact that general relativity describes an interaction between matter and spacetime that makes strict isolation exceptionally hard to achieve.

Chapter 3 "Patchwork Gravitation" turns to the role of approximation methods in general relativity. The title has its own small history. Craig Callender first suggested "Patchwork General Relativity" as a title for the project. I later decided to change it to "Patchwork Gravitation," partly in homage to Vladimir Fock, who preferred to call Einstein's theory the theory of gravitation rather than the general theory of relativity. Fock thought that making general covariance the central physical lesson of the theory was a mistake: general covariance, by itself, was a formal feature of the equations, not a physical constraint. What mattered, for him, was how a concrete gravitational problem is physically specified: through the character of the system, the boundary conditions, and the coordinates adapted to that problem.

Fock is an especially interesting figure in the history of general relativity. He was one of the major Soviet interpreters of Einstein's theory, but his work was shaped by an intellectual context in which relativity, Machianism, idealism, and dialectical materialism were politically and philosophically charged. Since Fock argued against Mach, with Lenin's criticism of Mach loomed in the background, I assumed that I would disagree with much of what he said. To my surprise, I came to find many of his arguments not only interesting but compelling. His defense of harmonic coordinates, his treatment of energy conservation, and even his resistance to Einstein's Machian ambitions all turned out to resonate with arguments I had developed in the preceding chapters. One aim of this chapter, then, is to give Fock a more visible place in philosophical discussions of general relativity.

The chapter develops this historical point into a broader methodological argument. General relativity is often admired for its conceptual unity and geometrical elegance. In its standard formulation, spacetime is represented by a smooth manifold equipped with a Lorentzian metric, matter is represented by tensor fields, and the dynamics of matter and geometry are governed by Einstein's field equations, which Penrose (2002, p.50) describes as "of unprecedented elegance and geometric simplicity." Yet the actual application of the theory in concrete physical contexts is often far less monolithic and more "fragmented". Exact solutions are available only in special cases, often under strong symmetry assumptions. In many physically important situations, including binary systems emitting gravitational radiation, the theory makes empirical contact through approximation schemes, idealizations, boundary conditions, and matching procedures.

The central claim of the chapter is that approximation methods in general relativity reveal something important about how the theory is applied. Different regimes of the same physical system often require different models, each valid under different assumptions and each bringing out different aspects of the underlying physics. This is especially clear in the relation between post-Newtonian and post-Minkowskian approximations. Post-Newtonian methods are well suited to slowly moving, weak-field systems, where relativistic corrections can be added order by order to a Newtonian baseline. Post-Minkowskian methods, by contrast, begin with a Minkowski background and treat gravity as a perturbation propagating through an approximately flat spacetime. The former are especially useful in the near zone of a bounded system; the latter are especially useful in the far zone, where radiative effects become central.

The chapter shows that Fock's approximation strategy was prescient. He treated the near-zone and wave-zone problems separately, imposed physically motivated boundary conditions on the radiative solution, and then asked whether the two descriptions agreed in an overlapping region. Later methods, such as the Blanchet–Damour formalism and the DIRE formalism, developed much more sophisticated versions of this same basic strategy: use post-Newtonian expansions in the near zone, post-Minkowskian expansions in the radiation zones, and matching techniques to connect the motion of the source with the outgoing gravitational field. Fock did not have the later formal machinery, but he saw something important about the structure of the problem. No single approximation scheme captures equally well both the near-source dynamics and the propagation of radiation to infinity.

The chapter further argues that the success of such pluralistic methods reflects the mixed mathematical character of Einstein's field equations. In the modern initial-value formulation, the equations contain both constraint equations, which restrict admissible initial data, and evolution equations, which govern how that data develops. Near a slowly moving source, general relativity can often be treated in a quasi-elliptic or Poisson-like way: retardation effects are suppressed, and the field is approximated by a sequence of Poisson-like gravitational potentials. Far from the source, however, the hyperbolic or wave-like character of the theory becomes essential. The finite propagation time of gravitational effects can no longer be ignored, and radiative degrees of freedom must be treated as genuinely propagating.

The philosophical moral of the chapter is Cartwrightian. Nancy Cartwright argues that scientific theories often work not by supplying a single model that applies uniformly across all circumstances, but by providing a plurality of models tailored to different domains. General relativity might seem, at first, to be an unlikely place to find such a lesson, since it is so often presented as a formally unified and geometrically elegant theory. But perhaps precisely because of its mathematical and physical richness, general relativity often achieves empirical success through a patchwork of locally or regionally valid descriptions. The post-Newtonian and post-Minkowskian schemes provide a clear example. Each is a controlled approximation method; each is valid only in a limited regime; and neither, by itself, captures the whole phenomenological story.

Chapter 3 therefore makes explicit a methodological theme running through Part I of the dissertation. Chapter 1 argues that the metaphysics of spacetime cannot be read directly from exact vacuum solutions without attending to how those solutions function in modeling practices. Chapter 2 argues that conservation and balance in general relativity must often be understood through approximation, scale, and boundary conditions. Chapter 3 extends this lesson to the practice of approximation itself. General relativity is not less successful because it is applied through a patchwork of models. Its success depends, in many cases, on knowing which patch to use, where it applies, and how it is to be stitched together with others.

Chapter 4 "Breaking Parity Against Disparity" is a more extensive chapter and forms Part II of the dissertation. Whereas Part I focuses on philosophical issues in general relativity, Part II turns to the history of physics by tracing the intellectual life and scientific work of Chien-Shiung Wu, and by situating her career within the broader history of twentieth-century physics.

I initially began working on this project because I was interested in the history of parity symmetry. Unlike continuous symmetries, parity is a discrete symmetry that does not yield a Noetherian conserved quantity in classical mechanics. In quantum mechanics, however, parity can be represented by an operator with associated eigenvalues, and the language of parity symmetry and parity conservation often becomes interchangeable. For much of the twentieth century, parity was widely assumed to be an exact symmetry of nature. That assumption was overturned in the 1950s by experiments showing parity violation in weak interactions, of which Wu's cobalt-60 experiment became the most famous.

The chapter centers on an intellectual biography of Chien-Shiung Wu and situates her scientific life within a broader social and political history. Wu is already a major figure in the history of modern physics: the first woman on the Princeton physics faculty, the first woman to receive tenure in physics at Columbia, the only Chinese scientist to participate in the Manhattan Project, the first woman president of the American Physical Society, and a central experimental figure in the discovery of parity violation. But the broader conditions under which she achieved these milestones remain underexplored. My aim is to place her career within the intersecting histories of war, migration, exclusion, Cold War politics, and gendered inequality in twentieth-century physics.

One of the chapter's central claims is that Wu's scientific career should be understood within a broader historical context. Born in 1912, the year of the Republic of China's founding, she was shaped by the intellectual currents of the New Culture and May Fourth movements, with their commitment to "science" and "democracy". She came to the United States to pursue physics in a period when Chinese intellectuals often saw American science as part of a broader modernist aspiration. But that aspiration collided with the realities of racial exclusion and gender hierarchy in the United States. Wu's career unfolded against the background of anti-Asian immigration law, wartime labor shortages, McCarthy-era suspicions, and the geopolitical fractures of the Cold War. At the same time, her ties to China were marked by prolonged separation and personal loss, including the deaths of close family members during the Great Leap Forward and Cultural Revolution. The chapter argues that these conditions shaped the opportunities available to her, the forms of recognition she received, and the burdens under which she worked.

One contribution of the chapter is to reconstruct Wu's early career in a way that makes visible the structural conditions under which she entered American physics. I show how wartime research functioned as a turning point. It allowed Wu to break through the glass ceiling in two connected ways: first, by enabling her to return to research rather than being confined to teaching, a privilege disproportionately reserved for male physicists; and second, by allowing her repeatedly to demonstrate her "loyalty" as a Chinese immigrant in wartime and postwar America.

Its second contribution is to retell the history of the parity-violation experiment itself. Rather than presenting the Wu experiment as a simple textbook case of a single decisive falsification, the chapter reconstructs the micropolitics of the episode: the coordination between theorists and experimentalists, the race to publication, the role of institutional prestige, the interactions among the Columbia, National Bureau of Standards (NBS), and Chicago groups, and the retrospective construction of the experiment as a uniquely "crucial" event. The familiar Popperian image of one decisive experiment overthrowing an established principle turns out to be too neat. What actually happened was more collaborative, more competitive, and more contingent. Multiple experiments converged on the same conclusion almost simultaneously, and the status of Wu's experiment as the decisive experiment was in part the product of later institutional and narrative consolidation. This lets the chapter speak not only to the history of parity violation, but also to broader science-studies questions about experiment, credit, replication, and the making of canonical episodes in physics.

A third contribution of the chapter is to recover Wu's overlooked activism. Wu's scientific achievements are well known; her sustained institutional work on behalf of women and minorities is not. Drawing on archival materials from Columbia and the American Physical Society (APS), I show that in the 1970s Wu played an important role in efforts to address salary and pension inequalities, childcare problems, and affirmative action at Columbia. She also used her presidency of the APS to support initiatives concerning women, minorities, scientific freedom, and research funding. These activities reveal forms of scientific labor that are often overlooked: committee work, institutional reform, and mentorship. The chapter argues that Wu's legacy extends beyond the laboratory and that the history of physics should take such forms of labor more seriously.

This also leads to a more explicitly philosophical question: what, if anything, does Wu's life reveal about feminist epistemology in physics? Her case is especially interesting because it resists simple moralization. Wu's achievements were made possible by unusual technical brilliance, extraordinary persistence, and support from some powerful colleagues; but they were also shaped by forms of exclusion and disparity that altered the conditions of her work. Her later activism, in turn, shows an acute awareness of how institutional arrangements affect who gets to do science and under what conditions. In this way, the chapter uses Wu's life to open a philosophical question about standpoint epistemology, exclusion, and the production of knowledge in physics.

Part I

Chapter 1

Nothing Matters

1.1 Introduction

Suppose that you're in a room with a lightbulb. A curtain is drawn between you and the lightbulb. The lightbulb cannot be seen, but some light still filters through the curtain into the room. You and your friend Feld have a disagreement. Feld thinks that there can be free (i.e., sourceless) fields, whereas you hold that all electromagnetic (EM) radiation is sourced. Feld claims that the room you're in is evidence that she is correct, for one can model just the room with the curtain as a boundary condition and the representation of that scenario corresponds to a solution of the relevant laws of nature. That argumentative move feels too cheap, you say. All Feld did was replace the source with an incomplete model possessing a special boundary condition: the state of the curtain. That state carries upon it an echo or trace of the real source. The free solution exists as an artifact of modeling, but in the real world radiation may be sourced, or so you think.

In the debates between substantivalists and relationists in general relativity (GR), the existence of distinct vacuum solutions to the Einstein field equations (EFE) is often taken to present a quick and serious challenge to relationism. For example, Minkowski and Schwarzschild spacetimes have different metric fields but are both vacuum solutions with the same matter content. Their existence suggests that the metric field is underdetermined by the matter content, which would conflict with versions of relationism that claim that the metric is always "sourced" by matter. In what follows I will argue that one can respond to this famous threat to relationism in a similar way to how one may respond to the above challenge to the claim that all radiation is sourced. The boundary condition contains suspicious "echoes" of forgotten material sources. This response will not prove that relationism is true any more than the earlier move proved that all radiation is sourced. But it will, I think, deflate one worry about relationism.

To see the general idea, note that the Schwarzschild solution is Ricci-flat but not Riemann-flat. Minkowski is both. What makes Schwarzschild not Riemann-flat is that it has non-vanishing Weyl curvature. The Weyl curvature tensor can be thought to represent the "free" gravitational degrees of freedom whereas Ricci represents the "source" degrees of freedom. Yet, just like in our imagined dialogue above, one might look with suspicion upon Schwarzschild. The Schwarzschild solution is an exact solution of the EFE regarding the outside of a spherical mass, often used to model static objects. All we've done is draw a curtain around the matter to make it a vacuum, yet we do this for the sake of modeling. The boundary conditions contain the trace of ignored material sources. Relationists can argue that the underdetermination arises from this modeling.

The paper proceeds as follows. Section 1.2 makes the challenge posed to relationism from vacuum solutions precise. I call this the vacuum challenge. Section 1.3 then moves to provide a relationist response to the challenge. Section 1.3.1 begins with the Schwarzschild solution. In this case, I argue that boundary conditions can be viewed as stand-ins for a source, either a distant star or a singularity. Section 1.3.2 then moves to what is perhaps the most hostile environment for my view, the Ozsváth-Schücking metric. This solution describes a pp-wave spacetime containing only gravitational radiation. Here I suggest two available responses. First, I show that the dimensional parameters of the model can be viewed as echoes of forgotten matter. Second, I show that one can pose a dilemma based on the Ehlers-Kundt conjecture and a theorem by Penrose to argue that the spacetime is either incomplete or a mere idealization. If one of these responses is successful, then even the most hostile vacuum solution can be made compatible with relationism.

1.2 Does $T_{\mu\nu}$ fully determine $g_{\mu\nu}$?

Spacetime curvature and matter fields in GR are deeply connected yet utterly distinct entities. Einstein analogized the left-hand side of his field equations to fine marble, while considering the right-hand side as low-grade wood.[1] The original non-augmented field equations (without the cosmological constant) read,

$$R_{\mu\nu} - \frac{1}{2}g_{\mu\nu}R = \kappa T_{\mu\nu}. \tag{1.1}$$

John Wheeler famously captures the essence of GR with the succinct statement: "matter tells spacetime how to curve, and spacetime tells matter how to move."[2] The metaphysical debate revolves around two conflicting viewpoints concerning the nature of spacetime. Substantivalism claims that spacetime is fundamental, existing as a distinct substance. Relationism posits that only matter is fundamental, with properties of spacetime (such as the metric) being derived from the relational distribution of matter.[3] The debate between substantivalists and relationists traces back to the seventeenth century in the renowned Leibniz-Clarke correspondence, and found a new voice in Mach's (1893) "fixed stars" response. However, it has become significantly more convoluted in response to modern theories of gravity. Some suggest we should refrain from imposing the categories of seventeenth-century metaphysics onto a theory that has outgrown them,[4] while others have proposed alternative formulations of GR to revive Mach's principle.[5]

In this section, I revisit and clarify the problem of matter-metric determination in GR.

[1] In contrast to the conventional interpretation that associates the term "marble" with the smooth and pristine nature of the geometry, and "wood" with the rough and knotty characteristics of the matter (or the stress-energy tensor), Lehmkuhl (2019) suggests that Einstein's reasoning was not primarily concerned with geometry itself. Instead, it pertains to the effectiveness of the stress-energy tensor in providing an accurate representation, including quantum features, of matter. In GR, the stress-energy tensor is "a place-holder for a theory of matter not yet delivered." (Lehmkuhl, 2019, p. 180)

[2] See Misner et al. (1973a, p. 5) and Wheeler and Ford (2000, p. 235).

[3] See Sklar (1977) and Lehmkuhl (2018) for a third alternative – super-substantivalism, which views spacetime as the *sole* fundamental substance, while matter as a "consequence" or derived property from the structure of spacetime.

[4] See Rynasiewicz (1996, 2000).

[5] See Sciama (1953); Barbour and Bertotti (1982); Raine (1995).

The problem has been aptly summarized by Callender and Hoefer (2002, p. 176) as follows:

> "Mach's *Science of Mechanics* (1989), at least as Einstein read it, proposed that inertia should be considered an effect of relative acceleration of a body with respect to other bodies—most notably, the "fixed stars." Transplanting this idea to the context of GTR seems to indicate having the inertial structure (which g determines) determined by the relational matter distribution. GTR does not seem to fulfill this idea in general. In some models (notably Friedman-Robertson-Walker (FRW) Big Bang models), this idea seems intuitively fulfilled. But making the idea both precise and satisfiable in GTR has proven difficult if not impossible..."

Making the Machian idea "both precise and satisfiable" in GR is the main goal of this paper. There are many perspectives from which GR appears not to satisfy this idea. Earman (1989, p. 130) illustrates that relational facts about the past motions of the particles (i.e., past world-lines for a finite system of particles) are not sufficient to fully determine their future motions. Maudlin (1993, p. 199) points out that regardless of the extent of information available concerning the past motions of a set of particles, it remains impossible to ascertain the presence of an incoming gravitational wave from beyond the system. In the 1993 Tübingen conference "Mach's Principle: From Newton's Bucket to Quantum Gravity" (Barbour and Pfister, 1995, p. 94), Hermann Bondi expressed a similar idea as a response to Julian Barbour: two Cauchy surfaces with the same stress-energy tensor but different gravitational-wave situations can develop differences.

These worries are closely connected to a common formulation of Mach's principle, according to which *the metric field $g_{\mu\nu}$ must be fully determined by the stress-energy tensor $T_{\mu\nu}$*. Call this Mach's Principle-T. This formulation can be found in Einstein (1918b, pp. 241-242), Weingard (1975, p. 427), Callender and Hoefer (2002, p. 176), Lehmkuhl (2011, p. 455) and many others. It is both mathematically and physically correct that GR does not satisfy Mach's Principle-T. In this paper, I reformulate this problem as a problem from vacuum solutions. According to Pooley, relationists in GR grapple with the fact that "the dynamically significant chronometric facts outstrip the chronometric facts about matter, as is most vividly illustrated by the abundance of interesting vacuum solutions" (2013, p. 578). Call this the vacuum challenge.

Vacuum solutions are solutions to EFE with a vanishing stress-energy tensor (i.e., $T_{\mu\nu} = 0$). The vacuum challenge offers a clear and sharp way of formulating the problem of underdetermination: differences in metrics given "the same" matter content.[6]

To delineate the scope of my argument, I begin with two disclaimers. First, I only focus on the problem of non-Minkowski vacuum solutions and restrict my discussion to the non-augmented EFE, which admit the Minkowski solution.[7] Arguably, Minkowski spacetime can take on a relationist interpretation if one adopts a dynamical approach to GR.[8] In this paper, I table the matter of Minkowski spacetime to focus on a more pressing concern. To that effect, my argument is based on what is commonly referred to as "Mach's Principle-3" formulated by Pirani (1956, p. 199), which says that *spacetime should be Minkowskian in the absence of matter*. In this context, I will henceforth use relationism and Machianism interchangeably.

Second, I take it for granted that gauge freedom does not cause trouble for relationism. Between 1913 and 1914 when the failure of general covariance in the *Entwurf* theory still bothered Einstein, his commitment to Mach's principle led him to the hole argument and made him believe there was a deeper reason—the metric $g_{\mu\nu}$ could not be fully determined by the stress-energy tensor $T_{\mu\nu}$ in any generally covariant theory.[9] We now understand this underdetermination as a feature rather than a bug. The Bianchi identity implies that there are four out of ten equations in the EFE that lack second-order time derivatives of the metric. In the 3+1 decomposition, these four equations are constraints (of elliptic type), signifying gauge freedom. We cannot determine the solutions to more than up to diffeomorphism. This particular kind of underdetermination of $g_{\mu\nu}$ is comparatively benign, and is analogous to the failure of

[6] One might object that the vacuum cases do not exhaust the underdetermination problem: even when $T_{\mu\nu} \neq 0$, the same stress-energy tensor may be compatible with different metrics. I agree. My claim is not that resolving the vacuum cases would settle all instances of matter–metric underdetermination, but that the vacuum challenge is the sharpest and most tractable starting point. If relationists can explain how vacuum solutions are compatible with their view, they will have addressed the most vivid form of the problem and gained resources for thinking about the non-vacuum cases. I thank Sam Fletcher and Dennis Lehmkuhl for pressing me on this point.

[7] See Earman (2003) for considerations regarding the cosmological constant.

[8] See Brown (2005a); Brown and Pooley (2006).

[9] See Einstein's letter to de Sitter, 4 November 1916 (CPAE 8: Schulmann et al. (1998), Doc. 273). See also Norton (1984, 1993); Maudlin (1990); Hoefer (1994).

Maxwell's equations to determine the electromagnetic potential A_μ uniquely.[10]

The relationists' challenge in GR is associated with another kind of underdetermination manifested in the existence of multiple vacuum solutions. In four or more dimensions, the Ricci tensor $R_{\mu\nu}$ is not sufficient to describe the curvature of the space. The full Riemann tensor $R_{\rho\sigma\mu\nu}$ is needed. On a common view, "matter" only couples with the ten algebraically independent components captured by the Ricci tensor, that is, the trace of the Riemann curvature tensor. The other ten components come from the Weyl tensor $C_{\rho\sigma\mu\nu}$, representing the "free" gravitational field present in a vacuum region. To satisfy the EFE in a vacuum region where $T_{\mu\nu} = 0$, only Ricci-flatness, i.e., $R_{\mu\nu} = 0$ is required. There are Ricci-flat solutions that are not conformally-flat, i.e., solutions where the Weyl curvature is non-vanishing.

The gravitational potential $g_{\mu\nu}$ and the stress-energy tensor $T_{\mu\nu}$ are analogous to the electromagnetic potential A_α and the charge-current vector J_α in electromagnetism (EM), respectively. Both $g_{\mu\nu}$ and A_α represent the potential, whereas both $T_{\mu\nu}$ and J_α can represent the source. On the other hand, the Weyl tensor $C_{\rho\sigma\mu\nu}$, in analogy to the trace-free Maxwell field tensor $F_{\alpha\beta}$, describes the trace-free part of the Riemann tensor. It captures the curvature in the Riemann tensor but with the "Ricci part" removed:

$$C_{\rho\sigma\mu\nu} = R_{\rho\sigma\mu\nu} - g_{\rho[\mu}R_{\nu]\sigma} + g_{\sigma[\mu}R_{\nu]\rho} + \frac{1}{3} g_{\rho[\mu}g_{\nu]\sigma}R. \tag{1.2}$$

Mathematically, the Riemann tensor is decomposed into three irreducible parts (the Ricci scalar, the trace-removed Ricci tensor, and the Weyl tensor). Physically, the Weyl tensor is the only surviving part in source-free regions. It encapsulates the *gravitational degrees of freedom*. While $R_{\mu\nu}$ and $T_{\mu\nu}$ are algebraically related through Eq.(1.1), the relation between $C_{\rho\sigma\mu\nu}$ and $T_{\mu\nu}$ is less explicit. It involves a first-order differential equation obtained from the Bianchi identity and

[10]Gauge freedom concerns diffeomorphic representatives of the same physical solution. The vacuum challenge concerns distinct equivalence classes of solutions: metrics that have the same stress-energy tensor but are not related by a diffeomorphism.

the EFE:

$$\nabla^\rho C_{\rho\sigma\mu\nu} = \kappa\Big(\nabla_{[\mu}T_{\nu]\sigma} + \frac{1}{3}g_{\sigma[\mu}\nabla_{\nu]}T\Big). \tag{1.3}$$

One can see a close resemblance to EM by comparing Eq.(1.3) with Maxwell's equation $\nabla^\mu F_{\nu\mu} = J_\nu$. Indeed, Eq.(1.3) can describe the propagation of gravitational waves. The freedom in the choice of $C_{\rho\sigma\mu\nu}$ means that given $T_{\mu\nu}$, multiple solutions exist, each specified by particular boundary conditions.[11] Not all components in $C_{\rho\sigma\mu\nu}$ contribute to dynamical degrees of freedom though. In linearized gravity, the gauge and residual gauge conditions cut down $4\times 2 = 8$ degrees of freedom from 10 equations, leaving 2 degrees of freedom for a propagating spin-2 field.[12]

Without the later terminology from Weyl's (1918) work available to him, Einstein was aware of the problem of boundary conditions in his Machian pursuit. In a letter to Lorentz in January 1915, regarding Coriolis and centrifugal force components of the metric (which should not be viewed as 'real' according to Einstein), he says,

> "I would like to respond to this with the supposition that we never can see the stars. According to my understanding, these [Coriolis and centrifugal] force fields are determined exclusively by the boundary conditions and the field equations, if the influence of the masses belonging to the system under study can be disregarded here. It is admittedly awkward that the boundary conditions must be picked out suitably instead of being able to assume that all boundary conditions vanish into infinity. But are you so sure that you will manage with such simple boundary conditions in you view of the world?"[13]

Einstein interprets the boundary conditions as a stand-in for the distant stars epistemically inaccessible to us, while admitting the tension between attributing Coriolis and centrifugal forces to boundary conditions and setting all-vanishing boundary conditions. He was concerned about the arbitrariness involved in stipulating spacetime properties at infinity. His goal between 1916

[11] See Carroll (2004, p. 169-70).

[12] See Wald (1984, Ch.10) for counting degrees of freedom in the initial value formulation.

[13] "Ich will darauf antworten unter der Supposition, dass wir nie die Sterne sehen könnten. Nach meiner Auffassung werden diese Kraftfelder ausschliesslich durch die Randbedingungen und die Feldgleichungen bestimmt wenn von der Wirkung der zu dem studierten System gehörenden Massen hier abgesehen werden kann. Nun ist es allerdings peinlich, dass man sich die Randbedingungen passend heraussuchen muss, anstatt annehmen zu können, alle Feldkomponenten verschwinden im Unendlichen. Aber wissen Sie denn so sicher, dass Sie bei Ihrer Auffassung der Welt mit so einfachen Grenzbedingungen auskommen?" (CPAE 8, Doc.47, English translation by Ann Hentschel)

and 1918 was to find Machian boundary conditions that are compatible with observations of the actual universe.[14]

Now we are ready to state the problem of underdetermination in GR more precisely. Similar to how degrees of freedom in the electromagnetic potential A_α can be broken down into gauge, source (J_α), and field ($F_{\alpha\beta}$) components, degrees of freedom in the gravitational potential $g_{\mu\nu}$ can also be decomposed into gauge, source ($R_{\mu\nu}$), and gravitational ($C_{\rho\sigma\mu\nu}$) components (see Table 1.1). While the gauge degrees of freedom based on the hole argument might help to reject the existence of spacetime as a fundamental and distinct substance, the gravitational degrees of freedom associated with the Weyl tensor seem to require it (since the EFE determine the Ricci part of the curvature from the stress-energy tensor, but do not algebraically determine the Weyl part). I will show that the partial determination of $g_{\mu\nu}$ from the source (after gauge fixing) does not automatically favor substantivalism, that the question remains as to what else is needed to determine $g_{\mu\nu}$. Is the extra ingredient "source from elsewhere" or "substantival spacetime"? It follows from my analysis that Mach's Principle-3 might be upheld in GR even though Mach's Principle-T fails.

Table 1.1. Degrees of Freedom in EM and GR

Physical Interpretation	**EM**	**GR**	**Degrees of Freedom**
Source	J_a	$T_{\mu\nu}$ ($R_{\mu\nu}$)	Source degrees of freedom
Trace-free part	F_{ab}	$C_{\mu\nu\lambda\sigma}$	Field degrees of freedom
Potential	A_a	$g_{\mu\nu}$	Gauge + field + source degrees of freedom

[14]One might date the end of this project to Einstein's (1917b) paper "Cosmological Considerations in the General Theory of Relativity," where he introduced a spatially closed model partly to avoid boundary conditions at spatial infinity. This is a natural stopping point if the project is understood narrowly as the search for a closed, matter-filled cosmological solution. I understand it more broadly, however, as Einstein's attempt to secure a Machian determination of inertial structure. In that sense, the problem persisted in his exchange with de Sitter, where the central issue remained whether the metric and inertial structure of the universe could be fixed by matter. I thank Dennis Lehmkuhl for pressing me on this dating.

1.3 Echoes of Hidden Matter

When Machianizing boundary conditions at infinity turned out to be trickier than Einstein initially thought, he came up with an alternative solution: eliminate infinity. He introduced the cosmological constant in 1917 and advocated for a closed spherical universe, later known as the Einstein universe. He defended Machianism in his extensive correspondence with de Sitter between 1916 and 1918, only to relinquish it in his later years. However, as Hoefer (1994) suggests, the historical intricacy of Einstein's Machian pursuit presents various possibilities for implementing Mach's ideas in GR, rather than justifying Einstein's own reason for abandonment. The purpose of this section is to provide such a possibility.

It appears that two exact solutions to the EFE are particularly suitable for this purpose: the Schwarzschild metric and Ozsváth-Schücking metric. The Schwarzschild solution is historically significant for many reasons: it was the first exact solution to Einstein's field equations, became central to relativistic gravitation, and later played a crucial role in the physics of black holes. For the present argument, however, its significance lies in Einstein's attempt to replace the Minkowskian boundary conditions used in Schwarzschild's solution with Machian boundary conditions. I demonstrate that this transition is not a major concern. Another brilliant yet less well-known example of vacuum solutions was proposed by Ozsváth and Schücking (1962) in their paper titled "An Anti-Mach-Metric". It describes a singularity-free spacetime with gravitational waves. The Ozsváth-Schücking metric may seem to pose the biggest challenge to relationism. Nevertheless, I show that relationists have two available responses.

1.3.1 Schwarzschild Spacetime

Solving the EFE is difficult, but simple solutions can be found if symmetry conditions eliminate some degrees of freedom. A symmetric metric tensor written in a matrix form has 10 slots to be determined in the 4×4 matrix. In Schwarzschild spacetime, isotropy and staticity simplify the metric so that after applying harmonic gauge in polar coordinates (t, r, θ, ϕ), the line element becomes $A(r)dt^2 + B(r)dr^2 + r^2(d\theta^2 + \sin^2\theta d\phi^2)$. Only two slots $A(r)$ and $B(r)$ need to be determined. One piece of information comes from the Newtonian limit about the mass of the central object, which solves $A(r)$. The other comes from the Minkowski limit regarding the asymptotic behavior far away from the central object. As r approaches infinity, the metric approaches the Minkowskian limit. That is, $A(r) \to -1$ and $B(r) \to 1$ as $r \to \infty$. These two pieces of information together with the EFE give us the Schwarzschild solution in its familiar form:

$$ds^2 = -\left(1 - \frac{2GM}{r}\right)dt^2 + \left(1 - \frac{2GM}{r}\right)^{-1} dr^2 + r^2(d\theta^2 + \sin^2\theta d\phi^2), \tag{1.4}$$

where G is the gravitational constant and M is the mass of the central object.

A common interpretation is that the M in the Schwarzschild metric manifests a geometric property instead of the existence of matter, because the solution can model the gravitational field of a star as well as a black hole. One can always curtain a source and replace it with boundary conditions, but "this is purely a matter of taste" ("eine reine Geschmacksfrage"), to borrow Einstein's words.[15] While de Sitter preferred to leave inertia unexplained, Einstein preferred to explain inertia via matter.[16] The standard geometrical interpretation may be appealing to substantivalists, but for relationists, Mach's explanans are better – there are effects "which masses *outside* of the system exert on its parts" (1893/1919, p,288 emphasis in original).

In fact, it was the Minkowskian limit rather than the Newtonian limit that bothered Einstein, since singularities could be viewed as a "placeholder" (Lehmkuhl, 2017, p. 212) or "surrogates for matter" (Norton, 2024, §10.4). Einstein attributed the non-Machian characteristics in Schwarzschild spacetime to the fact that Minkowskian boundary conditions are stipulated in a specific coordinate system, hence not generally covariant. To find boundary conditions that are invariant under coordinate transformations meant to find a metric whose components are either 0 or ∞. According to de Sitter, Einstein found the metric that takes on degenerate values at infinity ($g_{i4} = g_{4i} = \infty$ for $i = 1,2,3$, $g_{44} = \infty^2$, and all other components equal to zero) but eventually

[15]CPAE 8: Schulmann et al. (1998), Doc. 273.
[16]CPAE 8: Schulmann et al. (1998), Doc. 272.

abandoned it because it conflicted with observations.[17]

From a modern perspective, since choosing a coordinate system is always required to express any solution, it is interesting that Einstein required Machian boundary conditions to be generally covariant. A change in perspective may circumvent the worry about boundary conditions. Instead of requiring the metric to approach the Minkowskian limit at infinity, we can find source-carrier parameters that, if they vanish, result in the vanishing of the Riemann tensor. Vishwakarma (2015b) proposed a systematic way to define the source of curvature in vacuum solutions in terms of dimensional parameters. These parameters correspond to observable quantities such as energy, momentum, angular momentum and their densities. Based on this formulation, by requiring the gravitational field to reduce to the Newtonian potential in the limit, the source-carrier parameter L in Schwarzschild spacetime can be determined by the source mass: $L = -\frac{2GM}{c^2}$, so (4) can be rewritten as

$$ds^2 = -\left(1+\frac{L}{r}\right)c^2dt^2 + \left(1+\frac{L}{r}\right)^{-1}dr^2 + r^2(d\theta^2 + \sin^2\theta d\phi^2). \tag{1.5}$$

If L vanishes, the Riemann curvature also vanishes, reducing the solution to Minkowski spacetime. The idea aligns naturally with perturbation theory. Minkowski spacetime as a Riemann-flat solution has no gravitational field. Introducing some matter to it alters the metric, and this deviation is described by the source-carrier L.

Birkhoff's theorem gives a further reason to think that the Schwarzschild case is less damaging to Machian relationism than it first appears.[18] The theorem states that any spherically symmetric solution of the vacuum EFE is locally isometric to a member of the Schwarzschild family.[19] Thus, once the exterior region is assumed to be vacuum and spherically symmetric, the field equations leave only the Schwarzschild mass parameter (and normalization of the time

[17]See De Sitter (1916b, fn.2)). The time component is represented by index "4" based on de Sitter's convention. See Hoefer (1994, p. 304-316) for a comprehensive summary.

[18]I thank Dennis Lehmkuhl for pointing this out to me.

[19]See Birkhoff (1923).

coordinate). For finite M, the resulting metric has $g_{tt} \to -1$ and $g_{rr} \to 1$ as $r \to \infty$, after the time coordinate has been normalized. The approach to Minkowski form therefore belongs to the Schwarzschild exterior solution itself determined by the symmetry. It expresses the symmetry-reduced vacuum equations and the finite source parameter, without adding a further independent requirement of the boundary condition at infinity.[20]

The Schwarzschild solution is only valid outside a spherically symmetric mass. The boundary at the Schwarzschild radius (just like the state of the curtain) depends on the property of the source, be it a singularity or a star. The Minkowski boundary assumes effectively that there are no other masses in spacetime, or if there are, they are too distant to make a difference. The boundary conditions in Schwarzschild spacetime are not arbitrary but effective, determined by the symmetry of the external source (and the absence of any other source).

1.3.2 Ozsváth-Schücking Spacetime

In 1962, Ozsváth and Schücking proposed an ultimate anti-Machian metric. They adopted Pirani's formulation of Mach's Principle-3, which says spacetime should be Minkowskian in the absence of matter, where they interpreted "the absence of matter" as the absence of any form of source-matter, including singularities (1962, p.339). The Ozsváth-Schücking (O-S) metric is an exact vacuum solution that is singularity-free and not asymptotically flat. The line element is given by

$$ds^2 = -(dx_1)^2 + 4x_4 dx_1 dx_3 - 2dx_2 dx_3 - 2(x_4)^2 (dx_3)^2 - (dx_4)^2. \tag{1.6}$$

Unlike the Schwarzschild solution, it doesn't approximate Minkowski spacetime at infinity. Rather, it describes a pp-wave (plane-fronted waves with parallel propagation) spacetime filled up with gravitational radiation but nothing else.

This is different from the weak-field case, where gravitational waves are produced by

[20] This point has a limited scope. Birkhoff's theorem does not remove the general problem of boundary conditions in GR, since it applies only in the spherically symmetric vacuum sector and has a local character. But it does show that the Schwarzschild example is less threatening to Machian relationism than Einstein's own worry suggests.

a massive spinning object or a compact binary system. Under weak-field assumptions, the far zone allows the gravitational radiation to be meaningfully calculated just like EM radiation.[21] While the background metric is determined by $T_{\mu\nu} = 0$ in the far zone, the stress-energy tensor of the source in the near zone is non-vanishing. Since the waves in the far zone are effectively treated as plane waves, the information about the location of the source is hidden. Curtaining the lightbulb doesn't mean light is sourceless though. The dynamics is determined by the vacuum background *and* the non-trivial stress-energy tensor in the near zone.

In weak-field approximations, the source appears once the curtain that separates the far zone and the near zone is removed. But the O-S solution, being geodesically complete, does not describe a scenario where a source is hidden by a curtain. Rather, it describes a case where there is light in the room, the curtain is removed, and you can see everywhere yet there is still no lightbulb. How is that possible?

Vishwakarma (2015b) suggests one way to find the presumed hidden matter using dimensional parameters and identifies a family of vacuum solutions, in which the angular momentum density sources the curvature. Among them there is the O-S spacetime, sourced by a particular value of a parameter l, related to the angular momentum density $\mathscr{J}$ by $l = \frac{G\mathscr{J}}{c^3}$. The O-S solution (1.6) can be rewritten in new coordinates as

$$ds^2 = \left(1 - \frac{l^2x^2}{8}\right)c^2dt^2 - dx^2 - dy^2 - \left(1 + \frac{l^2x^2}{8}\right)dz^2 + lx(cdt - dz)dy + \frac{l^2x^2}{4}cdtdz, \quad (1.7)$$

where $l = 2\sqrt{2}$. Similar to the Schwarzschild case, if l vanishes, the solution reduces to Minkowski spacetime. Thus, it is the source-carrier l, or the angular momentum density, which explains the non-vanishing Riemann tensor. While the stress-energy tensor cannot fully represent the source in the spacetime, dimensional parameters can reveal the hidden matter. Vishwakarma argues that his method Machianizes Ozsváth and Schücking's anti-Mach metric—"if the presence of source in the vacuum solutions is defined by the presence of such parameters...all the solutions

[21] See Gomes and Rovelli (2024) as well as discussions in Chapter 2 and Chapter 3 of this dissertation.

become Machian!"(2015b, p. 1109)

Representing the source of curvature in terms of angular momentum density may be taken with a pinch of salt. While Vishwakarma (2015a) suggests that (1.7) results from a rotating matter distribution, the attribution of angular momentum density remains unclear.[22] At the very least, this approach offers an alternative interpretation that circumvents reliance on the geometric property to explain the metric. If shoe-horning dimensional parameters into sources seems unconvincing, one can also represent sources by constructing an effective stress-energy tensor (known as the Bel-Robinson tensor) from the Weyl tensor. Goswami and Ellis (2018) showed that such a construction is always possible for radiation-like Petrov type N spacetimes, which include the O-S spacetime.

There is yet another response that relationists can offer. The short version simply asserts that the situation is "unphysical" if there is light in the room and when the curtain is removed, there is no lightbulb. The longer version appeals to a conjecture posed by Ehlers and Kundt (1962) that the plane waves are the only geodesically complete pp-waves, in combination with Penrose's 1965 theorem that the plane waves are not globally hyperbolic. Ehlers and Kundt (1962, p. 97) posed the following problem:

> "Prove the plane waves to be the only *g*-complete *pp* waves, no matter which topology one chooses."[23]

Translated into our modern language, the conjecture suggests the plane waves are the only geodesically complete, Ricci-flat classical pp-waves. It is founded on the belief that gravitational radiation cannot be present in a completely sourceless spacetime. It considers plane waves as mathematical idealizations and any other pp-wave spacetime as incomplete, with the source left out in modelling.[24]

[22] One might wonder, for instance, if it is the spacetime that possesses the angular momentum, this maneuver supports substantivalism rather than relationism.

[23] See Flores and Sánchez (2020, p. 7508) regarding interpretive issues with the claim "no matter which topology one chooses."

[24] No counterexample to this conjecture has been identified. Significant progress has been made in establishing its validity under specific conditions, yet it generally remains as an open question. See Flores and Sánchez (2020) and Roche et al. (2023).

Ozsváth and Schücking regard their solution as a real threat to Machianism precisely because it is complete. Since complete spacetimes are inextendible, we are not just curtaining the part of the spacetime that contains a source. However, the O-S solution is a plane-wave solution. Penrose (1965) shows that embedding a plane wave globally within any hyperbolic normal pseudo-Euclidean space is impossible due to the absence of an adequate spacelike hypersurface in the spacetime for the global specification of Cauchy data. He employs a plane "sandwich" wave comprising a series of null geodesics converging on a pair of nonintersecting null geodesics and examines the exact "focusing effect" of such a wave on null cones.[25] For purely electromagnetic waves, the null cone of is "focused" by the wave to the single point (called "anastygmatic focusing"). For purely gravitational waves, the null cone is focused by the wave onto a spacelike line (called "astygmatic focusing").[26]

The focusing effect renders the identification of a global Cauchy hypersurface impossible. For a Cauchy hypersurface to obtain globally to specify Cauchy data, it's necessary that each null geodesic intersects it precisely once. However, if every null geodesic of the sequence within the sandwich wave were to intersect any potential Cauchy hypersurface once, the focusing property would necessitate multiple null geodesics to intersect the hypersurface more than once. If each null geodesic were to intersect the hypersurface *only once*, then the hypersurface had to bend in a way such that *at most one* null geodesic could intersect it. Other null geodesics would inevitably bypass the hypersurface entirely.[27] There is no spacelike hypersurface in the spacetime for the global specification of Cauchy data, so a plane-wave spacetime such as the O-S spacetime is not globally hyperbolic.

The absence of global hyperbolicity alone may not render a spacetime as unphysical, provided it can be extended, repairing its global hyperbolicity. However, if the spacetime is also

[25] To precisely define a "sandwich" wave, one must express the metric in Brinkmann coordinates using a quadratic function. I omit it here due to space constraints. See Eq.(3.1-3.7) and Fig.1 in Penrose (1965).

[26] Penrose proves the detailed result using sufficiently weak sandwich waves because they are mathematically and visually transparent: flat before the wave, curved inside the sandwich, flat after the wave. But he explicitly intends the method and the global-Cauchy-surface obstruction to apply to general complete plane waves.

[27] See Penrose (1965, Fig. 2) for a visual demonstration.

complete, it is inextendible. Roche et al. (2023), following Flores and Sánchez (2020), regard the *conjunction* of being complete and not globally hyperbolic as indicative of the spacetime being "unphysical" (or a mere idealization), because the solutions from arbitrary initial data would not be *unique*.[28] The essence of the Ehlers-Kundt conjecture, as interpreted by Flores and Sánchez (2020), is that source-free dynamics fall under one of two possibilities: either the spacetime is incomplete, or it represents a mere idealization. Accordingly, relationists can either remove the curtain to reveal the source, or claim that it is unphysical.

The idea that all gravitational waves must have a source mirrors Ritz's hypothesis (1908) or the Sommerfeld (1912) condition that all electromagnetic waves must have a source and that there is no incoming wave from infinity.[29] Zeh (2007 p.20) notes,

> In Maxwell's classical *field theory* ... every bounded region of spacetime may contain 'free fields', which possess neither past nor future sources *in this region*. Therefore, one can consistently understand Ritz's hypothesis only cosmologically: *all* fields must possess advanced sources ('causes') somewhere in the Universe.

Zeh's emphasis in italics applying to GR exactly highlights the idea behind the Ehlers-Kundt conjecture and the moral of the curtain story. Free fields are only "free" in *bounded regions*.

The two Machian responses to the O-S spacetime are very different in kind. The dimensional parameters suggest that the presence of hidden matter (which is not represented by the stress-energy tensor) can explain the metric. Even though the formulation of source-carrier parameters is not generally covariant, at the very least, this approach offers an alternative interpretation that circumvents reliance on the a priori geometric property to explain the metric. Einstein took issue with "geometrization" of gravity and dismissed it as an Eselsbrücke ("donkey bridge"), a kind of crutch for novices.[30] It is tempting to explain differences in metric in terms the Weyl curvature, viewed as a geometric property of spacetime, but dimensional parameters

[28]The same worry—the failure of determinism, is also used in the Strong Cosmic Censorship Conjecture by Penrose (1979) to filter out "physically unreasonable" spacetimes. See Manchak (2011) for a different view regarding "physically-reasonable" spacetimes. It's also worth noting that Penrose (1965, pp.219-220) thinks plane-wave spacetimes are "no less physical (as idealizations) than their flat space counterparts".

[29]The restricted formulation of EM that forbids unsourced radiation can be found in Jefimenko (2000) and Griffiths (2013, sec.10.2.2). See also Lange (2002).

[30]Letter to Hans Reichenbach, 1926 (CPAE 15, Doc.249).

provide an alternative explanation. Again, in a nod to Einstein it's *a matter of taste*. While the first response aims to Machianize the anti-Mach solution through identifying hidden matter, the second response suggests that the anti-Mach solution does not pose a real threat because it is physically unreasonable. Ultimately, the preference between these two responses hinges on whether one worries more about general covariance or the justification for "physically unreasonable" spacetimes.

Finally, it isn't surprising that GR fails to satisfy Mach's Principle-T, because $T_{\mu\nu}$ has limitations in describing the global matter distribution. It can represent a uniform and homogeneous matter distribution globally, but falls short when matter clusters, as in the early universe. Penrose (1981, 1986) proposed a cosmological initial condition $C_{\rho\sigma\mu\nu} \approx 0$, known as the Weyl curvature hypothesis.[31] The freedom to stipulate vanishing Weyl curvature would prima facie bother relationists. However, the worry removes itself if we follow Penrose's description of the early Universe, in which Ricci curvature represents the uniform matter and Weyl curvature represents "the clumped matter" (2007, p. 766).[32] When cosmic structures form, the stress-energy tensor represents the "regional source", whereas the Weyl tensor models the "external source" such as stars and blackholes. The degrees of freedom in the Weyl tensor simply allow "the external" to affect "the regional". This is compatible with what Barbour (2010) called Mach's dictum of *the All*, which takes into consideration "things which for the time being we left out of account".[33]

[31]It parallels Ritz's hypothesis 1908, which eliminates electromagnetic degrees of freedom by postulating the cosmological initial condition $F^{\alpha\beta} = 0$. For Zeh (2007, p. 138), the Weyl hypothesis is analogous to $A^{\alpha}_{\text{incoming}} = 0$, but I use $F^{\alpha\beta}$, because $F_{\alpha\beta}$ is analogous to $C_{\rho\sigma\mu\nu}$.

[32]This might strike readers as both interesting and puzzling. Earlier I described Ricci curvature as capturing the matter degrees of freedom and Weyl curvature as capturing the free gravitational degrees of freedom. Penrose's way of speaking is compatible with that distinction, provided we understand it as a claim about modeling domain: homogeneous matter included in the cosmological model is represented through Ricci curvature, while the gravitational influence of matter treated as external, clumped, or excised from the region is represented through Weyl curvature.

[33]"It is certainly fortunate for us, that we can, from time to time, turn aside our eyes from the overpowering unity of the All, and allow them to rest on individual details. But we should not omit, ultimately to complete and correct our views by a thorough consideration of the things which for the time being we left out of account." (Mach 1983/1919 p.235)

1.4 Conclusion

Many successful defenses of relationism have been put forth in the context of Newtonian physics and Special Relativity, but the same strategies are known to face significant challenges when extending to GR due to the problem of underdetermination. In this paper, I reformulated the problem of underdetermination as the vacuum challenge. While some relationists have addressed this challenge by excluding "unphysical" vacuum solutions by fiat, I offered a richer story by recognizing the modelling artifacts involved. The metric field is underdetermined by the stress-energy tensor because an extra ingredient, the Weyl tensor, is needed. Decomposing the degrees of freedom in the metric into gauge, "source", and gravitational degrees of freedom, I identified the gravitational degrees of freedom (associated with the Weyl tensor) as the "cause" of the problem of underdetermination.

There is nothing intrinsically substantival about the Weyl tensor. It allows for gravitational effects that *something outside of a system exerts on the system*. On my interpretation, the vacuum challenge originates from the assertion that there *are* gravitational effects from *nothing outside*. The boundary problem arises when we rely on boundary conditions to do the heavy-lifting of sourcing without permitting anything to exist on the opposite side of the boundary. I then proposed a relationist response: "free" gravitational degrees of freedom are only free modelling artifacts. I did not, however, argue that relationsim is correct. There are further interpretive questions regarding the nature of the propagating degrees of freedom in the context of energy conservation in GR. But at least for the vacuum challenge, relationists can respond: nothing matters because in nothing we find matter.

Acknowledgments

Chapter 1, in part, is based on the material as it appears in "Nothing Matters" published in *Philosophy of Science*, Volume 92, Issue 5, 2025. The dissertation author was the sole author of this paper.

Chapter 2

Why Not Perpetual Motion Machines?

2.1 Introduction

It is no surprise that energy conservation is a difficult topic in the foundations of General Relativity (GR). The background assumptions that normally underwrite conservation laws do not carry over smoothly to GR. In classical mechanics, energy conservation is invoked for *isolated* systems, and such systems are assumed to evolve against a fixed spacetime background—their motion does not itself change the background or break the symmetries of the spacetime in which they are embedded. Under these conditions, energy conservation functions as a tool for tracking an *isolated* system's *dynamical evolution* over time: it expresses the fact that some quantity remains invariant as the system evolves from one state to another. In GR, however, these conditions that make conservation laws conceptually transparent are no longer available. Because spacetime is itself dynamical and sourced by matter, the assumption of a fixed background that underwrites standard conservation reasoning is precisely what GR denies. As a result, the notion of an isolated system, which ordinary conservation arguments take for granted, is in tension with the theory's dynamical structure, given the coupling between matter and spacetime.

To make the tension more precise, we can begin by asking what it is for a system to count as isolated. Ordinarily, an isolated system is one that exchanges no energy with its surroundings. Take a box of gas, thermally sealed, and you have something close enough to an isolated system. But gravity cannot be switched off by enclosing a system within walls. There is no gravitational insulator. At best, one imposes boundary conditions at a distance, typically asymptotic flatness, so that the system behaves *as if* it were isolated. While this move is practically useful for most observational purposes, it is important to note that the theory does not naturally partition isolated systems from their "surroundings", since matter and spacetime are dynamically coupled through the Einstein field equations. The evolution of each depends on the other. Prima facie, if a system cannot be cleanly isolated, the usual conservation reasoning cannot be strictly applied.

GR does, of course, supply a kind of "conservation law": the covariant conservation of the stress–energy tensor. But this is not conservation in the familiar dynamical sense. In classical mechanics, conservation laws constrain evolution by relating one state of a system to another over time. By contrast, the covariant conservation of stress-energy, as a consequence of mathematical identities, does not *on its own* relate one state of a system to another over time, nor does it define a conserved total energy.[1] The conservation we want—one that licenses inferences of the form "this is the energy now, so this is what it will be later"—does not follow.

This explains why energy conservation in GR is such a difficult topic. The form of conservation that GR straightforwardly provides—the covariant conservation—is not the one that does the work we ordinarily want conservation laws to do; and the conservation law that *would* do that work presupposes a notion of isolation that GR does not naturally supply. These tensions lie at the heart of one of the theory's longest-running debates: do gravitational waves carry energy away from the systems that emit them? Even Einstein changed his mind more than once on the status of gravitational waves in GR.[2]

The debate is often framed as a dispute between two broad camps, which may be labelled *gravitational-energy realism* and *gravitational-energy eliminativism*. Realists hold that when matter energy fails to be conserved, it does not simply disappear; rather, it is transferred *by*, or *to*, the gravitational field. On this view, gravitational energy is *real* and can *do work*. The most famous argument in support of this position is the Bondi–Feynman sticky-bead thought

[1] Only when additional structure is imposed (e.g., a timelike Killing vector field) can it be converted into a genuine conservation law. In generic spacetimes, no such structure is available.

[2] See Kennefick (1996) for a historical review.

experiment, which is intended to show that gravitational waves can do work on matter—heating sticky beads as they pass—and thus that gravitational radiation carries energy. Rovelli (1997, p.197) expresses the same idea: a sufficiently strong burst of gravitational waves could knock down the Rock of Gibraltar, just as a sufficiently strong burst of electromagnetic radiation could. Versions of this realist position have been defended more recently by, among others, Pitts (2010), Read (2020), and Gomes and Rovelli (2024).

Eliminativists, by contrast, often emphasize that gravitational energy in GR is represented by pseudotensors, which are neither geometric objects nor uniquely defined. Such quantities can be made to vanish in suitable coordinate systems, and the claim thus goes that they fail to represent something real. Early skeptics of gravitational energy include e.g., Infeld and Scheidegger (1951), Rosen (1956), and Ehlers (1980). The elimitivsit view is developed more recently by Hoefer (2000), Duerr (2019a), Duerr (2019b), and Duerr (2021), with Fletcher (2025) holding a skeptical though not eliminativist view. On the view of Hoefer and Duerr, if gravitational energy is not real, we should accept that energy conservation is *simply violated* in GR. As Duerr (2021) puts it, gravitational energy is "explanatorily dispensable"; and, as Hoefer (2000, p. 196) suggests, "there is no need to make up a story about where [energy] has gone when a system loses it."

In this paper, I argue that there is still a need to "make up a story" of *energy balance*—specifically, to rule out gravitational perpetual motion machines. Although the literature often treats *energy balance* and *energy conservation* interchangeably, I propose a distinction that is suggested, though not explicitly formulated, by Einstein's use of the term *Energiebilanz* in several places. Energy balance concerns the transfer of energy between *open systems*, whereas energy conservation applies only to *closed systems*. Conservation is therefore the stronger notion, since it presupposes an isolation condition. This distinction is especially salient in GR. Strict energy conservation holds only in highly idealized cases, while approximate conservation and energy balance remain applicable in other scenarios.

I reassess the debate between realists and eliminativists about gravitational energy by shifting attention from *disputes about realism* to *objections to energy balance*. These objections typically take the following form: (1) if gravitational energy lacks property X, then it is not real; (2) if gravitational energy is not real, then there is no energy balance between matter and the gravitational field. Much of the existing discussion focuses on premise (1), leading to a stalemate over what counts as "real." I instead pursue a *reductio ad absurdum*: if there were no energy balance in GR, then gravitational perpetual motion machines would be possible.

I then suggest that eliminativists can reject premise (2). Doing so allows them to endorse energy balance while remaining skeptical about gravitational energy, thereby blocking gravitational perpetual motion machines. On this view, energy balance functions as a constraint on the *maximal work* that can be extracted from a gravitational system.[3] Work—such as tidal heating—is gauge-independent and objective, even by the standards of pseudotensor skeptics. The key idea is that work occurs when the physical situation itself induces a localization of gravitational energy that is otherwise non-local.

The paper proceeds as follows. Section 2.2 explicates the notion of energy balance and distinguishes it from energy conservation, clarifying the conditions under which local and global conservation equations hold. Section 2.3 examines the role of energy balance as a physical constraint on energy extraction, showing how it excludes gravitational perpetual motion machines—something that the mere failure of energy conservation cannot do. Section 2.4 concludes.

[3]One may remain non-committal about the ontology of the "gravitational system." The claim can be reformulated in terms of *work done* by the gravitational field, the source, or gravitational waves, provided tidal effects and radiation are understood as varieties of gravitational waves (cf. Goswami and Ellis (2021)).

2.2 Energy Balance and Energy Conservation

In the *Manuscript on Special Relativity*,[4] Einstein presents classical electrodynamics and, in particular, what he calls the "energy balance" equation for the electromagnetic field, commonly known as Poynting's theorem. In modern notation, it is often written as

$$\frac{\partial u}{\partial t} = -\nabla \cdot \mathbf{S} - \mathbf{E} \cdot \mathbf{j}, \tag{2.1}$$

where u is the density of the electromagnetic energy and $\mathbf{S}$ is the electromagnetic energy-flux (Poynting) vector, and $\mathbf{E} \cdot \mathbf{j}$ is the rate per unit volume at which energy is transferred from the electromagnetic field to charged matter. Einstein explains that, upon integrating this over an arbitrary spatial volume, one may interpret this equation as expressing the "*Energiebilanz*" (energy balance) of the electromagnetic field.[5] The change in electromagnetic field energy within the volume is balanced by (i) the net electromagnetic field energy flowing out across the boundary of the volume and (ii) the energy transferred from the field to charges inside the volume.

[4]Einstein's manuscript on special relativity was written for Erich Marx's *Handbuch der Radiologie*, probably between 1912 and 1914, with the bulk of the text composed in 1912. Einstein was working toward a generalized theory of relativity. The manuscript was not published in the handbook because the First World War delayed the project, and when Marx later asked Einstein to revise the manuscript for publication, Einstein declined, apparently regarding the text as obsolete. See the editorial note, "Einstein's Manuscript on the Special Theory of Relativity," in CPAE Vol.4 p. 3-7; the manuscript itself is printed in CPAE Vol.4 Doc.1. I thank Dennis Lehmkuhl for bringing the manuscript's publication status to my attention.

[5]The integrated equation in the original notation reads

$$\frac{\partial}{\partial t}\{\int \frac{1}{2}\left(\mathfrak{e}^2+\mathfrak{h}^2\right) d\tau\} = \int c\,[\mathfrak{e}\mathfrak{h}]_n\, d\sigma - \int \rho\, \mathfrak{q}\mathfrak{e}\, d\tau.$$

Here $\mathfrak{e}$ and $\mathfrak{h}$ denote the electric and magnetic fields, so that $\frac{1}{2}(\mathfrak{e}^2+\mathfrak{h}^2)$ corresponds to electromagnetic energy density u in modern notation (up to unit conventions); the bracket $[\mathfrak{e},\mathfrak{h}]$ denotes the vector product $\mathfrak{e}\times\mathfrak{h}$, so $c[\mathfrak{e},\mathfrak{h}]$ is the electromagnetic energy-flux vector, and $c[\mathfrak{e},\mathfrak{h}]_n$ denotes its component along the inward normal to the boundary surface; $\mathfrak{q}$ denotes the velocity of electrically charged matter, and $\rho\mathfrak{q}\mathfrak{e}$ is the rate at which energy is transferred from the electromagnetic field to matter per unit volume. Einstein writes: "One may understand this equation as the equation of energybalance for the electromagnetic field. In that case, $w=\frac{1}{2}(\mathfrak{e}^2+\mathfrak{h}^2)$ is to be understood as the density of electromagnetic energy, and $\mathbf{s}=c[\mathfrak{e}\mathfrak{h}]$ as the vector of energy current, while $\rho\mathfrak{q}\mathfrak{e}$ denotes the energy extracted from the electromagnetic field per unit volume per unit time." (CPAE Vol.4 Doc.1) I thank Dennis Lehmkuhl for translation suggestions; all remaining errors are mine.

"Man kann diese Gleichung als die Gleichung der Energiebilanz des elektromagnetischen Feldes auffassen. Dann ist $w=\frac{1}{2}\left(\mathfrak{e}^2+\mathfrak{h}^2\right)$ als die Dichte der elektromagnetischen Energie, $\mathbf{s}=c[\mathfrak{e},\mathfrak{h}]$ als Vektor der Energieströmung aufzufassen, während $\rho\mathfrak{q}\mathfrak{e}$ die pro Volumen und Zeiteinheit dem elektromagnetischen Felde entzogene Energie bedeutet."(CPAE Vol.4 Doc.1)

He then notes that this balance relation can *turn into* a conservation law if “the field strengths vanish permanently at the boundaries of the space.”[6] If the energy flux **S** through the boundary is zero (so that no electromagnetic energy crosses the boundary), then the balance relation reduces to $\int \mathbf{E}\cdot\mathbf{j}d^3x = -\frac{\partial}{\partial t}\int ud^3x$, i.e., any energy gained by the charges inside the isolated region is matched exactly by a decrease of electromagnetic field energy within the region.[7] Under this isolation condition, Eq.(2.1) becomes a genuine conservation statement: the total energy (field plus charged matter) is constant in time.

While it is common to describe Eq.(2.1) itself as a “conservation law,” and to use “energy balance” and “energy conservation” interchangeably, Einstein is careful to note that Eq.(2.1) is not yet a statement of conservation. It neither presupposes that the system under consideration is isolated nor, by itself, yields a quantity that remains constant in time. Rather, it expresses an open-system balance relation, allowing the field’s energy to change both by flux through the boundary and by exchange with charged matter. A conservation law follows only after imposing an *isolation condition* that makes the boundary term vanish—so that no energy enters or leaves the region, as in idealized cases where the fields fall off sufficiently fast at spatial infinity or where the region is enclosed by a perfect conductor (with the electric or magnetic field normal to the boundary). Under the isolation condition, the equation implies the conservation of total energy for the coupled system. Of course, if one further assumes that there is no energy exchange between charges and the field (i.e., $\mathbf{E}\cdot\mathbf{j} = 0$), then the field energy and the charge energy are each independently conserved.

Besides Einstein, a few authors also use energy balance in a way that is not simply interchangeable with energy conservation. For example, Müller (2007) describes “the familiar energy balance of classical mechanics” as the claim that “the rate of change of kinetic energy...equals the power of the force.” Poisson and Will (2014, Ch.12) emphasize that for radiative systems in electromagnetism “energy is not even conserved”, but instead satisfies a balance relation

[6] “...an den Grenzen des Raumes die Feldstärken dauernd verschwinden ...”(CPAE Vol.4 Doc.1)

[7] The equation in the original notation reads $\int \mathfrak{q}\mathfrak{f}d\tau = -\frac{\partial}{\partial t}\{\int wd\tau\}$, where $\mathfrak{q}\mathfrak{f} = \rho\,\mathfrak{q}\mathfrak{e}$ corresponds to $\mathbf{J}\cdot\mathbf{E}$ and $w = \frac{1}{2}(\mathfrak{e}^2 + \mathfrak{h}^2)$ corresponds to u in modern notation. (CPAE Vol.4 Doc.1, Eq.5b)

of the form $dE/dt = \mathscr{P}$, where E is the sum of mechanical and field energy, and $\mathscr{P}$ is the radiated power. This is exactly what one should expect because in radiative systems energy flux through infinity does not vanish, and hence there is no closed system to which a conservation law could apply. To my knowledge, however, no general definition of energy balance is standard. I therefore define:

> **Energy balance.** An energy-balance statement relates energy change in an *open* subsystem to balancing terms (e.g., energy flux across the boundary and energy transfer with other interacting subsystems).

With an additional *isolation* condition, a balance equation can turn into a conservation equation for the closed (total) system. This notion of energy balance is deliberately general. I do not intend to assign it any nomological status at this point.[8]

One might be tempted to identify energy balance simply with a local continuity equation (i.e., the change of a quantity in the volume V equals the flow of that quantity through the boundary of V). Often this is appropriate. But in electromagnetism and GR, balance relations also arise in a non-local and coarse-grained way, where one relates energies defined in different "zones." In radiative problems, for instance, it matters whether energy carried by waves at large radius r and coordinate time t is balanced by a corresponding decrease of a near-zone energy at the retarded time $(t - r/c)$. In such cases, balance is not associated with "local divergence-freeness," but a relation between energy losses and energy gains across different zones defined using matching techniques and boundary conditions.

Since the concept is general, there is no single equation for energy balance. But to set the stage, it is helpful to see how energy balance works in Special Relativity (SR). Let $T^{\mu\nu}$ be the stress-energy tensor associated with some system of interest. The balance relation can be written as

$$\partial_\mu T^{\mu\nu} = -f^\nu, \tag{2.2}$$

[8] As we will see, it can be an interpretation of a divergence equation; an assumption built into post-Newtonian and post-Minkowskian approximation schemes; a heuristic that guides the definition of energy; and in special cases, a genuine conservation law once an isolation condition is imposed.

where f^{ν} is the 4-force density exerted by the given system on the external system (or equivalently, given the action-reaction principle, $-f^{\nu}$ is the force density exerted by the external system on the given system). For example, a collection of point particles n, with 4-momentum p_n^{μ} has a stress-energy tensor

$$T^{\mu\nu}(\mathbf{x}) = \sum_n p_n^{\mu} \frac{dx_n^{\nu}(t)}{dt} \delta^3(\mathbf{x} - \mathbf{x_n}(t),$$

where $\delta^3(\mathbf{x} - \mathbf{x}_n(t))$ is the three-dimensional Dirac delta distribution centered on the spatial position $\mathbf{x}_n(t)$ of the nth particle. It represents the fact that the particle's energy and momentum are localized at its instantaneous position. If external forces act on the particles, the balance relation $\partial_{\mu} T^{\mu\nu} = -f^{\nu}$ holds and $-f^{\nu} \equiv \sum_n \delta^3(\mathbf{x} - \mathbf{x_n}(t)) \frac{dp_n^{\nu}(t)}{dt}$ encodes the force density; for free particles, $f^{\nu} = 0$ and the conservation holds $\partial_{\mu} T^{\mu\nu}(x) = 0$.[9]

As a second example, consider the electromagnetic stress-energy tensor

$$T^{\mu\nu}_{\text{EM}} = F^{\mu\alpha} F^{\nu}{}_{\alpha} - \frac{1}{4} \eta^{\mu\nu} F^{\alpha\beta} F_{\alpha\beta},$$

where $\eta^{\mu\nu}$ is the inverse Minkowski metric and $F^{\alpha\beta}$ is the electromagnetic field-strength tensor. In terms of the electromagnetic four-potential A^{μ}, it is given by $F^{\alpha\beta} = \partial^{\alpha} A^{\beta} - \partial^{\beta} A^{\alpha}$. The associated force density on charged matter is the Lorentz-force density $f^{\nu} \equiv F^{\nu\lambda} J_{\lambda}$, where J^{λ} is the electromagnetic four-current density. Accordingly, the electromagnetic balance equation is $\partial_{\mu} T^{\mu\nu}_{\text{EM}} = -F^{\nu\lambda} J_{\lambda}$. If we consider the time-like component of Eq.(2.2), i.e., take $\nu = 0$, then we recover Eq.(2.1), the Poynting theorem. From this balance relation one can obtain a conservation statement in (at least) two ways. First, *in absence of charges and currents*, $\partial_{\mu} T^{\mu\nu}_{\text{EM}} = 0$ and the electromagnetic stress–energy is locally conserved. Second, if one instead

[9]It's worth nothing, however, whether an equation represents 'conservation' or 'balance' can't just be read off from the form of the equation. I refer to $\partial_{\mu} T^{\mu\nu}(x) = 0$ as a conservation equation, although formally it is still a continuity equation and can therefore be interpreted as expressing a balance of densities and fluxes. For example, setting $\nu = 0$ yields a statement that the change in energy density is balanced by the energy flux. To obtain a *conserved* (time-independent) charge in the strict sense, one still need to impose suitable boundary conditions (e.g. vanishing fields on the boundary) so that surface terms vanish (see Gorelik (2002)). In SR (and locally in GR) this can always be arranged. Hence, I adopt the convention in this paper of referring to $\partial_{\mu} T^{\mu\nu}(x) = 0$ as a conservation equation, and reserving the term "balance" for cases involving an exchange between matter and the gravitational field.

treats *field and charged matter together* as a total system, then a conserved total energy follows provided the system is *isolated* in the relevant sense—e.g. the net Poynting flux through the boundary vanishes.

Energy balance is therefore more general than energy conservation: balance does not presuppose isolation. In what follows, we will see that the failure of (strict) energy conservation in generic spacetimes in GR turns out to have a familiar explanation: it mirrors cases in classical mechanics where conservation fails because the system is *not isolated*. In this respect, GR is less exceptional than it is often taken to be.

2.2.1 Conservation Equations and Energy-Balance Equation

In GR, a direct consequence of the contracted Bianchi identities and the Einstein field equations is that the stress-energy tensor is covariant divergence-free. This is captured by the covariant divergence equation

$$\nabla_\mu T^{\mu\nu} = \partial_\mu T^{\mu\nu} + \Gamma^\mu_{\mu\lambda} T^{\lambda\nu} + \Gamma^\nu_{\mu\lambda} T^{\mu\lambda} = 0, \tag{2.3}$$

where the Γ terms are Christoffel symbols of the unique torsion-free metric-compatible Levi-Civita connection. Christoffel symbols are expressed in terms of first-order derivatives of the metric.

Taken on its own, however, this equation does not constitute a conservation law in the usual sense—a point already noted by Einstein (1918a): "From a physical point of view, this equation cannot be considered a fully adequate equivalent of the conservation laws of momentum and energy, since there are no corresponding integral equations that could be interpreted as conservation laws of momentum and energy."[10] Without additional structure (such as a timelike Killing vector), it cannot be integrated to yield a conserved quantity, and it does not relate one

[10] "Vom physikalischen Standpunkt aus kann diese Gleichung nicht als vollwertiges Äquivalent für die Erhaltungssätze des Impulses und der Energie angesehen werden, weil ihr nicht Integralgleichungen entsprechen, die als Erhaltungssätze des Impulses und der Energie gedeutet werden können." Einstein (1918a) in CPAE Vol.7 Doc.9.

state of a system to another in the way energy conservation is supposed to do (cf. Hoefer (2000); De Haro (2022)). As Landau and Lifshitz (1975, p.280) remark, "this equation does not generally express any conservation law whatever".

One might note that Eq.(2.3) already looks like a balance equation. Indeed, if the terms $\Gamma^{\mu}_{\mu\lambda}T^{\lambda\nu}$ are moved to the right-hand side of Eq.(2.3), the result resembles the balance equation $\partial_\mu T^{\mu\nu} = -f^\nu$. This is not surprising. In SR gravity may be treated as a force, whereas in GR it is encoded in the connection coefficients $\Gamma^{\mu}_{\mu\lambda}$.[11] It is therefore understandable that Eq.(2.3) is sometimes described as a balance equation (by e.g. Einstein and Grossmann (1913), Pitts (2010), Pitts (2016)). Still, I will refer to Eq.(2.3) as the covariant divergence equation, and treat "energy–momentum balance" as an *interpretation* of it. The reason is dialectical: both realists and eliminativists about gravitational energy accept Eq.(2.3), but they disagree over whether it warrants reading the connection terms as encoding a genuine exchange between matter and a gravitational energy–momentum.

As in electromagnetism, there are two potential routes from here to conservation. Accordingly, I'll articulate two different senses of isolation. One is for matter itself to be *independently isolated*, so that its stress–energy tensor is conserved *by itself*. This may hold locally effectively, and globally if and only if the spacetime admits a timelike Killing vector field. The other is for matter and the gravitational field to form a *collectively isolated* system. This route to conservation is controversial as it appeals to a notion of gravitational energy, but there are reasons to think that it holds in asymptotically flat spacetimes. We will examine these conditions in turn.

2.2.2 Local Conservation

It is often said that energy conservation in GR always holds locally, on the grounds that locally the Christoffel symbols can be made to vanish, so that the covariant conservation equation

[11] This should be understood with some care. Contrary to the common assumption that Einstein viewed GR as eliminating gravitational force in favor of geometrization, Lehmkuhl (2014) argues that Einstein treated the connection coefficients as the gravitational-force terms.

reduces to

$$\partial_\mu T^{\mu\nu} = 0. \tag{2.4}$$

We call this the ordinary conservation equation for matter fields, to distinguish it from the covariant divergence equation. This is just the familiar conservation equation of flat (i.e. Minkowskian) spacetime.

To evaluate whether Eq.(2.4) holds locally in GR, an appropriate notion of locality is required. While mathematically one may speak of a point or an arbitrarily small neighborhood of a point, conservation is a dynamical notion that concerns not just a single event in spacetime but the evolution of physical quantities. Therefore, it should apply to extensions in spacetime. I therefore propose the following definition:

> **Local Region**. A region counts as local, in the effective sense relevant here, just in case curvature/tidal effects across the region are negligible for the physical process under consideration. In local inertial coordinates, this means that the second-order metric corrections remain small throughout the region.

This definition is motivated by the existence of local Lorentz frames.[12] At a given point p, the first-order derivatives of the metric can always be transformed away, but second-order derivatives cannot.[13] This means that we can make the metric Minkowskian only up to first order.

[12]This connects to the strong Equivalence Principle, but to avoid interpretive complications I formulate the claim directly in terms of local Lorentz frames. See Lehmkuhl (2022) for a survey of formulations of the Equivalence Principle and Knox (2013) for an effective interpretation that aligns with the approach taken here.

[13]We can show the existence of local Lorentz frame by comparing the independent components of freedom available in coordinate transformations with the independent components of freedom of the conditions imposed by the metric and its derivatives. The idea is that we can freely specify coordinate-transformation terms to obtain a metric as close to the Minkowski form as possible. This procedure makes explicit how much of the metric's structure can be transformed away at a given point. The rest is a counting game. At zeroth order at p, the metric imposes 10 conditions, and there are 16 independent components of freedom in coordinate transformations. Satisfying 10 conditions is then easy, leaving us an extra 6 independent components of freedom, which correspond to the 3 rotations and 3 boosts of the Lorentz group. At first order at p, the derivatives of the metric impose 40 conditions while coordinate transformations provide 40 components of freedom, exactly matching the number of conditions. So not only can we make the metric Minkowskian at zeroth order at p, but we can also make the tangent space flat. At second order, the second derivatives of the metric introduce 100 constraints, but the coordinate transformation provides only 80 components of freedom. The difference—20 degrees of freedom—precisely matches the number of independent components of the Riemann curvature tensor. See also Fletcher and Weatherall (2023) for geometric treatment that arrives at the same conclusion.

Schematically,

$$g_{\bar{\mu}\bar{\nu}} = \eta_{\bar{\mu}\bar{\nu}} + \mathscr{O}[(\partial^2 g)(\delta x)^2], \tag{2.5}$$

where δx is the coordinate distance from p and barred indices denote coordinates that we are free to choose. Linnemann, Read, and Teh (forthcoming) make the same point explicitly using Riemann normal coordinates, framing local Minkowskian structure as scale-relative.[14] While what counts as "sufficiently small" in GR is context-dependent—depending on the physical system under consideration (cf. Brown (2005b) and Knox (2013)), a natural choice of scale is determined by the second-order derivatives of the metric. Eq.(5) gives us a rough estimate for the *size* of a region over which spacetime can be treated as approximately Minkowskian: $l \sim 1/\sqrt{\partial^2 g}$, or, more invariantly, $l_R \sim 1/\sqrt{|R|}$ where l_R is the local curvature radius. For a spacetime region larger than this, the approximation in Eq.(2.5) breaks down, for instance when $\mathscr{O}[(\partial^2 g)(\delta x)^2]$ reaches the order of unity.[15] Physically, this also makes perfect sense: the stronger the spacetime curvature, the smaller the region in which special relativity remains a good approximation. So it is not about how small we make a region—it is about how small it needs to be for the approximation to hold.

The proposed definition of locality also clarifies the sense in which, locally, gravitational effects and gravitational energy may be treated as negligible.[16] First derivatives of the metric, and hence the connection coefficients, can be transformed away only at a point; away from that point, the connection generally reappears, with its leading behavior controlled by the second derivatives of the metric. When these second-order corrections are negligible, the region behaves,

[14]While there is outstanding controversy about the extent to which Minkwoski is 'special' locally, I don't engage with this here.

[15]This does not require the Christoffel symbols to vanish throughout the local region. They generally vanish only at the point of a local inertial frame. Rather, the point is that their accumulated effects remain negligible over the region, since $\Gamma \sim \partial g \sim (\partial^2 g)\,\delta x$, and $\Gamma\delta x \sim (\partial^2 g)\,(\delta x)^2$. I thank Henrique Gomes, Dennis Lehmkuhl, and Jingyi Wu for helpful comments pressing me to clarify my region-based definition of locality and its departure from a pointwise notion.

[16]The view that locally there is no gravitational effect and no gravitational energy is common in the literature (see Misner et al. (1973b, p.467), Trautman (1976, p.135–136), Goldberg (1980, p.469–470), Choquet-Bruhat (1983, p.399)), but Curiel (2009) and Duerr (2019a) argue that the standard appeal to the Equivalence Principle is too quick: it assumes that any localized gravitational energy would have to depend only on first derivatives of the metric.

to the required accuracy, as if it were Minkowskian. Correspondingly, gravitational energy is not represented at this order of approximation.

Now that we have fixed what we mean by local, we clarify what "local energy conservation" is. There are two ways to think about it. The first proceeds via the rigid-background approximation. We restrict attention to a region small enough (and a time-scale short enough) that the metric can be treated as effectively fixed and approximately Minkowskian throughout that region. This is not to say that the tangent space at a point is "Minkowskian-like."[17] Rather, the claim is that locally back-reaction and curvature-variation terms are suppressed and that the matter dynamics is, to a good approximation, invariant under (local) time translations. Under that idealization, Noether's First Theorem applies and yields a conserved energy current. Although any moving mass perturbs spacetime, the coupling is weak—spacetime is, in this sense, hard to curve. At the level of the local analysis, the reciprocal action of matter on geometry is therefore treated as higher order and ignored. This way of speaking aligns with Lam (2011): to obtain a standard conservation equation, one typically relies on a background structure with sufficient symmetry to underwrite time-translation invariance and the associated Noether charge.

The second interpretation appeals not to background rigidity but to freely falling frames. Along a timelike curve γ, one can introduce Fermi–normal coordinates such that $g_{\mu\nu}\big|_{\gamma} = \eta_{\mu\nu}\big|_{\gamma}$, and $\Gamma^{\nu}_{\mu\lambda}\big|_{\gamma} = 0$. Along γ, the vanishing Christoffel symbols ensure that Eq.(2.3) reduces to the ordinary conservation equation of matter, Eq.(2.4). As Duerr (2019a, p.4) remarks, "the energy-momentum 4-current along free-fall trajectories is locally conserved."

Is the along-the-geodesic interpretation of local conservation better than the rigid-background interpretation because the former avoids talk of a background? From the perspective of idealization, there is little reason to think so. The along-the-geodesic approach still has to implicitly invoke free-falling test particles—idealized particles with negligible mass and no self-gravity, which travel exactly along geodesics. But no physical system we study in relation

[17] See Gomes (2022) and Fletcher and Weatherall (2023) for the tangent-space interpretation of local flatness, although neither of them adopt it.

to energy conservation is a mere "test particle". In fact, the implicit role of the test particle in the along-the-geodesic interpretation has the same result as the role of the background in the other interpretation: both serve to block the reciprocal action between matter and spacetime based on *local* reasoning. In both cases, we assume that the local region of spacetime is, in some sense, dynamically *independent* of the physical system of interest—either by treating it as a rigid background or by treating the physical system as massless and hence incapable of changing spacetime. But they *are* different idealizations.

Based on these two interpretations, for generic spacetimes, *locally*, matter can be viewed as effectively "decoupled" from spacetime and hence matter effectively isolated and independently conserved. On the view developed here, locality itself should be understood in terms of approximation. To define a local region is precisely to neglect second-order derivatives of the metric. But it is those second-order terms that encode spacetime curvature, and the non-local features of the gravitational field.

2.2.3 Beyond Local Conservation

The difficulty of defining a global conservation of energy in a generic spacetime in GR stems from the difficulty of carving out a genuinely isolated system. In classical mechanics, conservation laws apply once isolation is imposed; in GR, that assumption is non-trivial. There are two kinds of isolation that one might hope to secure.[18] First, when spacetime is stationary, matter can be treated as *independently isolated*. Second, when spacetime is asymptotically flat, matter and the gravitational field may be treated as *collectively isolated*.

When Matter is Independently Isolated

A useful point of comparison is classical mechanics. Consider an idealized pendulum whose Lagrangian contains no explicit time dependence. By Noether's first theorem, the associated Hamiltonian is conserved along the system's trajectory. Note that this result is

[18] As Wallace (2017, p. 262) notes, "in realistic systems isolation is never complete". One has to appeal to idealizations and approximations.

based on the fact that the system's motion does not itself break the time-translation symmetry of the background in which it evolves. In effect, the system and its background are treated as independently isolated. When we appeal to time-translation symmetry to explain energy conservation in classical mechanics, we assume that the background remains fixed.

In GR, once we move beyond purely local considerations, the analogous move is available when spacetime itself is stationary.[19] In that case, spacetime admits a timelike Killing field, and one recovers an ordinary conservation equation for the associated matter-energy current. That is, if there exists a coordinate system in which the metric is time-independent (i.e., stationarity holds), then there exists a vector field ξ—a Killing vector field—such that $\mathscr{L}_\xi g_{\mu\nu} = 0$, where $\mathscr{L}_\xi$ is the Lie derivative along ξ. The corresponding conserved quantity along a geodesic with tangent u^α is $C = u^\alpha \xi_\alpha$.[20] Geometrically, this means that there exists a one-parameter group of isometries whose orbits are timelike curves, representing a global "time-translation symmetry" (Wald, 1984, p. 119). When this condition is satisfied, we may interpret the spacetime as satisfying a condition of *independent isolation*. Why does stationarity matter for isolation in this sense? The Einstein field equations describe how the metric evolves in response to matter. Yet, when the metric is time-independent, this coupling is effectively neutralized: matter and metric can be treated as if they were independent. No work is done by matter to alter the background geometry; no energy is exchanged in a way that would break time-translational symmetry. In effect, spacetime may be treated as a rigid background (cf. Lam (2011), Gomes and Rovelli (2024)).[21] Independent isolation, however, does not imply metaphysical independence. One may still hold the Machian idea that the metric depends on matter in a fundamental sense. The point is rather that, in a stationary spacetime, the matter distribution does not change with respect to

[19] Stationarity is sufficient but not necessary for ordinary matter conservation, as Fletcher (2025) notes. In vacuum spacetimes, Eq.(4) also holds.

[20] Let ξ^α be a Killing vector field and γ a geodesic with tangent u^α. Then $u^\beta \nabla_\beta (u^\alpha \xi_\alpha) = 0$ by the geodesic equation and the Killing equation, so $C = u^\alpha \xi_\alpha$ is conserved along γ. One can construct the Killing current $J^\mu := T^{\mu\nu}\xi_\nu$ and it satisfies an ordinary conservation equation $\partial_\mu(\sqrt{-g}\, T^{\mu\nu}\xi_\nu) = 0$, which can be turned into an integral version of the conservation equation (see e.g., Lam (2011, p.1016), Fletcher (2025, p.45)).

[21] The terminology in Lam (2011) differs from mine in a potentially confusing way, though the underlying idea is the same. For me, a fixed background means dynamical *independence* between matter and spacetime and hence matter alone is conserved; for Lam, matter conservation *depends* on a fixed background.

the relevant timelike Killing flow, and so neither does the metric.[22] The effective decoupling of matter and geometry, then, is a feature of *stationary stuff* in spacetime.

When Matter and Gravitational Field are Collectively Isolated

The kind of global energy conservation described above, when it is practically useful, holds only approximately. The familiar image of a body "moving along a geodesic" in Minkowski spacetime already relies on an idealization. Since a physical body cannot be a mere test particle, its motion perturbs the metric, however slightly, and thereby breaks the exact symmetry. Strictly speaking, the spacetime is no longer Minkowskian, and exact global conservation is lost. As Curiel (2000, p.47) puts it, "the smallest speck of dust, the tiniest bit out of place in only one spot in the entire spacetime, precludes the existence of a Killing field."

This is not the end of the story, however. A useful comparison can again be drawn in classical mechanics between an idealized pendulum and a realistic pendulum subject to dissipation. Energy conservation holds exactly for the former, but no perfectly isolated pendulum exists in the real world, so for a realistic pendulum, the total mechanical energy is only approximately conserved and the validity of the approximation depends on the timescale. (Over a single swing, dissipative effects are negligible; over longer timescales, however, the pendulum is observed to come to rest.) Once dissipative effects are included, the effective Lagrangian governing the pendulum involves explicit time dependence—or, more generally, fails to be invariant under time translations.[23] Energy is not conserved for the pendulum; the apparent loss reflects the fact that the pendulum is not an isolated system. Were we to enlarge the system to include the pendulum together with its environment—the surrounding air, the support, and the resulting thermal degrees of freedom—what appears as energy loss from the pendulum would be exactly balanced by energy gained elsewhere. The same applies to the failure of energy conservation

[22] In a stationary spacetime, the matter distribution can still change over time because matter need not be static. What is required is that the matter distribution does not change *along the timelike Killing flow*. Staticity is stronger, requiring the timelike Killing field to be hypersurface-orthogonal. Kerr is the standard example of a stationary but non-static spacetime: it has angular momentum and admits a conserved energy, even though it is not static. I thank Dennis Lehmkuhl for pointing this out.

[23] See Smith (2008) for a clear discussion of dissipative Lagrangians.

for radiative systems in electromagnetism: what holds is not conservation but balance, since the energy flux through any finite volume does not vanish.

In both dissipative and radiative cases, despite the absence of the time-translation symmetry associated with energy conservation via Noether's first theorem, we can still make sense of energy balance. I claim that the same structure of reasoning applies to the interaction between matter and spacetime, as in radiating spacetimes. In such cases, where stationarity fails, global conservation for the matter field fails as well. Nevertheless, one may still recover global balance by enlarging the system through the introduction of a hypothetical boundary of isolation. The underlying obstacle to conservation is again the absence of isolation. There is no gravitational analogue of an insulator: one cannot shield against tidal forces in the way a thermal insulator blocks heat flow. The only way to attenuate gravitational influence is to move sufficiently far from its sources. This is precisely the motivation for asymptotic flatness as a boundary condition—it provides the practical sense in which gravitational isolation can be approximated.

Although asymptotic flatness can be defined in a coordinate-independent way, for example via conformal compactification (Wald, 1984, p. 276–278), for present purposes, it suffices to require that a spacetime is asymptotically flat if there exists a coordinate system in which, at spatial or null infinity, the metric approaches the Minkowski metric, i.e., $g_{\mu\nu} = \eta_{\mu\nu} + \mathscr{O}(1/r)$ as $r \to \infty$, where r is a radial coordinate and the correction terms $\mathscr{O}(1/r)$ decay sufficiently fast to zero.[24] Call this the condition of *collective isolation*, which allows one to treat matter together with the gravitational field as a collectively isolated system.

This interpretation of isolation is widely endorsed. Penrose (1982, p.632) takes asymptotically flat spacetimes to "describe the gravitational fields of isolated systems" and argues that "it is only with asymptotic flatness that general relativity begins to relate in a clear way to many of the important aspects of the rest of physics, such as energy, momentum, and radiation, etc." Similarly, Wald (1984) and Wallace (2017) take asymptotic flatness to represent ideally isolated

[24]As Wald (1984, p.270) notes, this sort of definition is "adequate in many respects" but "difficult to work with" because of its coordinate dependence. I use it here to minimize the mathematical machinery needed to convey the underlying idea.

systems. Gomes and Rovelli (2024) describe asymptotic flatness as a condition of “dynamical isolation”.

Asymptotic flatness—under suitable differentiability assumptions—is also connected to the peeling behavior of the Weyl tensor, which makes precise the idea that radiative components dominate at null infinity.[25] The peeling property shows how the gravitational effects of black holes and stars in an otherwise empty universe “peel off” and almost vanish at large distances. In this sense, asymptotic flatness sets an idealized boundary of gravitational isolation.

Seen from this perspective, asymptotic flatness is a condition on the global structure of spacetime. In practice, however, it is often employed as an idealized boundary when modeling a subsystem of the universe: one treats the region of interest as effectively isolated, even though additional matter may exist elsewhere, so long as its influence is sufficiently weak to be observationally negligible. For example, in modeling the solar system one often imposes Minkowski boundary conditions at large distances, despite the presence of matter well beyond the solar system itself.

The asymptotic idealization underwrites the use of the post Minkowskian approximation in the far zone. Far from the source, one may write $g_{\mu\nu} = \eta_{\mu\nu} + h_{\mu\nu}$, $|h_{\mu\nu}| \ll 1$, where $h_{\mu\nu}$ is a small perturbation of the Minkowski background that vanishes at infinity. Under this weak-field condition, one can make precise the sense in which the ordinary conservation equation for matter is only approximate, or equivalently, the degree to which it is violated. Deviations from $\partial_\mu T^{\mu\nu} = 0$ (interpreted as small gains or losses of matter energy–momentum) are balanced by a stress–energy *pseudotensor* $t^{\mu\nu}$ that packages higher-order metric perturbations (e.g., terms quadratic in first derivatives of the metric perturbation). Accordingly, one can rewrite the exact Einstein field equations with the Ricci tensor linear in h as

$$^{(1)}R_{\mu\nu} - \frac{1}{2}\eta_{\mu\nu}{}^{(1)}R = 8\pi G(T_{\mu\nu} + t_{\mu\nu}), \tag{2.6}$$

[25] In the Newman–Penrose formalism, the Weyl scalars peel in an ordered hierarchy; the leading $1/r$ term (typically Ψ_4) encodes radiation. See Sachs (1962), Newman and Penrose (1962) and Gomes and Rovelli (2024) for a philosophical discussion.

where

$$t_{\mu\nu} = -\frac{1}{8\pi G}\left(R_{\mu\nu} - \frac{1}{2}g_{\mu\nu}R - {}^{(1)}R_{\mu\nu} + \frac{1}{2}\eta_{\mu\nu}{}^{(1)}R\right), \tag{2.7}$$

and ${}^{(n)}R_{\mu\nu}$ is the term in the Ricci tensor of order n in $h_{\mu\nu}$.[26] With a bit of algebra, it's straightforward to show that ${}^{(1)}R_{\mu\nu}$ obeys the linearized Bianchi identities and that

$$\partial_\mu(T^{\mu\nu} + t^{\mu\nu}) = 0. \tag{2.8}$$

Weinberg (1972) treats $t^{\mu\nu}$ as the energy-momentum of the gravitational field, even though $t^{\mu\nu}$ does not transform like a true tensor. It is common for gravitational-energy realists to interpret this equation as expressing the conservation of total energy-momentum: matter energy-momentum plus gravitational energy-momentum. This interpretive strategy, though not this particular definition of $t^{\mu\nu}$, has an Einsteinian precursor.

In his 1916 paper "The Foundation of the General Theory of Relativity", Einstein glossed an equation of the form given in Eq.(2.8) as showing that "the laws of conservation of momentum and energy are satisfied" for the totality of the energy components of matter and gravitational field.[27] The pseudotensor Einstein uses is not the same as the pseudotensor appearing in Eq.(2.7).[28] More generally, different pseudotensor definitions package the nonlinear gravitational terms in different ways, and they typically depend on different coordinate choices or auxiliary structures. The strategy, however, is the same: to rewrite the field equations so that matter and gravitational contributions together satisfy an *ordinary* conservation equation. In 1918 Einstein emphasized that this conservation claim applies to an isolated system—roughly,

[26]The Ricci tensor linear in h is defined as ${}^{(1)}R_{\mu\nu} = \frac{1}{2}(\partial_\mu\partial_\rho h^\rho_\nu - \partial_\mu\partial_\nu h^\kappa_\kappa - \partial^2 h_{\mu\nu} + \partial_\nu\partial_\rho h^\rho_\mu)$.

[27]This refers to Eq.(56) of his paper: $\frac{\partial(t^\sigma_\mu + T^\sigma_\mu)}{\partial x_\sigma} = 0$. Einstein writes, "It therefore follows from our field equations of gravitation that the conservation laws of momentum and energy are satisfied [...] here, however, in place of the energy components of the gravitational field, one must introduce the total energy components of matter and the gravitational field."
"Aus unseren Feldgleichungen der Gravitation geht also hervor, daß den Erhaltungssätzen des Impulses und der Energie Genüge geleistet ist. [...] nur hat man hier an Stelle der Energiekomponenten t^σ_μ des Gravitationsfeldes die Gesamtenergiekomponenten von Materie und Gravitationsfeld einzuführen." CPAE Vol.6, Doc. 30.

[28]I thank Dennis Lehmkuhl for pressing me to make this point explicit.

under asymptotic flatness (with a "Galilean coordinate" condition outside the isolated system).[29] On the view developed here, this restriction is important for conservation: in an asymptotic flat region, it is natural to treat $T_{\mu\nu}$ together with $t_{\mu\nu}$ as describing an effectively closed system, and thus to speak of conservation.

Einstein's view on energy conservation was not universally shared even among sympathetic contemporaries. Felix Klein, working in close dialogue with David Hilbert and Emmy Noether, emphasized that Eq.(2.8) arises from identities and superpotential constructions and doubted that the coordinate-dependent pseudotensors admit a straightforward interpretation as the energy of the gravitational field.[30] We will turn to the problems with the pseudotensor in the next section; before doing so, however, I should clarify that for *radiative* systems, Eq.(2.8) is better understood as a balance relation.

The "balancing" term $t^{\mu\nu}$ provides the natural handle on gravitational radiation—an effect that enters only at higher orders in perturbative treatments. This dovetails with the local picture: in weak-field expansions, radiative contributions appear precisely at the order where the matter-only conservation $\partial_\mu T^{\mu\nu} = 0$ ceases to hold. When a system emits gravitational waves, energy does not simply vanish, but is transferred outward and can be absorbed elsewhere.

Why is Eq.(2.8) a conservation equation for asymptotically flat systems but a balance equation for radiative systems? On the view defended here, this is because $t^{\mu\nu}$ contains both a density-like piece (a "field energy" contribution inside the volume) and a flux-like piece (radiative transport through the boundary). If one takes the electromagnetic analogy seriously, $t^{\mu\nu}$ plays a role analogous to both u (field energy density) and $\mathbf{S}$ (field energy flux) in Eq.(2.1). For

[29] Einstein writes, "In order to be able to talk meaningfully of energy-momentum of a system, it is necessary that the density of the energy-momentum vanish outside of a certain domain B. In general, this is the case only if the $g_{\mu\nu}$ are constant outside of B; that is, if the system under consideration is embedded in a 'Galilean space,' and we use 'Galilean coordinates' to describe its surroundings."
"Damit wir von der Energie bzw. dem Impuls eines Systemes reden können, muß außerhalb eines gewissen Bereiches B die Dichte der Energie und des Impulses verschwinden. Dies wird im allgemeinen nur dann der Fall sein, wenn außerhalb B die $g_{\mu\nu}$ konstant sind, d. h. wenn das betrachtete System in einen 'Galileischen Raum' eingebettet ist, und wir zur Beschreibung der Umgebung des Systems uns 'Galileischer Koordinaten' bedienen." CPAE Vol.7, Doc.9.

[30] For Klein's critique and its connection to Hilbert's and Noether's analyses, see De Haro (2022).

asymptotically flat systems, the flux through infinity vanishes (or is suitably controlled), and the balance relation reduces to a conservation statement for the closed system. For radiative systems, by contrast, the flux does not vanish, and Eq.(2.8) is best understood as a balance relation. We may thus specify

> **Energy Balance in GR.** An energy-balance statement in GR relates the change of matter energy–momentum within a region to balancing terms associated with the gravitational field, including both reversible-transfer and radiative contributions. With an additional isolation condition—typically asymptotic flatness—the balance equation yields a conservation equation for the total system.

The contribution from "reversible transfer", following Poisson and Will (2014), refers to field energy (in the near zone) that exchanges reversibly with matter. In electromagnetism, as Poisson and Will (2014, p.632) observe, "an ambiguity arises because it is impossible to decompose the field energy into a piece that is unambiguously associated with the reversible transfer, and another piece that is unambiguously associated with the radiation." In the near zone, energy exchange between field and charges cannot be separated from radiative transfer, but in the wave zone radiative transfer becomes unambiguous, corresponding to irreversible damping. A similar situation holds for radiative systems in GR. In the near zone, no clean separation can be drawn between energy attributed to matter and energy attributed to the gravitational field. However, this ambiguity dissolves in the wave zone, where $t^{\mu\nu}$ captures something *irreversible* and *unambiguous* (cf. Gomes and Rovelli (2024)).

In this section I have distinguished between energy conservation and energy balance, and shown how the distinction applies in GR. Roughly speaking, matter energy conservation holds locally, given my proposed notion of locality. It also holds globally when matter is *independently isolated*, that is, when spacetime admits a timelike Killing vector field. By contrast, energy balance supports the claim that when matter energy conservation fails, energy does not simply disappear: it is balanced by the gravitational contribution. Balance is not, by itself, a conservation law. But if one can treat the total system as collectively isolated—for example, by imposing asymptotic flatness and suitable falloff conditions—it can be promoted to a conservation statement for the combined matter-plus-gravitational system.

2.3 Why Not a Perpetual Motion Machine?

Doubts about the pseudotensor place a question mark over the very idea of gravitational energy.[31] There is no generally covariant expression for gravitational energy, so for any given pseudotensor one can always choose coordinates in which it vanishes. Although many quantities with clear physical significance in GR—Christoffel symbols, for example—are also coordinate-dependent, pseudotensors seem worse: unlike the Christoffel symbols, they do not transform as geometric objects (Duerr (2021)). A further concern is non-uniqueness: there are infinitely many gravitational energy–momentum pseudotensors, with no clear grounds for privileging any one of them.

These worries feed directly into one of the longest-running debates in GR: do gravitational waves carry energy away from the systems that emit them? The most influential argument in favor of gravitational energy is the Bondi-Feynman sticky bead thought experiment, first presented at the 1957 Chapel Hill Conference, held at a time when the existence of gravitational waves remained unsettled.[32] The scenario is simple. Imagine a rod with freely sliding beads. As a gravitational wave passes through, the beads oscillate, generating heat through friction. But heat requires energy, and that energy must come from somewhere. The natural conclusion is that the system loses energy to gravitational radiation, which does work on sticky beads and produces heat. And since heat is an objective physical effect, this energy loss cannot be dismissed as a mere coordinate artifact—otherwise, no real energy transfer would occur.

[31] These worries can be found in e.g., Ehlers (1980), Hoefer (2000), Curiel (2009), Duerr (2019a), Duerr (2019b), and Duerr (2021).

[32] Although the thought experiment is often attributed to Bondi (1957)—who was the first to suggest, in print, using gravitational radiation to rub something against a rigid frictional device—it was Richard Feynman who pressed the point more forcefully during the conference. Bondi was initially more sympathetic to the skeptic position held by Infeld and Scheidegger (1951) and **?**. Though later, drawing on Felix Pirani's proposal to characterize gravitational waves invariantly in terms of geodesic deviation, Bondi raised, during the conference, the question of whether one could construct "an absorber for gravitational energy...to learn what part of the Riemann tensor would be the energy-producing one" (DeWitt and DeWitt (1957, p.62)). See also DeWitt and Rickles (2017).

While the physics community largely converged on the view that gravitational waves carry energy by the late twentieth century,[33] the philosophical debate persists. One influential line of resistance is articulated by Hoefer (2000, p.196):

> "...it is possible to view energy loss by a gravity wave source as genuine loss, without insisting that the energy is still around somewhere...Similarly, energy gain in a gravity wave detector could be thought of as genuine gain, without our having to say that the energy existed somewhere beforehand."

On this view, energy in GR simply comes and goes. Energy conservation fails, here and there. This stance is also taken by Duerr (2015, p.16):

> "From this [sticky bead setup] we inferred the energy transfer from the [gravitational waves], so as to restore the energy balance. However, in GR energy-momentum is no longer conserved in general. Recall that conservation laws are associated via Noether's Theorem with spatiotemporal symmetries... Generic spacetimes, such as ones with [gravitational waves], lack these symmetries."

It is true that radiative spacetimes lack the symmetries required for Noether–First conservation laws. But that fact alone does not settle the question at issue here. As emphasized in Section 2.2, the absence of symmetries blocks ordinary matter conservation—owing to the failure of *independent isolation*—without precluding a balance relation between matter and gravitation. If anything, it is precisely the lack of such symmetries that motivates an energy-balance story: balance is meant for cases where symmetry-based conservation is unavailable.

For eliminativists, however, there is no gravitational energy, so once ordinary matter conservation fails, there is no further balance story to tell. Their objection to energy balance may be summarized as follows:

> **Premise 1.** If gravitational energy lacks property X, then it is not real.
>
> **Premise 2.** If gravitational energy is not real, then there is no energy balance between matter and the gravitational field.

[33] Physicists might disagree on exactly when the issue was settled. Many believe that the decisive moment came in the 1960s with a series of papers by Bondi et al. (1962), which introduced the news function and demonstrated a monotonic decrease in the system's mass whenever there was "flow of information" or "news" via gravitational waves. However, others think debates continued into the 1970s and 1980s surrounding the so-called quadrupole formula controversy. See Kennefick (1996) for a historical review.

Here, property X may refer to gauge invariance, general covariance, uniqueness, geometrical character, or localizability—features that both sides can agree gravitational energy lacks. The eliminativist therefore concludes that there is no energy balance in GR.

Existing replies to eliminativism have largely focused on rejecting Premise 1. For example, Pitts (2010), Read (2020), and Read (2022) argue that neither non-covariance nor non-uniqueness undermines gravitational energy. Pitts (2010) maintains that the relevant coordinate dependence is not vicious and appeals to Noether's theorems: if the theory's symmetries generate infinitely many conserved gravitational energies, then GR underwrites a corresponding plurality of conservation laws. Read (2020) emphasizes that gravitational energy earns its keep by playing the functional role of energy. Read (2022) argues that even non-geometric objects can carry physical content, in a perspectivalist sense, despite failing to transform as tensorial fields.

The aim of this section is different. I offer instead a reductio ad absurdum: if there were no energy balance at all in GR, then gravitational perpetual motion machines would be possible. The argument focuses on the role that energy balance plays in constraining physically possible processes in GR, in particular in excluding pathological scenarios. I argue that even for those who remain skeptical of gravitational energy pseudotensors, there is nevertheless strong reason to retain energy balance.

2.3.1 Why Retain Energy Balance?

An early and compelling story of energy balance in GR was told by Ashtekar and Magnon-Ashtekar (1979). It is demonstrated that if the total 4-momentum carried by gravitational waves emitted up to some retarded instant is finite, then an asymptotic energy-momentum balance holds for any retarded time. This balance specifically relates two distinct notions of energy in GR: the ADM energy and the Bondi energy.[34]

ADM energy, introduced by Arnowitt, Deser, and Misner, is defined within the Hamiltonian formulation and makes essential use of asymptotic symmetries. The idea is to consider

[34] See Ashtekar and Speziale (2024) for a review of ADM and Bondi energy.

a spacelike hypersurface with asymptotically flat initial data and to construct a corresponding phase space. We can then identify the ADM 4-momentum as the generator of asymptotic spacetime translations at spatial infinity i^0. The ADM energy E_{ADM} then corresponds to the time component of this 4-momentum. It remains constant at asymptotic spatial infinity. Bondi energy E_{Bondi}, however, is the time component of the Bondi-Sachs-4-momentum evaluated at null infinity $\mathscr{I}^+$. Unlike ADM energy, Bondi energy is dynamical—it decreases as gravitational waves radiate energy E_{Rad} away from the system. Hence, E_{Bondi} represents the time-dependent energy remaining in the system after accounting for the energy transferred away. The asymptotic energy-balance equation of Ashtekar and Magnon-Ashtekar (1979) makes this precise:

$$E_{\text{ADM}} = E_{\text{Bondi}}(\mathscr{S}) + E_{\text{Rad}}(\mathscr{S}), \tag{2.9}$$

which holds for any cross section $\mathscr{S} \subset \mathscr{I}^+$, i.e. for any retarded time. Here $E_{\text{Rad}}(\mathscr{S})$ is the total energy radiated through $\mathscr{I}^+$ between the early-time end of $\mathscr{I}^+$ (i.e., approaching i^0 along $\mathscr{I}^+$) and the cut $\mathscr{S}$; it is defined by integrating the energy flux over that portion of $\mathscr{I}^+$. For an isolated system that satisfies the relevant asymptotic conditions, Ashtekar and Magnon-Ashtekar (1979) show that an asymptotic energy–momentum balance holds for any retarded time (provided the Bondi news satisfies a natural falloff condition).[35] In other words, the difference between E_{ADM} and E_{Bondi} is exactly the energy transferred away by gravitational radiation up to $\mathscr{S}$.

What matters for present purposes is the structure of the asymptotic energy-balance equation and its physical upshot. If ADM energy, Bondi energy, and radiative energy are all non-negative, then the balance equation yields a substantive constraint: *the total amount of energy that a system could possibly emit through gravitational radiation could never exceed its initial ADM energy*. That is, one cannot radiate more energy than one initially has.[36]

[35]The falloff condition is used to control the limit toward the early-time end of $\mathscr{I}^+$: it ensures that no additional boundary contribution survives there. See remarks in Ashtekar and Magnon-Ashtekar (1979, p.183).

[36]Establishing this constraint was non-trivial. Although positivity of ADM energy and radiative energy had been shown earlier, positivity of Bondi energy remained open. Doubts expressed by Ehlers (1980, p. 279) about the energy-balance argument prompted independent proofs by Horowitz and Perry (1982), Schoen and Yau (1982),

The idea behind this constraint is intuitive. Radiation serves as a decay mode, meaning it cannot "overshoot" and extract more energy from the system than is initially available. Crucially, this reasoning relies on the assumption of *isolation*, implemented mathematically by asymptotic flatness. If incoming gravitational waves from past null infinity inject energy into the system, then we might not run out of fuel. Interestingly, early analyses of gravitational radiation by Hu (1945) and Peres (1959) appeared to suggest that binary systems could gain energy by emitting gravitational waves, a paradox later traced to inappropriate boundary conditions.[37]

By contrast, on an eliminativist view according to which energy conservation simply fails in GR, one may treat energy loss at the source as a "genuine loss" and energy gain at a detector as a "genuine gain", without supplying any causal account linking the two. What such a view lacks is precisely the constraint just described. If energy balance is abandoned altogether, there is no principled upper bound on the amount of energy that can be extracted from a gravitational system.

One might respond that, since gravitational energy is ill-defined, the question of whether we can run out of it is also ill-posed. But the issue can be framed without invoking any dubious ontology: suppose a sticky-bead apparatus is placed far from a binary emitting gravitational waves (with boundary conditions chosen so that no additional energy enters the system), will the beads oscillate forever? This is a well-posed and coordinate-independent question.

The beads will eventually come to rest. By ruling out unbounded energy extraction, the energy balance argument excludes gravitational perpetual motion machines.[38] If, by contrast, energy is simply allowed to disappear at the source and appear at the detector, nothing prevents the beads from oscillating indefinitely. Worse, absent any balance story there is no explanation of why gravitational-wave incidents on Earth do not deposit arbitrarily large amounts of energy

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Ludvigsen and Vickers (1982), and Reula and Tod (1984).

[37] See Kennefick (2016, Ch.7) for the historical development.

[38] One might still wonder, where has the energy gone? In this setup, it's left in the past (at null infinity). In thermodynamics we can distinguish between two kinds of perpetual motion machines: idealized, perfectly reversible systems (such as an ideal pendulum or a reversible Carnot engine), which saturate but do not violate a bound, and machines that genuinely violate a constraint. The gravitational analogue here is posed to be of the latter, more problematic kind.

and wreak catastrophic effects. A balance relation fixes the upper bound of energy that can be extracted gravitationally.

2.3.2 How to Retain Energy Balance?

If gravitational perpetual motion machines are to be excluded, then skeptics of gravitational energy face a choice: they must reject either Premise 1 or Premise 2. For completeness, I briefly sktch a few non-exhaustive options.

To reject Premise 1, namely, the inference from the lack of property X to non-reality, one may treat the non-tensorial and non-unique character of gravitational energy as merely reflecting *the absence of any preferred localization of gravitational energy*. Gravitational waves act through non-local tidal effects. Although those tidal effects admit a generally covariant description via geodesic deviation, gravitational radiation is typically captured only after one introduces a separation of scales and an averaging procedure over spacetime regions large compared to the wavelength, washing out the small-scale oscillations—just as in the standard treatment of electromagnetic radiation.[39] In that regime, the relevant radiative quantities are gauge-invariant (or, more carefully, invariant up to controlled errors set by the scale separation), and the apparent "pathologies" of localization are better seen as artifacts of insisting on a uniquely local energy density where the physics only supports a scale-relative, coarse-grained notion. On this view, the absence of any preferred localization is not a reason to reject gravitational energy *tout court*. It is a reason to invoke it where weak-field or radiative conditions justify it.

Disagreement about Premise 1 leads to a stalemate of what counts as real—whether reality requires invariance in a strong local sense.[40] Suppose one does demand such local invariance, and so denies that gravitational energy is real. Even then, Premise 2 can be resisted. One can retain the balance equations without interpreting the pseudotensor in Eq.(2.8) or the radiative energy Eq.(2.9) as representing energy carried by a gravitational field. Instead, they can

[39] A version of this averaging procedure was introduced by Brill and Hartle (1964) and Isaacson (1968); see also, for example, Weinberg (1972, Ch. 10.3) and Ciufolini et al. (2001, Ch. 5).

[40] See, e.g., Nozick (2001, Ch. 2) and Vollmer (2010) for invariance-based proposals.

be read as representing the *maximal work* that gravitational waves can potentially do via tidal effects. Gravitational energy, as represented by $t^{\mu\nu}$, is ambiguous and coordinate-dependent, but when a physical situation *induces a localization* (e.g., when detectors extract energy from gravitational waves), the system *does work*.

This idea is already present in Feynman's own way of framing the issue. In a 1961 letter to Victor ("Viki") Weisskopf, Feynman recalls his sticky-bead argument at the Chapel Hill Conference and reflects on the reaction it received:

> I was surprised to find a whole day at the conference devoted to this question, and that "experts" were confused. That is what comes from looking for conserved energy tensors, etc., instead of asking "can the waves do work?"[41]

Focusing on work resolves the familiar worries about non-uniqueness and non-covariance. As Feynman remarks in discussing Poynting's theorem, one can "juggle the terms around" to produce infinitely many pairs (u, $\mathbf{S}$) (energy density and flux) that satisfy the same balance equation (Eq.(1)); neither the formalism nor experiment fixes a unique localization of electromagnetic field energy.[42] In practice one adopts the simplest expressions, since the ambiguity does not affect the physically relevant claims about energy transfer and work.

In GR, for a quasi-isolated body subject to external tidal fields,[43] Favata (2001) shows, different gravitational pseudotensors assign different values to the *interaction energy* between the body and the external field, but they agree on the rate at which (tidal) *work is done on the body*. The ambiguity in the mass–energy of a quasi-isolated system therefore does not attach to the *work* done. Rather, it resides entirely in how the gravitational interaction energy is partitioned between body and field (Thorne, 1998; Favata, 2001).[44]

On the view developed here, energy balance is not underwritten by the ontology of

[41] Feynman Papers, California Institute of Technology, box 29, folder 14.

[42] "Field Energy and Field Momentum," The Feynman Lectures on Physics.

[43] The literature typically speaks of an "isolated body" in this context (Thorne and Hartle (1985), Zhang (1985), Thorne (1998), Purdue (1999), and Favata (2001)). Since such bodies nonetheless exchange energy with an external field, and to avoid confusion with the notion of isolation introduced in the previous section, I use the term *quasi-isolated* here.

[44] This same ambiguity already arises in Newtonian gravity; see Purdue (1999).

gravitational energy, but by a constraint on what gravitational dynamics can *do*. Einstein himself seems to have taken a similar stance. When confronted with "differences of opinion on whether or not the t_σ^ν have to be interpreted as the energy components of the gravitational field," he dismisses the dispute as "irrelevant, as a mere question of words." What mattered, in his view, is that Eq.(8) "provides the facilitations of perspective which constitutes the value of the conservation."[45]

The proposed position also has an interesting historical precedent. In classical mechanics, potential energy was not always seen as "real" energy. It became what Leibniz, Huygens, and Carnot called *vis viva*—a real, living force—*only when converted into another form*, such as kinetic energy. Absent such conversion, it remained "potential," latent, or even fictional.[46] Likewise, skeptics may hold that gravitational energy becomes *real only upon conversion*, without discarding energy balance altogether—since it is precisely the balance relation that tracks how much such conversion can occur.

2.3.3 Potential Objections

I will address three potential concerns about the account of energy balance defended here. The first worry is that our world is not, in fact, asymptotically flat, so that neither a global conservation equation nor a balance story applies. Eliminativists may accept energy balance in asymptotically flat spacetimes while maintaining that no balance story is available in generic spacetimes—or in the spacetime we actually inhabit.

This objection, however, relies on an overly literal reading of the asymptotic condition. In most physically relevant applications, asymptotic flatness functions as an idealization within an approximation scheme. Cosmological expansion becomes significant only on scales of order megaparsecs, whereas most astrophysical systems of interest—binary inspirals, stellar dynamics, gravitational-wave sources—are modeled on much smaller scales, where the influence of cosmic expansion is observationally negligible. One may raise the same problem in thermodynamics,

[45] CPAE Vol.7, Doc.1.

[46] See Hecht (2003) for a historical account.

too: no real system is perfectly isolated, and no real engine is perfectly Carnot-efficient. Yet we still appeal to conservation reasoning and balance equations, because isolation holds to a good approximation over the relevant length- and time-scales, and the balance relation provides a principled bound.

A central contribution of this paper is to motivate an explicitly scale-sensitive understanding of conservation and balance, and to interpret apparent failures of conservation as failures of isolation at the relevant scale. Philosophers of physics often bracket approximation methods in foundational debates, but without them we would lose the main route by which the theory connects to empirical applications and observation.

The second worry is that the balance story for gravitational radiation is ultimately grounded in approximation methods developed in linearized theory. In the full nonlinear theory, it is not immediately obvious that the energy transferred away to infinity by gravitational waves must be balanced by a corresponding loss of energy by the source.

Historically, this concern fueled a genuine controversy—especially in the 1970s and early 1980s—about whether one could reliably use the "energy balance argument" to determine radiation reaction for self-gravitating systems. The modern resolution is that the relevant nonlinear approximation framework treats the near zone and the wave zone by different expansions and then matches them consistently. In particular, for compact binaries, one can compute the near-zone dynamics (including radiation-reaction terms) and independently compute the far-zone flux; Blanchet (1997) shows that these calculations are consistent, vindicating the energy-balance relation (with the near-zone metric computed up to 3.5 Post-Newtonian order).

The third worry is that it is ad hoc to redefine the total energy of a system merely in order to recover a desired energy-balance relation.

This worry, however, rests on a mistaken picture of how energy is defined in physical theories. Energy is not fixed a priori; it is determined by the dynamics of the system under consideration. Consider again the idealized pendulum. In that case, the total energy is given by the Hamiltonian, the sum of kinetic and potential energy. For a realistic pendulum, however, this definition must be modified to accommodate dissipative effects. The additional terms—representing friction or air resistance—are not fundamental, but they have clear explanatory power: they account for the fact that real pendulums do not swing indefinitely. In this respect, the "redefinition" of total energy is no more ad hoc than the physics that motivates it.

In GR, the situation is even more complicated. There is no single definition of mass-energy. Instead, we have a plurality of notions—ADM energy, Bondi energy, Komar energy, quasi-local energy—none of them simply *the* correct one, but each more or less apt for different regimes and purposes (see Szabados (2009) for a survey). GR itself offers no built-in definition of what it means to be isolated, and the resulting underdetermination of mass-energy definitions reflects this absence. As Geroch (1977, p.4) remarks:

> "The theory is, at least in principle, as viable, as self-contained, as predictive without such a notion [of isolated system] as with it. A definition is sought, rather, for its convenience, for we choose to understand the Universe through analysis of smaller, simpler systems, one at a time. There are no 'correct' or 'incorrect' definitions, only more or less useful ones."

On this view, both isolation and energy must be defined pragmatically. A good definition of energy is typically one that, under suitable boundary or isolation conditions, is conserved in some qualified sense. It may seem ad hoc, or even question-begging, to treat conservation under specific conditions as a heuristic for a good definition of energy. But this strategy has proven remarkably fruitful, yielding conceptual and mathematical frameworks that not only clarify classical gravitational dynamics but also offer potential insights into energy in quantum gravity. There is, in short, a need to "make up a story" suited to the systems we aim to understand.

Both realists and eliminitivists agree that generic spacetimes lack spatiotemporal symmetries and therefore admit no conservation laws of the Noether–First type. The remaining question, then, is whether anything is left to constrain the dynamics, and whether we can still say something instructive about energy exchange and energy transfer. In other words, does something else play a role analogous to conservation? I think the answer is yes: energy balance. Energy balance helps us to explain otherwise opaque non-linear dynamical behaviors and implies that the maximum energy a system can emit in gravitational waves cannot exceed its total ADM energy. No comparable inference would be available if energy conservation in GR were treated as simply violated.

2.4 Conclusion

The idea that fundamental substances can neither be created nor destroyed traces back at least to the pre-Socratics. It is a compelling Parmenidean principle: *ex nihilo nihil fit*—nothing comes from nothing. And by the same token, nothing simply vanishes. When *vis viva* appeared to be lost in classical mechanics, physicists introduced friction and potential energy to account for the missing energy.

Skeptics of potential energy, such as John W.N. Sullivan (1933/1949, p.155) were quick to dismiss it as a convenient fiction: "the notion of potential energy explains away apparent violations of the principle of the conservation of energy. But is not this the very reason for the importation of the notion of potential energy? Is it not a mathematical fiction, brought in for convenience?" And yet, the concept endured and ultimately prevailed. With the advent of special relativity, the debate was settled: the total energy of a free particle is the sum of its rest energy and kinetic energy. Perhaps something similar holds for gravitational energy. We may not yet have a fully satisfactory, "fundamental" account—perhaps that will have to wait for a theory of quantum gravity, but in the meantime, energy balance serves as a heuristic. It helps us identify boundary conditions, interpret radiative phenomena, and rule out perpetual motion machines.

Within the framework of classical GR, I've argued that energy conservation holds under the right conditions and there are physical constraints on how much work gravitational waves can deliver. To clarify these conditions, I have distinguished energy conservation from energy balance and motivated a notion of *locality*. The broader lesson is that energy in GR can be defined approximately. In particular, whether it is meaningful to invoke energy conservation or energy balance depends on 1) *when* and *where* we can treat matter and spacetime as *independently* *isolated*, or 2) *when* and *where* we can treat the system of interest as *collectively isolated*. The contextuality is a feature, not a bug. It is a consequence of the fact that matter interacts with spacetime. The very interaction described by the Einstein field equations makes defining isolated systems difficult. Nevertheless, balance holds even when conservation fails, allowing us to make meaningful inferences order by order and explain the dynamics of such interactions.

Finally, why not a gravitational perpetual motion machine? One the one hand, the answer appeals to the asymptotic energy-balance equation, which ensures that one cannot extract infinite amount of energy from a gravitationally isolated system. One the other hand, there shouldn't be. The idea that "no gravitational perpetual motion machine is possible" has, in many ways, functioned as a heuristic guiding constraint in the search for meaningful definitions of isolation and mass-energy in GR. It's less a derived result than a normative expectation that shapes what we're looking for, even before the mathematics delivers a definitive answer. This heuristic has not been idle. For instance, the derivation of gravitational redshift—later confirmed in the Rebka-Pound experiment—relies on the assumption that perpetual motion by gravitational means is impossible. Is this putting the cart before the horse? GR offers no a priori definition of mass-energy, and yet the insistence that some form of energy balance has led to fruitful results: it motivates boundary conditions, informs candidate energy definitions, and helps rule out unphysical models. In this case, the heuristic has earned its keep by guiding us toward deeper understanding.

Acknowledgments

Chapter 2, in part, is currently being prepared for submission for publication. The dissertation author was the sole author of this paper.

Chapter 3

Patchwork Gravitation

3.1 Introduction

In *Gravitation*, one of the most influential textbooks on General Relativity (GR), Misner, Thorne, and Wheeler (1973) open their chapter on weak field approximations with a telling epigraph from *Tao Te Ching* by the ancient Chinese philosopher Laozi: "The way that can be walked is not the perfect way. The word that can be said is not the perfect word."[1] The epigraph neatly captures an interesting tension in GR between the theory's conceptual beauty in its geometric formulation and the compromises required to render it analytically tractable in practice.

GR exhibits a sharp contrast between its formal elegance and its "fragmented" use in concrete applications. Formulated in the language of differential geometry, GR brings physical concepts into close alignment with geometrical objects: spacetime is represented by a smooth manifold equipped with a Lorentzian metric, matter is represented by tensor fields, and physical equivalence is expressed through the invariance of geometrical structures under appropriate smooth maps. At the center of the theory lies Einstein's field equation, which Penrose (2002, p.50) describes as "of unprecedented elegance and geometric simplicity." For an ideal reasoner, say, an Einstein–Laplace demon with infinite computational capacity and complete information, the world described by GR might well appear as having the "perfect way": its global spacetime

[1]"道可道，非常道。名可名，非常名。" – 老子《道德经》

structure transparent, its metric and dynamics exactly specified. From the top down, GR is an ideal theory for an ideal reasoner.

But for us, "the way that can be walked" is typically the way of approximation. As Thorne and Hartle (1985, p.1816) pointed out, "in dealing with the problem of motion in a form that makes contact with external observations, there can be no discussion without approximation". GR was historically confirmed through the use of approximation methods, and in most physically relevant cases it continues to make empirical contact through such methods. In practice, we often work with a mosaic of locally or regionally valid models: Schwarzschild here but Minkowski there, Friedmann–Lemaître–Robertson–Walker on cosmic scales, and linearized gravity for weak fields and asymptotically flat systems. What is available to us is not a single, fully surveyed, God's-eye description of *the manifold* of the world, but a collection of descriptions held together by coordinate conventions, boundary conditions, and approximation schemes.

Two especially important approximation methods developed in the twentieth century are the post-Newtonian and post-Minkowskian approximations. In relation to one of the most debated topics in the history of GR—the existence of gravitational radiation—the question arose: which approximation should we trust? At low orders, post-Newtonian methods treat a binary system much like a corrected Newtonian system: the bodies orbit without damping, so their orbital energy remains conserved rather than being carried away by gravitational waves. Post-Minkowskian methods, by contrast, are naturally suited to wave propagation and, by analogy with electromagnetism, yield gravitational radiation. In the end, the solution did not come simply from refining either camp's methods, though both were substantially improved, but from combining them.

As early as the mid-twentieth century, Soviet physicist Vladimir Fock recognized that different approximation schemes ought to be applied in different regions and then matched at their boundaries. For various reasons, Fock's proposal did not become immediately popular. His insistence on harmonic gauge was seen by some as clashing with the spirit of general covariance; and his Marxist philosophical leanings may have made his work less palatable in the West during the Cold War era. Yet the basic idea—that of matched post-Newtonian and post-Minkowski patches—proved prescient. By the 1980s and 1990s, hybrid approaches such as the Blanchet–Damour method and the DIRE (Direct Integration of the Relaxed Einstein equations) formalism systematically implemented this strategy: post-Newtonian expansions in the near zone, post-Minkowski expansions in the far zone, with rigorous matching techniques.

In this paper, I examine one important sense in which the predictive success of GR depends on pluralistic, regime-sensitive, or "patch-stitching" methods of the kind Fock anticipated. I argue that this strategy is not just a response to calculational difficulty, but is rooted in the mathematical character of Einstein's field equations (EFE). Near a slow-moving source, GR can often be treated in a "quasi-elliptic" or "Poisson-like" way: retardation effects are negligible, and the field is approximated by a sequence of quasi-instantaneous gravitational potentials. This is the regime in which post-Newtonian methods are especially useful. Far from the source, by contrast, the "hyperbolic" or "wave" character of GR becomes essential. Finite propagation time of gravitation can no longer be ignored, and the radiative degrees of freedom must be treated as genuinely propagating disturbances. This is the regime in which post-Minkowskian methods become especially useful. The need to move between these regimes is precisely why a patchwork strategy is required in certain contexts.

I bring this practice into contact with Nancy Cartwright's patchwork picture of science, which emphasizes the role of non-fundamental laws, idealizations, and approximations tailored to specific situations. In Cartwright (1983)'s terms, "No single model serves all purposes best". The post-Newtonian and post-Minkowskian expansions provide a useful case in point. Each is a controlled approximation scheme, but each is valid only under specific assumptions and within a limited physical regime. Although such methods are central to the actual practice of relativistic physics, they have received comparatively little attention in the philosophical literature. Their structure and limitations suggest that even a theory as unified and geometrically elegant as GR often achieves empirical success through a plurality of models, approximations, and matching procedures.

The structure of the paper is as follows. Section 3.2 begins with a brief history of the problem of motion in GR. It explains why approximation methods became indispensable, and then turns to the Fock–Infeld dispute, situating it against the background of earlier post-Newtonian work. Section 3.3 compares two distinct approximation methods, post-Newtonian and post-Minkowskian approximation, relating their different domains of applicability to the mathematical character of the EFE. It argues that Fock's emphasis on harmonic coordinates and boundary conditions is best understood as part of a broader patchwork strategy. Section 3.4 draws out the philosophical moral of this strategy by connecting it to Cartwright's account of theory-entry, model pluralism, and the patchwork of laws. Section 3.5 concludes.

3.2 A Path through Approximations: A Brief History

3.2.1 The Problem of Motion

One of the most important roles of any theory of gravity is to address the so-called problem of motion: the problem of describing, as accurately as possible, the dynamics of gravitationally interacting extended bodies. The problem of motion in GR concerns the question of whether the equations of motion for matter can be derived from the EFE themselves, rather than being added as independent laws. Einstein's ambition for his field equations was bold: the motions that particles are permitted to make (the so-called geodesic equations) are, in principle, completely determined by the field equations themselves (Einstein and Grommer, 1927). That is unusual. Most other field theories require a division of labor between the field equations and a separate force law.

For example, in electromagnetism, Maxwell's equations tell us what electromagnetic field a distribution of charges and currents generates, and what force a test charge will feel in that field. But they do not, by themselves, provide a complete dynamics for charged matter. To determine how a charged particle moves, one must also invoke the Lorentz force law, together with the appropriate equation of motion. Only then is the story closed, since the resulting motion of the charge in turn affects the electromagnetic field. Newtonian gravitation works similarly. In its formulation as Poisson's equation, the theory provides the configuration of gravitational potential sourced by matter in a Euclidean background. But Poisson's equation is elliptic, i.e., with no second-order time derivatives; it does not, by itself, describe temporal evolution. Again, Newton's second law must be called upon to say how bodies move in response to that potential. By contrast, Einstein and Grommer (1927) thought that GR was different in kind. Its field equations do more: they not only determine the configuration of the gravitational field, but also fix the allowed motions of particles within it, i.e., the geodesic equation. That, they thought, was a distinctive virtue of the theory.

This conceptual elegance, however, comes at a price: the perfect way crafted by the theory is not the way that can simply be walked. In practice, solving the problem of motion in GR is notoriously difficult. Unlike in electromagnetism, where one can stipulate the motion of charges and then compute the resulting radiation, GR allows no such *natural* division of labor. The motion of gravitating bodies must already satisfy the field equations, which themselves depend on that very motion. As Kennefick (2007, p. 155) remarks, it seems "as if we must know beforehand how the bodies are moving in order to figure out how they move!"

For a Laplace-Einstein demon with perfect information about the motions of all bodies, this is not a problem: the field equations would simply provide a consistency check. For us, however, progress in generic, dynamically interacting spacetimes requires approximation. Exact solutions are available only under special assumptions, such as cases with high symmetry; even the two-body problem for comparable masses has no exact analytic solution. The way forward is perturbative and iterative: begin with a Newtonian description of weakly interacting, slowly moving bodies, and then calculate relativistic corrections order by order in small parameters such as v/c. At lowest order, the scheme reduces to Newtonian gravity.

Although the basic idea of such *post-Newtonian* methods had already been introduced in the early years of GR by De Sitter (1916a), Lorentz, and Droste (1917), the first systematic treatment of the problem of motion for several comparable gravitating bodies was carried out by Einstein, Infeld, and Hoffmann in 1938.[2] Their method, now known as the EIH method, derived the first-order post-Newtonian corrections to the motion of gravitating bodies. A year later, Fock (1939a,b) independently solved the same problem, but in a different way. He used harmonic coordinates and treated masses as having finite extension, whereas the EIH approach represented masses by singularities. Fock (1964) himself later described these as two distinct lines of development: one school associated with Einstein, Infeld, and their collaborators, the other with Fock and, later, Papapetrou (1951). As Martinez (2020) points out, their technical differences became central to Fock's broader interpretation of GR and his confrontation with Infeld.

Compared to the EIH approach, Fock's work did not acquire the same canonical status in the West. Several factors limited its reception: the Second World War disrupted scientific communication; his 1939 results appeared both in Russian and in French, but English increasingly became the dominant language of postwar relativity.[3] Moreover, his unique insistence on harmonic coordinates in approximation methods marginalized his "school." This is what we will turn to next.

3.2.2 Bern in 1955: the Debate on Harmonic Coordinates

The year 1955 marked the 50th anniversary of the special relativity. Held in the city where Einstein had worked at the Patent Office, the meeting brought together many of the leading figures in relativity theory. It also marked a transition from what Jean Eisenstaedt (1986, 1989) called "low-water-mark of general relativity" to what Clifford Will (1986, 1989)

[2]See De Sitter (1916a) and Lorentz et al. (1917). These early post-Newtonian treatments contributed to the relativistic celestial-mechanical program that confirmed Einstein's calculation of Mercury's anomalous perihelion advance. What became canonical in EIH was rather a systematic treatment of the problem of motion for several comparable gravitating bodies, deriving equations of motion from the field equations themselves. See Kennefick (2007, chap. 8); cf. Maggiore (2007, sec. 5.1.2).

[3]Scientists were expected to read several languages, especially German, French, and English. The Second World War and its aftermath accelerated a reconfiguration, and English eventually became the default global vehicular language. See Gordin (2015) for the history of this transition from multilingual to overwhelmingly Anglophone.

called the “renaissance of general relativity.”[4] The Bern conference was especially significant because it helped inaugurate the regular international conference series on GR, contributing to the reconstruction of the relativity community after WWII. Unfortunately, Einstein was not there. He passed away in Princeton on April 18, 1955, just three months before the conference.[5]

Among many prominent relativists who gathered in Bern during the Cold War, were there Soviet physicist Vladimir Fock and Polish physicist Leopold Infeld. Their confrontation at Bern has been remembered through a vivid anecdote reported by John Moffat (2010, p.101-102):

> Infeld and Fock gave talks one after the other at the meeting. Fock, a large, stocky Russian who kept adjusting his hearing aids since he was completely deaf without them, gave the first talk, presenting his reasons for believing in the gravitational wave solutions in Einstein’s theory. Infeld stood off to one side near the podium as Fock spoke. He was also a large man, a Polish physicist from the University of Warsaw. He was already beginning to bristle with indignation over Fock’s talk. When Fock finished speaking, Infeld went to the podium. When he started speaking, presenting his reasons for not believing in the existence of gravitational waves, Fock ostentatiously removed his hearing aids, raising laughter in the auditorium. Even Hermann Weyl, the chair of the session, smiled with amusement.

Moffat’s recollection captures something real about the drama of the encounter. Fock was indeed almost entirely deaf without his hearing aids, as many who knew him remarked. It is also true that Fock accepted the physical reality of gravitational waves, whereas Infeld was, at this stage, skeptical. But Moffat (2010) slightly misidentified the main point of dispute at the Bern session. Fock and Infeld were not, at least in their presentations, debating the existence of gravitational waves. Their explicit disagreement concerned harmonic coordinates, and more generally the status of privileged coordinate systems in GR.[6] The two issues were, of course, closely related.

[4]Eisenstaedt (1986, 1989) characterizes the period between 1925 and 1955 as the “low-water mark” of general relativity, a phase of research dispersion and stagnation. For a synthesis of how this was later activated into a “renaissance,” see Blum, Lalli, and Renn (2016; 2020).

[5]It is unclear if he would have attended, however. Like many German-speaking émigré scientists, Einstein never returned to Europe even when he had the opportunity after the war.

[6]See Mercier and Kervaire (1956) for the conference proceedings. It is possible that Moffat conflated the formal conference exchange with a later discussion during the excursion to Lake Brienz. He writes: “One day, the conference organizers took the delegates on a trip to Lake Brienz in the Bernese Mountains. As we sailed around the lake, I had the opportunity to stand next to Infeld and Fock, who continued to attack one another on the issue of gravitational waves. This time Fock did not remove his hearing aids” (Moffat, 2010, p. 103).

As we will see later, harmonic gauge helps put the field equations into a wave-like form and, like the Lorenz gauge in electromagnetism, can make finite-speed propagation especially transparent when supplemented by appropriate boundary conditions.

Fock's insistence on the use of harmonic coordinates was unconvincing to many relativists. It was even rejected by several leading Soviet physicists, including Landau, Tamm, and Ginzburg.[7] Infeld, in his presentation in Bern, emphasized that EIH obtained the same post-Newtonian results as Fock using different coordinate conditions, and therefore harmonic coordinates had no privileged role Fock assigned to them. Fock (1960) later noted that the first-order coordinate conditions actually used by EIH coincide with the harmonic ones, and that any difference is too small to affect the first post-Newtonian equations of motion. That is, EIH coordinates are "harmonic" at the relevant post-Newtonian order. However, his view remained "unorthodox".

It is worth noting several key aspects of Fock's career before proceeding to his controversial stance on the harmonic gauge. Fock is best known to many physicists for his contributions to quantum theory, especially "Fock space" and the Hartree–Fock method. He was also known by his peers for his extraordinary mathematical skill: "Fock kann einen Stiefel ausrechnen" ("Fock could integrate even a boot"), as Paul Ehrenfest once said.[8] But he was also a major, if often overlooked, figure in the history of relativity theory. Graham (1981) compares the role Fock played for relativity theory in the Soviet Union to the role Arthur Eddington played in Great Britain: both were important public interpreters of relativity in their respective countries; both were attacked by philosophers who thought they were doing bad philosophy and by physicists who disliked the philosophical interpretations they attached to relativity; both authored books

[7]Cited in Gorelik (1993). The original source is L. Infeld, "My Recollections of Einstein" [in Russian], in *Einshtein i sovremennaya fizika*, ed. I. E. Tamm (Moscow: Nauka, 1956), 197–260.

[8]"When P. S. Ehrenfest met Fock back at the beginning of the latter's scientific career, the usually strict and exacting critic expressed his astonishment at Fock's mathematical talent in his usual distinctive manner: 'Fock could calculate himself out of a closed barrel" ('Fock kann einen Stiefel ausrechnen'). Translated from Ehrenfest's typical metaphor, this was the highest possible appraisal of Fock's talent—the observation that there was no mathematical problem with which he could not deal. This characterization followed Fock for the rest of his days." *Vladimir Aieksandrovich Fock (obituary)* by M. G. Veselov, P. L. Kapitza, and M. A. Leontovich (1975). Here I've used Graham (1981)'s translation of Ehrenfest's metaphor instead.

with strikingly similar titles: Eddington's *Space, Time and Gravitation* and Fock's *The Theory of Space, Time and Gravitation*. Fock, like Eddington, framed relativity in a manner tailored to the values of his surrounding intellectual culture. This means, in contrast to Eddington who believed relativity was compatible with idealism and incompatible with materialism, Fock sought to present the theory as consistent with the Marxist rhetoric of *dialectical materialism* and the existence of an objective physical world independent of the mind.

This context clarifies one of the reasons why Fock insisted on referring to Einstein's theory as the "theory of gravitation" rather than the "general theory of relativity." In the Soviet Union, the terminology of "relativity" could be rhetorically exploited to suggest subjectivism, idealism, or a Machian denial of objective reality—positions fiercely criticized by Lenin in his *Materialism and Empirio-criticism*. By reframing the nomenclature, Fock rendered the theory intellectually acceptable to a Marxist audience, "cleansing" it of the "idealist" and "bourgeois-scientist" associations often attributed to it by its Soviet critics.

Philosophers of physics today would largely agree with Fock that GR is compatible with an objective, physical, and mind-independent world at least in principle.[9] Much of the surrounding polemic in Soviet Union was a product of bad philosophy entangled with bad interpretations of the physics. While Fock strategically employed the Marxist rhetoric of dialectical materialism to shield relativity from ideological attacks and criticized Einstein's own interpretations in the process, it would be a mistake to view his reinterpretation as mere catering to Marxist philosophy. His "unorthodox" stance was also (and perhaps primarily) motivated by deep-seated technical and physical considerations to which he was genuinely committed.

In fact, Fock accepted "relativity" as the proper name for the special theory, because in that case the Poincaré group expresses the uniformity of Minkowski spacetime and the equivalence of inertial frames. What he resisted was the extension of the same label to Einstein's gravitational theory. For him, general covariance by itself, was too weak to be a physical

[9]Relativism does not yeild subjectivism. Specifically, there is an objective fact of the matter that, given a specific coordinate choice, a system is described in a particular way; and there is an objective fact regarding how different coordinate systems relate to one another.

principle. It only marked a freedom in the representation of equations. Fock made this explicit at the Bern conference, in his short lecture "Sur les systèmes de coordonnées privilégiés dans la théorie de gravitation d'Einstein" (On Preferred Coordinate Systems in Einstein's Theory of Gravitation) delivered in French. There he distinguished between invariance "in the physical space of coordinates" and invariance "in a functional space," and insisted that only the former should be associated with relativity in the proper physical sense.[10] He said,

> Covariance in a functional space, which allows for the uniform treatment of all coordinate systems, already exists in Newtonian mechanics (Lagrange equations of the second kind) and is not characteristic of any specific physical law. As for the equivalence between the gravitational and acceleration fields, it is purely local.[11]

Like Kretschmann (1917), Fock did not think general covariance was a physical constraint.[12] Therefore, he thought it could not be what made Einstein's gravitational theory a theory of "general relativity". He then explained his *physical* and *non-local* motivation for harmonic coordinates, which he denoted as $\mathfrak{g}^{\mu\nu}{}_{,\nu} = 0$ (in modern notation, this is often represented by the vanishing of the contracted Christoffel symbols, $\Gamma^{\mu} := g^{\alpha\beta}\Gamma^{\mu}_{\alpha\beta} = 0$):

> In the problem of defining coordinates, it is indispensable to specify the character of the physical system under consideration. If the system of masses is isolated and embedded in a pseudo-Euclidean spacetime, one can indicate a coordinate system defined uniquely, up to a Lorentz transformation. Since the problem is not local, conditions at infinity play an essential role.[13]

For Fock, the term "Lorentz transformation" includes spacetime translations, thus constituting

[10] "La notion 'relativité' est étroitement liée à la notion 'invariance' (ou covariance). Cette dernière s'emploie souvent dans deux sens différents qu'il faut cependant distinguer. On a (a) l'invariance dans l'espace physique des coordonnées et (b) l'invariance dans un espace fonctionnel. Pour éviter la confusion, la notion 'relativité' ne devrait s'appliquer qu'à l'invariance dans le sens (a)." Fock (1956b, p. 239)

[11] "La covariance dans un espace fonctionnel qui permet de traiter d'une facon uniforme tous les systèmes de coordonnées existe déjà dans la mécanique newtonienne (équations de Lagrange de seconde espèce) et ne caractérise aucune loi physique. Quant à l'équivalence entre le champ de gravitation et celui de l'accélération, elle est purement locale." Fock (1956b, p. 239).

[12] For discussion of Kretschamann's objection, see Norton (2003).

[13] "Dans le problème de la définition des coordonnées il est indispensable de préciser le caractère du système physique considéré. Si le système des masses est isolé (et plongé dans un espace-temps pseudo-euclidien), on peut indiquer un système de coordonnées défini d'une manière unique, à une transformation de Loeentz près. Le problème n'étant pas local, les conditions à l'infini y jouent un rôle essentiel. Fock (1956b, p. 239).

the 10-parameter group now referred to as the Poincaré group.[14] His emphasis on the non-local nature of the problem and the importance of boundary conditions at infinity marks a clear departure from the early, Mach-inspired Einstein.[15] In the discussion that followed Fock's short lecture, Infeld objected (in English) that by adding harmonic coordinate conditions Fock obtained a system invariant only under Lorentz transformations, and that this amounted to a loss of "the beauty of" and "a retrogressive step from" relativity theory, which from the mathematical point of view admits all coordinate systems. Fock then replied (in German) that the relation between harmonic coordinate systems and the generally covariant Einstein equations is analogous to the relation between inertial systems and the generally covariant Lagrange equations of classical mechanics. The recognition of the significance of harmonic coordinates in gravitation theory, he argued, is no more retrogressive than the recognition of the significance of inertial systems in classical mechanics, which can itself be formulated generally covariantly.[16] He told Infeld,

> In itself, the idea of general covariance is not a physical idea, and I cannot see how it can conflict with the fact that privileged coordinate systems exist.[17]

If general relativity teaches us that the laws of physics can be written in arbitrary coordinates, then how could one coordinate condition have a privileged physical status? Fock's answer seems to be that this question involves two conflations. First, it conflates a *formal* property of the equations with the *physical* conditions needed to formulate a concrete problem. Second, it

[14]This is clarified in his book. "The most general transformation involves ten parameters. This is the Lorentz transformation. It is well known that in a space of n dimensions the group of transformations which leave invariant the expression for the squared distance between infinitely near points, can contain at most $\frac{1}{2}n(n+1)$ parameters. If there is a group involving all $\frac{1}{2}n(n+1)$ parameters then the space is of maximal uniformity; it may be a space of constant curvature, or, if the curvature vanishes, a Euclidean or pseudo-Euclidean space." (Fock, 1964, p. 1).

[15]Einstein initially hoped to avoid any appeal to asymptotic structure in determining the metric and the dynamics of the gravitational field. In 1955, however, Einstein had already given up the Machian program.

[16]"Die Beziehung zwischen den harmonischen Koordinatensystemen und den allgemein-kovarianten Gleichungen der Einsteinschen Gravitationstheorie ist ganz analog der Beziehung zwischen den Inertialsystemen und den allgemein-kovarianten Lagrangeschen Gleichungen zweiter Art der klassischen Mechanik. Die Anerkennung der prinzipiellen Bedeutung der harmonischen Koordinatensysteme in der Gravitationstheorie ist ebensowenig ein rückgängiger Schritt...wie die Anerkennung der prinzipiellen Bedeutung der Inertialsysteme in der klassischen Mechanik, die ja auch allgemein-kovariant formuliert werden kann." Fock (1956b, p. 240-241).

[17]"An und für sich ist die Idee der allgemeinen Kovarianz keine physikalische Idee, und ich kann nicht einsehen, wie sie mit der Tatsache der Existenz bevorzugter Koordinatensysteme in Konflikt geraten kann." Fock (1956b, p. 241).

conflates the *local* freedom to use arbitrary coordinates with the *global* conditions required to characterize an isolated system. We may restate Fock's reply to Infeld as follows:

> *In itself, the idea of general covariance is not a physical idea and, much like the principle of equivalence, it only applies locally. Therefore, it does not conflict with the existence of privileged coordinate systems when the theory is supplied with non-local assumptions given the character of the physical system under consideration (such as the system being isolated and asymptotically flat).*

The extra qualifications added here can be found both in the Bern conference discussion and in Fock's other writings. For the isolated systems Fock primarily considered, his defense of harmonic coordinates was rooted in the global formulation of the physical problem. He assumed that space is uniform at infinity: local non-uniformities due to masses are, as he put it, embedded in an "unlimited Galilean space." In Fock's terminology, this does not mean Newtonian Galilean spacetime, but the flat pseudo-Euclidean, i.e., Minkowskian spacetime of special relativity. More precisely, Fock treats the asymptotic region as having the structure whose uniformity is expressed by the ten-parameter group of transformations consisting of spacetime translations, spatial rotations, and Lorentz boosts. In modern terminology, this is naturally understood as a form of asymptotic flatness at spatial infinity, with an asymptotic Poincaré structure built into the boundary conditions.[18] This condition of asymptotic flatness is what allows a system to be treated as isolated.

In a Galilean space, Cartesian coordinates are "privileged" or more adapted to the geometry because Lorentz transformations are linear in those coordinates. Fock argued that for a spacetime that is uniform at infinity, it is similarly possible to introduce preferred coordinates that

[18] Fock does not use the later terminology of "asymptotic flatness" or "Poincaré symmetry at infinity." But his description strongly suggests this interpretation. For instance, the introduction of *The Theory of Space, Time and Gravitation* says "...the uniformity of Galilean space are related to the following transformations: (a) To the equivalence of all points and instants corresponds the transformation of displacing the origins of the spatial coordinates and of time; the transformation involves four parameters, namely, the three space coordinates and the time coordinate of the origin. (b) To the equivalence of all directions corresponds the transformation of rotating the spatial coordinate axes; this involves three parameters, the three angles of rotation. (c) To the equivalence of inertial frames corresponds a change from one frame of reference to another moving uniformly in a straight line with respect to the first; this transformation involves three parameters, the three components of relative velocity." Fock (1964, p. 1).

are unique up to a Lorentz transformation: the harmonic coordinates. In an isolated gravitational system, spacetime is not exactly uniform, but it is asymptotically uniform. Harmonic coordinates are Fock's way of extending the asymptotic Galilean/Minkowskian structure into the curved spacetime, recovering a preferred class of coordinates up to Lorentz transformations. Fock (1956b, 1964) maintained that only with this framework can one demonstrate that Einstein's theory retains a preferred position for the heliocentric Copernican system over the geocentric Ptolemaic one. For him, the preferred status of heliocentrism was not a subjective choice, but a consequence of the global distribution of matter and the boundary conditions of the problem. This interpretation is further supported by the fact that Fock agreed that in cosmological models (such as FLRW metrics), where such boundary conditions do not apply, the harmonic gauge would not be preferred.[19]

Fock's critics thought he had mistaken a useful gauge condition for a feature of the world. This is not entirely fair. If we view Fock's insistence on harmonic coordinates as a strategy to safeguard a mind-independent, materialist description of the world in the Leninist sense, we risk overlooking a profound physical insight—one that remains under-appreciated by physicists, historians, and philosophers alike. Fock correctly observed that general covariance is a *local* property—allowing for arbitrary coordinates *locally*—but the study of actual physical systems requires a global perspective. Contrary to the common assumption that Fock privileged the harmonic gauge *simpliciter*, his preference was conditional and physically motivated. While he may have occasionally been guilty of overstating his case, he did not view the harmonic condition as a metaphysical requirement. Rather, he argued that for isolated systems (i.e., those that are asymptotically flat), the harmonic gauge is far more than a tool for computational convenience; it carries physical significance.

In what follows, we shall see how Fock's understanding of the physical significance of

[19]Instead, the coordinates best adapted to the problem are the "conformally-Galilean" coordinates, in which the metric is written in a conformally pseudo-Euclidean, or conformally Minkowskian, form. Fock writes that, in Friedmann-Lobachevsky space, "the conditions are quite different," and the "conformally-Galilean" coordinates are more appropriate to the character of the problem. See (Fock, 1964, §94).

the harmonic gauge enabled him to develop a prescient and pluralistic approximation scheme: a three-zone patchwork construction that supported the existence of gravitational waves.

3.3 Towards Patchwork Gravitation

3.3.1 Post-Newtonian versus Post-Minkowskian Approximations

From a modern standpoint, Fock overstated the *unique* status of harmonic coordinates. Methodologically, however, he was onto something important: without a consistent prescription for gauge and boundary conditions, radiation-reaction calculations can yield spurious damping, antidamping, or no damping at all. This difficulty, internal to the problem of motion, fueled decades of uncertainty about the status of gravitational waves. From the start, physicists disputed whether such waves were genuine physical phenomena or merely artifacts of coordinate choice. Do gravitational waves exist and do they carry energy as electromagnetic waves do? Einstein himself wavered: in 1916 he introduced gravitational waves (Einstein, 1916), and in 1918 derived the quadrupole formula for gravitational energy loss (Einstein, 1918c), only to argue two decades later, in an unpublished manuscript with Rosen, that such waves do not exist—a conclusion they reversed in the published 1937 paper (Einstein and Rosen, 1937).[20] At the Bern conference, Rosen (1956) presented his gravitational-wave solution as a standing wave, and hence as involving no damping or energy loss.[21] Peres (1960)'s later work provides another useful example: with inappropriate boundary conditions (by allowing incoming radiation), he showed that a binary could gain energy rather than lose it.

[20] For the publication history of the Einstein–Rosen episode, see Kennefick (2005). Kennefick notes that Einstein and Rosen's original submission to *Physical Review* was titled "Do Gravitational Waves Exist?" and argued against the existence of gravitational waves, but that the published version, Einstein and Rosen (1937), appeared with substantially revised conclusions.

[21] Rosen (1956) at this point did not deny all exact wave-like solutions in GR. His skepticism was directed instead at gravitational waves understood as physically radiative, energy-carrying phenomena analogous to electromagnetic waves. He argued that the plane monochromatic waves of the linearized theory do not correspond to exact solutions, and that the cylindrical waves he analyzed carry no energy or momentum. Fock's response after Rosen's presentation shifted attention away from cylindrical waves: he argued that the physically relevant case is that of spherical waves emitted by a finite system of bodies, where nonlinear corrections can be included. Rosen granted that spherical waves are physically more important, but maintained that exact solutions are needed to settle the question.

An important strand of the early gravitational-wave controversy was methodological: it concerned a dispute between post-Newtonian and post-Minkowskian approaches over how radiative phenomena should be extracted from Einstein's nonlinear field equations (cf. Kennefick (2007, Ch 8)). The EIH method belongs to the former tradition. It uses a first post-Newtonian expansion, formally justified for systems in which the bodies move slowly compared with the speed of light and the gravitational field in the interbody region is weak.[22] The technique expands in small dimensionless parameters to generate post-Newtonian corrections. When done iteratively, it turns out to be remarkably useful as a conceptual probe of how the non-linear structure of GR constrains motion.

However, because this slow-motion approximation effectively reduces GR to its Newtonian limit, it fails to capture the richness of relativistic gravitational phenomena, especially in the far-field region. As Maggiore (2007, p. 248) notes, the straightforward post-Newtonian expansion used in early work, suffers from two serious problems. The first is technical: extending the post-Newtonian expansion into the far field led to divergences at higher orders.[23] The second is conceptual: the post-Newtonian expansion fails to incorporate boundary conditions at spatial infinity. Asymptotically, the series blows up with distance approaching to infinity, making it impossible to impose the no-incoming-radiation condition. Thus, while the post-Newtonian scheme reduces smoothly to Newtonian gravity, it cannot straightforwardly capture the dynamics of the far zone, or what, in post-Minkowski terminology, is called the wave zone.

Another approach is to assume a Minkowski background and calculate perturbations induced by matter and their higher-order contributions in iterations. This is known as the *post-Minkowskian* (or weak-field, linearized) approximation. At lowest order, this reduces to special relativity. Einstein's 1916 paper on gravitational waves first introduced the linearized

[22]Later work showed, however, that suitably formulated post-Newtonian methods can be remarkably effective even for systems containing bodies with strong internal gravity, such as neutron stars and black holes (Will, 2011).

[23]More specifically, beyond 2PN order divergences appear in the so-called Poisson integrals. Up to 2.5PN order these could be finessed with ad hoc tricks, but by 3PN the divergences become unavoidable. Now we know that the correct treatment employs not the naïve Poisson integral but what Damour called "analytic continuation," or what Havas, according to Kennefick (2007, p. 250), preferred to refer to as Riesz potentials.

approximation, which provided the basis for the post-Minkowski expansion. Newtonian gravity admits no radiation, and the post-Newtonian scheme likewise showed no evidence of gravitational waves (at low orders).[24] By contrast, the post-Minkowski method emerged precisely to model those radiative phenomena that the post-Newtonian expansion could not handle. It treats the gravitational field as a perturbation of flat Minkowski spacetime and naturally leads to the derivation of wave equations and the quadrupole formula.

Yet the post-Minkowski approach has its own limitations—linearizing the field equations stripped GR of its defining feature: nonlinearity. Modeling gravitational waves as if they travel through a fixed background risks introducing coordinate artifacts, neglecting self-interaction, and misrepresenting the interplay between matter and radiation. Historically, these worries were compounded by calculative failures: ill-specified boundary conditions (with incoming waves) sometimes led to unphysical results, such as binary systems appearing to gain energy through radiation (e.g. Peres (1960)), and some of these sign errors were not corrected until decades later. For its detractors, the post-Minkowski approach, which requires a split of background and perturbation, seems to lose touch with the geometric unity of GR.

Both approximation methods have flaws. After all, if the effects are genuinely relativistic, how can Newtonian dynamics serve as a reliable baseline? And if matter is truly present, how can spacetime be Minkowski to start with? The question then arises: which approximation should we trust? Post-Newtonian methods suggest orbital motion of a rotating binary system remains undamped, as in Newtonian mechanics; post-Minkowski methods, by analogy with electromagnetism, yield gravitational radiation. As Trautman (1958, p. 627) put it, the early debate about gravitational radiation was precisely a debate over which analogy to privilege, i.e., "whether the situation in General Relativity resembles that in Newtonian mechanics rather than that in electrodynamics." Each camp had its refrain. For post-Newtonian relativists, gravity is nonlinear, so the analogy with electromagnetism misleads. For post-Minkowski relativists, gravitational radiation is as real as electromagnetic radiation.

[24] Higher-order corrections revealing radiation were discovered much later.

3.3.2 What Type of Equations are Einstein's Field Equations?

The contrast between post-Newtonian and post-Minkowskian methods becomes clearer once we ask why these approximation schemes work at all. I argue in this section that their success depends, in part, on the mixed mathematical character of the EFE. Mid-twentieth-century relativists often handled this mixture in practice. But we can now describe it more cleanly using the modern initial-value and Hamiltonian formulations of GR. In particular, the $3+1$ decomposition makes explicit that the EFE are neither simply Poisson equations nor simply wave equations. They contain constraint equations of the elliptic type, which restrict admissible initial data on a spatial hypersurface, as well as evolution equations of the hyperbolic type, which determine how those data develop. This distinction helps explain why different approximation schemes bring different aspects of GR to the foreground.

A useful way to see the point is to begin with three familiar types of partial differential equation (PDE). First, elliptic equations determine a field across space *at a given time*. The standard example is the Poisson equation,

$$\nabla^2 u = f \tag{3.1}$$

where $\nabla^2 = \delta^{ij}\partial_i\partial_j$ is the flat spatial Laplacian, u is the field to be solved for, and f is a source term. Newtonian gravity has precisely this form:

$$\nabla^2 \Phi = 4\pi G\rho, \tag{3.2}$$

where Φ is the Newtonian gravitational potential, ρ is the mass density, and G is Newton's constant. The equation does not have "time" in it and does not describe any propagation. Given the mass distribution and suitable boundary conditions, it fixes the gravitational potential. Since elliptic equations do not have time derivatives, they are naturally associated with boundary-value problems: given sources and suitable boundary conditions, one solves for the spatial field.

Second, parabolic equations introduce time evolution, but of a dissipative kind. The standard example is the heat equation,

$$\partial_t u = \nabla^2 u, \tag{3.3}$$

which describes the rate of change of u with respect to time is proportional to the spatial Laplacian of u. The equation can describe how temperature changes in response to spatial differences in temperature and how the system approaches equilibrium. Unlike Newtonian equations of motion, the heat equation is not time-reversal invariant.

Third, hyperbolic equations describe finite-speed propagation. The standard example is the wave equation,

$$\partial_t^2 u = \nabla^2 u, \tag{3.4}$$

where the time derivative is in the second order. Or equivalently,

$$\Box u = 0, \qquad \Box = -\partial_t^2 + \nabla^2, \tag{3.5}$$

where $\Box$ is the flat-spacetime d'Alembertian. They are naturally associated with initial-value problems: given a field and its first time derivative on an initial slice, the equation determines its subsequent propagation. For instance, Klein-Gordon equations are evolution equations of this type, given a scalar field on an initial time slice and its first derivative, we get a unique solution.

In the opening of his 1917b paper "Cosmological Considerations in the General Theory of Relativity", Einstein compared his field equations with the Poisson equation $\nabla^2\Phi = 4\pi G\rho$. This should not imply that the full EFE simply have the same PDE character as the Poisson's equation. Rather, as Belot (ms.) notes, Einstein was concerned with the way field equations require boundary conditions to pick out the physically intended solution: just as Poisson's equation alone does not yet select the Newtonian potential generated by a given mass distribution, the relativistic field equations also require attention to asymptotic conditions.[25]

What type of equations are the EFE? Setting aside the cosmological constant, Einstein's field equations are

$$G_{\mu\nu} = \kappa T_{\mu\nu}, \qquad \kappa = \frac{8\pi G}{c^4}. \tag{3.6}$$

Here $G_{\mu\nu}$ is the Einstein tensor, $T_{\mu\nu}$ is the stress-energy tensor, and $g_{\mu\nu}$ is the metric to be solved for. Since both $G_{\mu\nu}$ and $T_{\mu\nu}$ are symmetric, the equation has ten independent components. It is therefore natural to say that the field equations are ten coupled, nonlinear, second-order equations for the metric components $g_{\mu\nu}$. But they are not ten independent evolution equations.

The reason is that the Einstein tensor satisfies the contracted Bianchi identity,

$$\nabla_\mu G^{\mu\nu} = 0. \tag{3.7}$$

This identity implies that four combinations of the field equations function as constraints on the initial data rather than as equations that independently evolve that data.[26] Thus, in a $3+1$ decomposition, *four of the ten equations are constraint equations of elliptic type*.[27] These constraints are closely connected with the gauge freedom of the theory. The remaining *six hyperbolic equations govern the evolution* of the spatial metric and extrinsic curvature. Once an appropriate gauge has been fixed, this evolution sector can be written in a hyperbolic, wave-like

[25] Belot's reconstruction is roughly this. If ϕ_N is the Newtonian potential for a given density ρ, then $\phi_N + \phi_0$ is also a solution of the Poisson equation whenever $\nabla^2\phi_0 = 0$. Unless a condition at spatial infinity is imposed, the extra harmonic term ϕ_0 can generate non-Newtonian forces. Einstein saw an analogous issue in GR: in the weak-field, quasi-static limit, one must ensure that the metric perturbation yields the Newtonian potential associated with the finite source, rather than merely some solution of the corresponding Poisson equation. See Belot (ms.), *Cosmological Reconsiderations*, ch. 1.

[26] The contracted Bianchi identity can be written schematically as

$$\partial_0 G^{0\nu} + \partial_i G^{i\nu} + \Gamma^\mu_{\mu\lambda} G^{\lambda\nu} + \Gamma^\nu_{\mu\lambda} G^{\mu\lambda} \equiv 0.$$

Here 0 labels the time coordinate, $i = 1,2,3$ label spatial coordinates, and $\Gamma^\alpha_{\beta\gamma}$ are the Christoffel symbols. In a $3+1$ decomposition, equations $G_{i\nu}$ contain at most second time derivatives of the metric, so $G_{0\nu}$ contain no second time derivatives of the metric. Therefore, there are four equations that constrain the initial data rather than supply independent second-order evolution equations.

[27] They restrict how the spatial metric γ_{ij} and the extrinsic curvature K_{ij} may be distributed on an initial hypersurface.

form.

Understanding the mixed elliptic and hyperbolic character of Einstein's field equations helps explain the division of labor as opposed to the incompatibility between the two approximation schemes: Post-Newtonian methods work best for slow-moving sources in the near zone, where GR can often be treated as "quasi-elliptic" or "Poisson-like," while Post-Minkowskian methods are better suited to the far zone, where the hyperbolic, wave-propagating character of the field equations becomes essential.

Post-Newtonian methods are naturally adapted to the slow-motion, near-zone regime, where retardation effects are suppressed and the equations reduce, order by order, to Poisson-like equations for gravitational potentials. In this setting, the elliptic, constraint-like face of GR is manifested. The expansion parameter is typically taken to be

$$\varepsilon \sim \frac{v}{c} \ll 1, \tag{3.8}$$

where v is the characteristic velocity of the source. For a self-gravitating system, this slow-motion assumption is tied, by the virial theorem, to the weak-field assumption. Thus the PN expansion is not merely an expansion in velocity but also an expansion in gravitational field strength. The key scaling is that time derivatives are suppressed relative to spatial derivatives:

$$\partial_t \sim O(\varepsilon)\partial_i. \tag{3.9}$$

Hence the temporal part of the flat-spacetime wave operator is smaller than the spatial Laplacian by v^2/c^2. At Newtonian order, or zeroth PN order, the d'Alembertian therefore reduces to the spatial Laplacian:

$$\Box = -\frac{1}{c^2}\partial_t^2 + \nabla^2 = [1 + O(\varepsilon^2)]\nabla^2 \approx \nabla^2. \tag{3.10}$$

This is the sense in which the leading, Newtonian part of the PN scheme is Poisson-like: it gives instantaneous potentials determined by the matter distribution at a given time. The first post-Newtonian approximation then keeps the corrections of relative order $\varepsilon^2 \sim v^2/c^2$ beyond this Newtonian limit. 1PN adds the next relativistic corrections to the Poisson-like hierarchy. As Blanchet (2009, p. 131) notes, "the post-Newtonian expansion replaces the resolution of an hyperbolic-like d'Alembertian equation by a perturbatively equivalent hierarchy of elliptic-like Poisson equations." While the PN approximation does not deny the hyperbolic character of GR, retardation effects are suppressed at Newtonian order and re-enter only as higher-order corrections in the PN hierarchy, or through the matching of the near-zone PN solution to the far-zone Post-Minkowskian wave solution. This is why the PN scheme is best described as quasi-elliptic.

In post-Minkowskian expansion, one writes the spacetime metric as a small perturbation of the Minkowski metric:

$$g_{\mu\nu} = \eta_{\mu\nu} + h_{\mu\nu}, \qquad |h_{\mu\nu}| \ll 1, \tag{3.11}$$

where $\eta_{\mu\nu}$ is the flat Minkowski metric and $h_{\mu\nu}$ is the metric perturbation. Harmonic gauge helps put the field equations into a manifestly hyperbolic form. It provides four coordinate conditions,

$$g^{\alpha\beta}\Gamma^{\mu}_{\alpha\beta} = 0. \tag{3.12}$$

Harmonic gauge is helpful because it turns the principal part of the Einstein equations into a wave operator acting on the metric components. Under this condition, the linearized Einstein equations reduce to the inhomogeneous wave equation

$$\Box h_{\mu\nu} = -\frac{16\pi G}{c^4} S_{\mu\nu}, \tag{3.13}$$

where

$$S_{\mu\nu} = T_{\mu\nu} - \frac{1}{2} g_{\mu\nu} T \tag{3.14}$$

represents the source. The wave-like character of GR becomes explicit. The metric perturbation propagates according to a hyperbolic equation sourced by stress-energy. In the full nonlinear theory, harmonic gauge similarly allows Einstein's equations to be written as quasilinear wave equations for the metric, with lower-order nonlinear terms. While this does not mean that harmonic coordinates are uniquely privileged in the strong sense Fock claimed, it explains why they're not merely a calculative trick. There is a well-motivated physical reason why they were so effective for problems involving radiation, retardation, and matching between near-zone and far-zone analyses.

3.3.3 Fock's Zone-Based Construction

In summary, GR can often be treated as "quasi-elliptic" or "Poisson-like" near a slow-moving source. Gravitational influences still propagate at the speed of light, but in the near zone the source changes slowly with respect to the light-crossing time of the system. So the field can be approximated by a sequence of quasi-instantaneous gravitational potentials. This is the regime in which Post-Newtonian methods, which exploit the fact that, order by order, the near-zone field equations resemble Poisson equations, are useful. Far from the source, by contrast, the "hyperbolic" or wave character of GR can no longer be suppressed. The finite propagation time of the gravitational field matters, and the radiative degrees of freedom must be treated as genuinely propagating degrees of freedom. In this wave zone, Post-Minkowskian methods become especially useful, since they emphasize the hyperbolic structure of the field equations.

The role of harmonic gauge is somewhat different in these two schemes. In the Post-Minkowskian approximation, harmonic gauge helps put the Einstein equations into a manifestly hyperbolic form, with the metric perturbation governed by wave equations on a Minkowski background. In the Post-Newtonian approximation, the same gauge condition plays a different practical role. After the slow-motion expansion has been imposed, the harmonic condition relates the various PN metric components and potentials to one another, so that the metric can be solved order by order from a hierarchy of Poisson-type equations. Thus the potentials behave, at each PN order, much like instantaneous Newtonian potentials.

The two methods are not merely different calculational conveniences. They make tractable different aspects of a mixed and complicated system. In his historical discussion of the problem of motion, Kennefick (2007, p. 145) notes that the slow-motion or post-Newtonian scheme was well adapted to calculating relativistic corrections to two-body celestial motion, but became “ambiguous and ad hoc” when applied to radiation effects; by contrast, the fast-motion or post-Minkowskian scheme was “eminently suited” to radiation, but difficult to apply to the problem of motion for gravitationally bound systems.

It is therefore not surprising that the best approach is pluralistic—modeling different regions with different approximation methods. Fock treated the near-zone and wave-zone problems separately, imposed boundary conditions on the wave-zone solution, and then matched the two solutions in an intermediate region. This strategy is especially illuminating once we take seriously Fock (1964)’s emphasis on the finite propagation speed of gravitational effects and his insistence on the privileged role of harmonic coordinates for isolated systems. The electromagnetic analogy helps explain why this package is physically natural (even if Fock himself did not formulate the point in these terms): just as Lorenz gauge is well suited to radiation problems involving retarded electromagnetic potentials, harmonic coordinates make it possible to connect the motion of the source with the outgoing gravitational field. In this respect, Fock’s method can be read as a gravitational analogue of familiar electromagnetic strategies, where near, intermediate, and radiation zones are treated differently.[28]

Fock’s approximation scheme is best understood as a zone-based construction, not as one uniformly valid expansion. Fock (1964, Sec. 84-85) studies what he calls the field at “moderately large” distances: distances “large compared to the dimensions of the system yet small in comparison with the lengths of the waves radiated by the system.” In modern notation, this is often referred to as the exterior near-zone regime, $d \ll r \ll \lambda$, where d is the characteristic size of the source, r is the distance from the source, and λ is the characteristic wavelength of

[28] See D’Ambrosio et al. (2022) for a modern treatment of gravitational waves in comparison with electromagnetism and see Gomes and Rovelli (2024) for philosophical discussion.

the emitted gravitational radiation. In this region, the field point is far enough from the material source that one can use multipole expansions, but not so far that retardation dominates the structure of the solution. A post-Newtonian-type expansion remains appropriate because powers of r/λ are still small.

Fock then explicitly changes method when he turns to the wave zone, because as he puts it, "in the wave-zone the term in the wave equation involving the second time derivative may no longer be treated as a correction and the method of solving the equation must be different" (Fock, 1964, Sec. 86). The post-Newtonian expansion has lost its domain of validity: once r becomes comparable to, or larger than, λ, the terms that were small in the near-zone expansion are no longer small. Fock reorganizes the solution around retarded time and outgoing $1/r$-type radiation fields. His choice of the retarded solution is also physically motivated: for an isolated system, the waves in question are produced by the bodies themselves, so there shouldn't be incoming waves from infinity. After deriving the wave-zone field, he explicitly asks how it compares with the metric obtained earlier for moderately large distances (Fock, 1964, Sec. 87). He then argues that, once the logarithmic terms associated with radiative energy loss are neglected, the remaining expressions "go over into" the corresponding moderately-large-distance formulae. While this is not yet a formal matching technique developed more sophisticatedly later, it is a physically-motivated consistency check across overlapping regimes: the near-zone expansion and the wave-zone expansion should agree in the matching zone where both can be compared.

In retrospect, Fock's zone-based strategy looks prescient. Later systematic approaches to gravitational-wave generation—especially the Blanchet–Damour formalism and the DIRE formalism—implement a much more sophisticated version of the same logic. One uses post-Newtonian expansions in the near zone, post-Minkowskian expansions in the exterior and radiation zones, and matching or direct-integration techniques to connect the motion of the source with the outgoing gravitational field. Fock did not yet possess that later formal machinery but his practice already displays the methodological lesson that later became unavoidable: in GR, no single approximation scheme captures equally well both the quasi-elliptic, constraint-dominated dynamics of the source and the hyperbolic propagation of radiation to infinity.

The shift toward the modern matching approach did not come in a single conceptual breakthrough but rather through a gradual accumulation of insights between the 1950s and the 1970s. Several key developments stand out. Bondi (1957)'s use of the sticky bead thought experiment, following Feynman's remarks at the Chapel Hill conference, convinced many that gravitational waves carry energy (see DeWitt and Rickles (2017)). Further theoretical work by Bondi et al. (1962) showed that such energy loss is accompanied by a reduction in the system's effective gravitational mass. Sachs (1961) Newman and Penrose (1962) introduced the idea that the Weyl curvature tensor exhibits a characteristic "peeling" behavior near null infinity: as one approaches asymptotic regions, the various algebraic components of the Weyl tensor fall off at different rates, so that the radiative degrees of freedom can be isolated. The discovery of the Hulse–Taylor binary pulsar in 1974 opened the way to the first clear astrophysical test of gravitational-wave backreaction; subsequent timing observations showed that its orbital decay agreed with the loss of energy predicted by quadrupole radiation (see Hulse and Taylor (1975); Taylor et al. (1979); Taylor and Weisberg (1982)).

3.4 Morals of Patchwork Gravitation

The twenty-first-century detection of gravitational waves makes Fock's position look stronger than Infeld's in at least one respect. However, Fock's contribution to relativity is often dismissed by other relativists as an "unsuccessful appendage to relativity theory emanating from his eccentric criticism of Einstein's general relativity" (Graham, 1981, p. 94). Such a reception seems unfair. Fock downplayed the importance of the principle of equivalence and general covariance not because he denied them, but because he thought that, taken by themselves, they are local and therefore insufficient for specifying the physical content of a concrete problem. It is true that if we take these two principles to mean that every coordinate system is equally valid in every physically relevant sense, and that because at every spacetime point we can find coordinates in which the metric is Minkowskian up to first order, it would be an "equally good" description to pick a different locally inertial coordinate system at each point and thereby treat spacetime as Minkowskian everywhere, then we would have negated the need for a theory of gravity entirely. This, of course, is a reductio ad absurdum. Local Minkowskian structure does not remove curvature, and pointwise local inertial frames cannot be patched together into a single global inertial frame except in flat spacetime. But that is precisely Fock's point: if everyone agrees that this literal reading is absurd, then the physical significance of coordinates cannot be exhausted by the bare claim that all coordinate systems are formally admissible. Fock rightly warned that emphasizing local principles and the equivalence of all coordinates risks missing the actual physical scenarios of the problem at hand.

My presentation of the problem of motion and of approximation methods is inevitably incomplete, and possibly biased. Still, the morals it supports are, I hope, clear. First, Fock's emphasis on the physical context of a problem, and his use of harmonic coordinates as motivated by that context, helped him model isolated gravitating systems more effectively. Second, the problem of motion in GR cannot be resolved by a single model or a unique method, but requires a patchwork of regionally valid approximations, stitched together with care. The lesson is Cartwrightian: "One model brings out some aspects of the phenomenon; a different model brings out others. Some equations give a rougher estimate for a quantity of interest, but are easier to solve. No single model serves all purposes best" Cartwright (1983, p. 11). In GR, at least for the concrete problems at issue here, we cannot expect a single exact model to capture at once the source dynamics, the radiative field, and the relevant global structure of spacetime. This patch-stitching is a feature, not a bug.

In fact, the mathematics of differential geometry already provides a literal notion of patches: coordinate charts or n-charts on a manifold, which are the device we use to induce the local smoothness structure on the manifold. In GR, spacetime is modeled as a 4-dimensional *smooth manifold* equipped with a Lorentzian metric. To define a smooth manifold, we first begin with the notion of a topological manifold and then introduce the additional structure required for smoothness. A *topological manifold* of dimension n is a topological space with certain properties (i.e., Hausdorff, second-countable, and locally Euclidean of dimension n) that make the space locally resemble a Euclidean space. That is, each point on the manifold has a neighborhood homeomorphic to an open subset of $\mathbb{R}^n$. However, being locally Euclidean is insufficient for defining the kinds of differential structure required for calculus.[29] An extra structure is required. To supply this structure, we define a *smooth manifold* as a topological manifold endowed with a maximal atlas of *coordinate charts* (or *n-charts*) that satisfy a compatibility condition. Each chart is a pair (U, φ), where U is an open subset of the manifold, and $\varphi : U \to \mathbb{R}^n$ is a homeomorphism onto its image. These coordinate charts, often referred to as *coordinate patches*, are "the mechanism with which one induces local smoothness structure" on the manifold (Malament, 2012, p.1). To construct a smooth manifold of dimension n, one must specify a collection of such n-charts whose domains collectively cover the underlying topological space. Crucially, where two patches overlap, the associated coordinate transformations, known as transition maps, must be infinitely differentiable. It is this patchwise construction, together with the smooth compatibility of overlapping charts, that endows the manifold with its differentiable character.

But in practice, when we are not simply given a manifold equipped with *the metric of the world*, but are instead inferring the metric and its relevant features from phenomena, GR requires patchwork in a second and more physical sense. Since the equations are quintessentially local, GR realizes model pluralism to an extreme. At every point, or more realistically in every suitably small region, the EFE relate the curvature of spacetime to the stress-energy content in that region, subject to boundary conditions determined by the distribution of matter outside it. Solving the equations in any one patch requires information from its neighbors. Yet because gravitational effects are so weak, most ordinary objects—tables and chairs, for instance—can be idealized as vacuum, while only sufficiently massive bodies really "matter" for the purpose at hand. We

[29]As Lee (2012) notes, there is no purely topological invariant that captures the notion of smoothness: homeomorphic spaces may differ dramatically in their differentiable structure. For example, a circle and a square are homeomorphic as topological spaces, yet only the former is smooth. Smoothness cannot be characterized purely in topological terms.

therefore move from pointwise talk in differential geometry to large-scale modeling in practice. To model well, we must attend to the relative scale of the problem and use different models for different regions, depending on the context. As Cartwright (1983, p. 11) puts it, “We construct different models for different purposes, with different equations to describe them. Which is the right model, which the ‘true’ set of equations? The question is a mistake.”

GR provides a particularly interesting case of this moral because the theory appears, at first sight, to be the wrong place to look for Cartwrightian pluralism. Einstein’s field equations present a remarkably unified law-like structure: spacetime geometry and stress-energy are related by one set of generally covariant equations. But the practice of extracting predictions from those equations is far less monolithic. In the near zone of a slowly moving bound system, the post-Newtonian expansion treats the field in a way that is approximately Poisson-potential-like; in the wave zone, the relevant structure is different because retardation becomes essential. The same field equations underlie both descriptions, but the effective mathematical character of the problem changes with region, scale, and approximation order.

In *How the Laws of Physics Lie*, Cartwright argues that fundamental laws do not usually describe concrete physical situations directly. They acquire empirical application through models. The route runs from theory to model, and from model to a more concrete phenomenological claim. This is why she rejects the thought that explanatory power by itself is a guide to literal truth: powerful theoretical equations often explain by way of idealized objects that are not straightforward descriptions of the world. Her preferred picture is the “simulacrum” account of explanation: we construct a model that resembles the target system in some respects, neglects or distorts it in others, and permits the derivation of claims that are reliable for the purposes at hand (Cartwright, 1983). Her later examples in “Fundamentalism vs. the Patchwork of Laws” elaborates on this point. The familiar law $F = ma$ is not to be read as governing every motion in every circumstance. A falling coin may be modeled well by simple mechanics; a thousand-dollar bill swept through a square by the wind may require fluid-dynamical modeling, if it can be modeled adequately at all. Similarly, optical solitons are stable and real, but only under carefully engineered conditions in which dispersion and nonlinearity are balanced.[30] Theories succeed in domains, and sometimes we build the domain so that the theory applies (Cartwright, 1994).

Cartwright's account of theory-entry makes a close connection to Fock's methodology. On Cartwright (1983, p. 133)'s view, theory-entry has two stages: start with an unprepared description, convert it into a prepared description, then match that prepared description to a mathematical representation. Fock's harmonic-coordinate program can be read as a way of preparing the target system for treatment. It does not simply apply Einstein's equations to the world. It constructs the world as a mathematically tractable target. In the problem of motion, the "unprepared description" is something like: there are gravitating bodies, moving, interacting, with no exact analytic solution available. The "prepared description" is: treat them as slowly moving weak-field bodies in a near zone; treat radiation in a wave zone; impose retarded boundary conditions; use harmonic coordinates; match solutions in the overlap region.

Fock's patchwork gravitation shows that the successful extraction of gravitational radiation from the field equations depends on special representational conditions: isolated-system assumptions, asymptotic boundary conditions, near-zone and wave-zone descriptions, and matching. Even if one is realist and fundamentalist about Einstein's field equations, one should be Cartwrightian about their application. Fundamental laws, she argues, "are thoroughly abstract formulae which describe no particular circumstances." (p.11) They acquire empirical traction only when embedded in specially constructed models that are domain-specific and purpose-built.

3.5 Conclusion

Walking in the streets of Oxford, you might see lanes paved with stones worn smooth by centuries of weather and foot traffic—flags for flat expanses, kerbs guiding the edge, cobbles winding through alleys and hills. These stones were not laid uniformly. Each stretch was matched to its terrain. Modeling in GR is much the same. It requires selecting the right tools and patches

[30] Cartwright (1994)'s optical soliton example shows how a stable and technologically powerful phenomenon can be local to a specially arranged domain. Solitons are not a generic feature of low-loss optical fibers; they arise when dispersion and nonlinearity are carefully balanced.

for the right geometry.

In GR, the equivalence principle promises a qualified sense of local flatness, but the size of the region over which this holds depends on curvature. In weak-field regions, “flagstone” models, such as linearized GR, may suffice. But in regions of strong curvature, near black holes or in rapidly evolving spacetimes, only tightly fitted “cobbles”, numerical methods and local approximations, can get the job done. One cannot model a binary merger with Kerr, nor describe gravitational waves unambiguously in the near field. The model must match the terrain.

And just as some pavements are jarring, some coordinate systems in GR are ill-suited to the problem at hand. While the physics remains invariant under diffeomorphisms, the tractability, transparency, and interpretability of a model depend heavily on one’s coordinate choice. All coordinates may be equal in principle, but not in practice.

Physicists, like skilled masons, have developed techniques for laying good groundwork. Harmonic gauge, Fermi frames, and tetrad formalisms all offer tools for bringing out the right features. A poor choice may obscure conserved quantities, introduce fictitious singularities, or make numerical methods unworkable. A good one clarifies symmetries, simplifies equations, and makes key physical structures, such as event horizons or radiation fields, manifest. Much like a mason anticipates the curve of a street and selects good stones to match, the relativist tailors the frame to the problem. Fock was one of such great masons. He did not create the “perfect way”, but he helped show how to make one “walkable”.

Acknowledgments

Chapter 3 is original dissertation material. Parts of this chapter may be developed into a collaborative paper in the future. The dissertation author is the sole author of the chapter as it appears here.

Part II

Chapter 4

Chien-Shiung Wu: Breaking Parity Against Disparity

4.1 Introduction

Chien-Shiung Wu's life was remarkable. She was the first woman to join the physics faculty at Princeton University and the first female physics professor to secure tenure at Columbia University. She was the only Chinese scientist invited to participate in the Manhattan Project, the first woman to serve as president of the American Physical Society (APS), and the inaugural recipient of the Wolf Prize in Physics. Her cobalt-60 experiment, which demonstrated the violation of parity conservation, remains a cornerstone in the history of physics.

Her exclusion from the 1957 Nobel Prize, awarded to Chen-Ning Yang and Tsung-Dao Lee, sparked lasting controversy. Despite at least 12 subsequent nominations, Wu never received the Nobel Prize. But her scientific contributions went far beyond parity violation. In 1950, she and Shaknov demonstrated the existence of spatially separated entangled states. In 1963, she and her students confirmed the conserved vector current hypothesis in beta decay.

While Wu's contributions to physics are well-documented, the socio-political contexts in which she achieved these milestones remain underexplored. These contexts gave rise to her remarkable, yet often overlooked, feminist activism. This paper seeks to illuminate those socio-political realities, focusing especially on the challenges she faced as a woman scientist and a Chinese immigrant navigating life in 20th-century America.

Wu's life spanned some of the most turbulent chapters in modern Chinese and American history. Born in 1912, the year the Republic of China was founded, she came of age amid the intellectual ferment of the New Culture Movement and the May Fourth Movement, whose calls for "science" and "democracy" left a lasting mark on her. Guided by these ideals, she pursued a Ph.D. in physics at the University of California, Berkeley, studying under Ernest Lawrence, now known as one of the architects of "big science." It was Lawrence who later recommended her for work on the Manhattan Project.

Her life was shaped by separation and loss. The Sino-Japanese War cut off contact between her and her family, a silence that stretched through the Second World War. Though her family survived, the postwar years brought new challenges. She faced the political climate of McCarthyism, with mounting suspicion of alleged communist affiliations. She was dismayed to see her former professor and good friend, J. Robert Oppenheimer, placed under interrogations in 1954.

The news from China was no less devastating. She learned that her parents and older brother died from poor health during the Great Leap Forward, a time of famine and hardship. Later, during the Cultural Revolution, her younger brother was "persecuted into suicide".[1] Despite these personal losses, she remained committed to bridging divides. She witnessed the establishment of diplomatic relations between the U.S. and the People's Republic of China in 1979. She helped pioneer scholarly exchange programs between the two countries, fostering collaboration where there had long been division.

Wu's achievements reached beyond the laboratory. In the 1970s, as a member of Columbia President William McGill's advisory committee, she compiled and analyzed data from different departments to press for equal opportunity, pay, and pension. Within the committee, she led an informal subcommittee of women members focused on gender equity, addressing salary

[1] The official account that appears in the Zhengzhou Library's compiled *Chronology of Major Events in Zhengzhou* records that in 1979 the local authorities fully rehabilitated "Wu Jianhao, a former engineer at the municipal water company, who had been persecuted into suicide in October 1968." Some accounts suggest that he was in fact tortured to death though.

disparities and advancing affirmative action.

How did she do it? How did she carve out her place in a field that has historically been, and continues to be, hostile to women and minorities? Further, what might her story tell us about the ways marginalized standpoints shape scientific research? While authors have long pointed to examples of marginalized researchers making distinctive contributions in biology and the social sciences, some doubt whether the same occurs in physics.[2] Does Wu's story corroborate these doubts, or complicate them? By examining her life and work, I hope to offer insights into these questions.

Section 4.2 traces Wu's move from China to the U.S., culminating in a discussion of her wartime contributions to the Manhattan project. I show that her wartime research allowed her to break the glass ceiling in two ways: first, it allowed her to conduct research once more (rather than remain confined to teaching), a privilege at the time largely reserved for men; second, it allowed her to prove her "loyalty" as an Asian immigrant living in the U.S. during and after the Second World War. Section 4.3 tells the story of her parity-breaking experiment—better known as the Wu experiment—with a focus on how the experiment was performed, reproduced, and institutionalized. Section 4.4 explores her overlooked activism, especially her efforts to advance opportunities for women and minorities during the 1970s. This included her work on equal opportunity and affirmative action initiatives at Columbia University, and her historic role as the first woman to serve as president of the APS. Section 4.5 concludes.

4.2 Journey to the West

This section explores Wu's early life and career, from her upbringing in China to the end of the Second World War. It is divided into two parts. The first traces her journey across the Pacific to the West, driven by the ideals of "science" and "democracy" that emerged in the wake of the Qing dynasty's fall. The second examines her transition from the West Coast to the East Coast, where she contributed to wartime research. Throughout both periods, Wu's uniquely

[2] See, for example, Harding (1992), Wylie (2003), and Kukla (2006).

intersectional identity found her entangled with the historical and political forces of the Second World War and the Second Red Scare.

4.2.1 From Shanghai to San Francisco

Chien-Shiung Wu was born in May 1912, just months after the Xinhai Revolution, which culminated in the abdication of the Qing emperor in February 1912. The revolution marked the end of over two millennia of imperial rule in China. On March 10, 1912, Yuan Shi-kai was sworn in as the provisional president of the Republic of China—a connection Wu could never have anticipated would become personal. Thirty years later in 1942, she married Yuan's grandson.

Wu's father, Wu Zhong-Yi, was a progressive revolutionist and a staunch feminist.[3]Unlike many of his time, he celebrated the birth of his daughter with great enthusiasm and made a bold vow that he would establish a girls' elementary school in their town, a commitment that would shape Wu's early education. Her name, Chien-Shiung (or "Jian-Xiong" in Pinyin), given by her father, translates to "strong hero" or, in her case, "strong heroine." She had one older brother and one younger brother whose names both carry the same meaning. From an early age, she was raised to believe in the equality of men and women.

Wu began her education at Ming-De Girls' School, the very school her father had founded. She then went to a Girls' Normal School, training to become a teacher. Her exceptional abilities soon set her apart, and she was selected from her graduating class to attend the National Central University in Nanjing. Although she was interested in mathematics and physics, she initially assumed she would have to study education, given her limited scientific training at the Normal School. Her father persuaded her otherwise, assuring her there was time to prepare and urging her to follow her passion.[4] Without his encouragement, Wu might have taken a very different

[3]吳仲裔 (1888-1959). In this paper, all Chinese names are presented in the Chinese convention, with the surname first followed by the given name. Exceptions are made for Chien-Shiung Wu, Chia-Liu Yuan (Wu's husband), Tsung-Dao Lee and Chen-Ning Yang, whose names are shown in the Western order as they appear in their publications.

[4]Written material and cassette based on Dinah L. Moche's interview. C.S. Wu Papers, Box 9, Folder 20, Columbia University Archives (henceforth CUA).

path, and the world might have lost one of the foremost physicists of the twentieth century.

Wu's ambitions were deeply influenced by the New Culture Movement (1915-1919) and the May Fourth Movement (1919). Both anti-imperialists movements championed democracy and science, personified as "Mr Democracy" and "Mr Science" and associated with American philosopher John Dewey who was visiting China at the time.[5] After graduating from the National Central University in 1934, Wu worked with Dr. Gu Jing-Hui, the first Chinese woman to earn a Ph.D. in physics, who became an important mentor.[6] Gu had studied spectroscopy under David Dennison at the University of Michigan. At the Institute of Physics, Academia Sinica, in Shanghai, Wu and Gu shared a lab that was divided into a darkroom and a reading room. Focused on studying gas spectra at low temperatures, they spent long hours refining equipment, purifying gases, and achieving high vacuums. Wu's dedication was unmatched—she often skipped meals and forgot to rest, even on Sundays, emerging from the darkroom only when her colleagues came looking for her.[7]Her early exposure to liberal ideals and her admiration of Gu inspired her to study physics in America.

Following Gu, Wu successfully secured an offer to pursue a Ph.D. in physics at the University of Michigan. In the summer of 1936, she boarded the steamship S.S. President Hoover for the long voyage across the Pacific, arriving in San Francisco on August 26.[8]She waved goodbye to her parents believing she would return in just a few years. It was the last time she saw them.

Like many Chinese intellectuals of her generation, Wu viewed America as a bastion of liberal modernity—of freedom, equality, and progress. Unfortunately, life in America proved

[5]"德先生" (Mr. Democracy) and "賽先生" (Mr. Science) were called to replace "Mr. Confucius".

[6]顧靜徽 (1900-1983) or Ku Zing-Whai. She came to the University of Michigan as a Barbour Scholar in 1928. The Barbour Scholarship for Oriental Women, established in 1917 by Levi Barbour, aimed to "bring girls from the Orient, give them an Occidental education and let them take back whatever they find good and assimilate the blessings among the peoples from which they come." Carl W. Rufus, "Barbour Scholars Around the World," unpublished paper (1937), Bentley Historical Library, University of Michigan.

[7]Undated article in Chinese by 程崇道 (Cheng Chong-Dao), "我心中的吳健雄博士". C.S. Wu Papers, Box 9, Folder 22, CUA.

[8]Action Sheet, First Day Landings, Immigration Arrival Investigation Case Files 1884 – 1944, NAID: 28883982, National Archives.

less aligned with that ideal. Soon after arriving on the West Coast, Wu began hearing troubling reports from the University of Michigan—women were barred from entering the student union building.[9] Wu found this hard to reconcile with her idealization of the US.[10]

But Wu never went to Michigan. After arriving on the West Coast, she visited Berkeley and met with Raymond Birge, Ernest Lawrence, and J. Robert Oppenheimer, who urged her to enroll there despite the semester being underway.[11] Wu's decision to study at Berkeley proved both strategic and fortunate, not only because it was where she met her future husband, physicist Chia-Liu Yuan, but also because it allowed her to enter the burgeoning field of nuclear physics.

At the time, Berkeley was a hub for nuclear physicists. Lawrence, her advisor, had recently constructed the first cyclotron at his Radiation Laboratory, ushering in what would later be called the "big science" era. By the time Wu defended her thesis in 1940, Lawrence was already a Nobel laureate and the most celebrated American-born physicist, even profiled on the cover of *TIME*.[12] Two other members of Wu's thesis committee, Glenn Seaborg and Luis Alvarez, would also later receive Nobel Prizes in 1951 and 1968.

Wu was a standout student, earning top grades at Berkeley. Yet, despite her exceptional work, Wu received no job offers after graduation—a stark contrast to her male peers. Instead, Wu found herself in the spotlight of newspaper articles that emphasized her gender and ethnicity, often with sensational and patronizing headlines. A 1941 Oakland Tribune article appeared under the headline, "Petite Chinese Girl Shows Research in Atom Smashing,"[13] while the Indianapolis Star ran one titled, "Petite Chinese Girl Is Able Scientist: Miss Wu Appears Shy, But She Can Talk About Atoms."[14] For Wu, who understood herself first and foremost as a physicist, this must have been a profound cultural shock. In the United States, she was seen first as a "petite Chinese girl," and only second as a scientist.

[9]The University of Michigan was co-educational at the time, but women were barred from the student union building, which hosted most student activities.

[10]Zhu (2001, p. 88).

[11]Zhu (2001, p. 98-99).

[12]*Time*, November 7, 1937.

[13]Oakland Tribune, April 25, 1941.

[14]The Indianapolis Star, May 11, 1941.

4.2.2 From Berkeley to the Manhattan Project

Her difficulty securing a position after earning her Ph.D. was due in part to the anti-Asian racism that structured American life at the time. Anti-Chinese discrimination had deep roots, most visibly in the Chinese Exclusion Act (1882–1943), the first major U.S. law to bar immigration on an explicitly racial basis against a national group. In California, Asian American children, like Black and Native American children, were barred from white public schools. Worse, for a period in the 1870s, extending into the early 1880s, when the state authorized separate schools for Black and Native American children, it left Chinese children without any clear provision for state-funded schooling, despite the fact that their parents contributed taxes to support the system. During World War II, the prejudice against Asians sharpened further as anti-Japanese hostility spilled over onto all Asians, including Chinese and Chinese Americans. Reflecting on the aftermath of Pearl Harbor, Chinese American veteran Wing Lai recalled: "We were kind of scared at that time, because we looked Japanese, and they discriminated [against] Japanese at that time."[15] On the West Coast in particular, fear and suspicion reached a fever pitch: Japanese Americans were forced into internment camps, and Asians of all backgrounds were viewed with distrust.

The irony was that during World War II, China was an ally of the United States, fighting the same enemy: Japan. The second Sino-Japanese war, which began in 1937 and later merged into World War II, brought devastation to Nanjing, where Wu had studied. In December 1937, Japanese forces entered the city and massacred 200,000 people in atrocities later known as the Nanjing Massacre, or the Rape of Nanjing.[16] In what became one of the most horrific episodes of wartime violence in the twentieth century, women and girls were systematically raped, tortured,

[15]Wing Lai clarified, however, that he did not face any actual threat, since others could tell that he was Chinese. Wing Lai (黎玉榮), 14th Air Service Group, 555 Air Service Squadron, China-Burma-India theater (also known as the Flying Tigers), interviewed by David Cheng Chang (常成), January 24, 2024, San Francisco; material provided by Andy Lai (賴恩典).

[16]The number of victims remains a matter of scholarly debate. The Nanjing Massacre Memorial Hall's figure of 300,000 is often regarded as too high, while conservative Japanese estimates of 30,000 to 40,000 are widely regarded as too low. Some historians place the figure at around 200,000.

and often killed, in some cases after acts of genital mutilation. Elsewhere in China, Japanese counterinsurgency campaigns turned the countryside into what were effectively "unpopulated zones," under the genocidal policy known as the "Three Alls" (sankō sakusen): "kill all, burn all, loot all."[17] The war lasted eight years and claimed an estimated twenty million Chinese lives. During this time, Wu lost all contact with her family.

Wu considered returning to China to join the war effort. She even attempted to send a package of medical supplies to her family, but it was returned.[18] Hu Shih, then China's ambassador to the United States and Wu's former college professor, who had taught her Chinese philosophy, advised her to remain where she was. He urged her to concentrate on her studies and stressed that scientists would be essential to rebuilding China after the war.[19] Persuaded by her former teacher, Wu stayed in the United States and devoted herself to her work.

There was, of course, another reason why Wu couldn't find a faculty position after graduating top-class from Berkeley: she was a woman. At the time, many universities in the U.S. were not co-educational, which turned out to be a convenient excuse not to hire a female instructor. Lawrence wrote strong, albeit patronizing, letters for Wu. To Henry Smyth, chair of Princeton's physics department, Lawrence wrote,

> She is, in my judgment, the ablest woman physicist that I have ever known. She has a very sound knowledge of physics, and is extraordinarily able as an experimenter. At the same time, she is a charming person, and altogether a decorative addition to any laboratory. I think the Princeton boys would be quite charmed with her as an instructor.
>
> Nothing would please me more than to get her back here [in Berkeley] next year, but of course I know this is out of the question because of her husband's position in the East with RCA.[20]

---

[17] Although the term "genocide" has rarely been used for Imperial Japan's atrocities in China, apart from notable exceptions such as Iris Chang (1997)'s *The Rape of Nanking: The Forgotten Holocaust of World War II*, Gavan McCormack (2003) argues that there is a case for applying the term to at least some of Japan's actions if one adopts a broad reading of the 1948 UN definition of genocide.

[18] Chiang (2013, p. 105).

[19] Shu Chien, interview with the author, April 2, 2025.

[20] Letter to Henry Smyth, March 23, 1943. Carton 18, Folder 36, Ernest O. Lawrence Papers, the Bancroft Library, UC Berkeley (henceforth Berkeley).

Lawrence was plainly trying to help Wu obtain a faculty post. Yet his description of her as "a decorative addition to any laboratory," and his appeal to how "Princeton boys" might respond to her, also reveals the pervasive sexism and the broader social structures that shaped women's experience in academia. Lawrence was exceptionally well connected and wrote letters to multiple institutions. But even with the enthusiastic support of a Nobel laureate, Wu confronted formidable institutional barriers. William W. Watson, chair of Yale's physics department, replied to Lawrence:

> The only trouble is that since Yale is not a co-educational institution it is rather difficult to use women instructors. However, for the Army and Navy Training Program they may be quite acceptable.
>
> We have been short of instructors, of course, and for the Army and Navy teaching we will need a number of extra instructors. The only way we have been able to get along during the past year has been to promote the most eligible graduate students to instructorships...[21]

Watson's response lays the hierarchy bare. A male graduate student without a doctorate was regarded as a more suitable appointment than a woman who already held a Ph.D. and had an established research record. Women were not treated as part of the ordinary academic pipeline; their employment was acceptable only when wartime necessity made it expedient.

Lawrence initially offered Wu a research position at Berkeley after her graduation, but a university comptroller soon informed him that, as a non-citizen, Wu was "not eligible for employment by the University" and that "immediate steps should be taken to dismiss this employee."[22] Despite this bureaucratic roadblock, Lawrence appears to have taken measures to make Wu stay in America. In a letter to a special assistant to the Attorney General in the Immigration and Naturalization Service, Lawrence cited "a stringent shortage of personnel" and requested that Wu's employment be permitted "at least for the duration of the war."[23]

[21] Letter from William (Bill) Watson, March 29, 1943. Carton 18, Folder 36, Ernest O. Lawrence Papers, the Bancroft Library, Berkeley.

[22] J.H. Corley's letter to Lawrence, July 29, 1941. Carton 18, Folder 36, Ernest O. Lawrence Papers, the Bancroft Library, Berkeley.

[23] Letter to Lemuel B. Schofield, March 2, 1942. Carton 18, Folder 36, Ernest O. Lawrence Papers, the Bancroft Library, Berkeley.

The wartime demand for physicists was acute. In 1939, the American Physical Society had 3,600 members, and by the end of 1941, approximately 1,700 physicists were engaged in defense-related research.[24] The shortage certainly worked in Wu's favor. In 1942, she secured a teaching job at Smith College, a women's college in Northampton, where she became its first Chinese instructor.[25]

A year later, as the Roosevelt administration repealed the Chinese Exclusion Act in a symbolic move to bolster the wartime alliance with China, Wu started teaching at Princeton University for the Navy's training program. Lawrence, recognizing that her talents were being underused in the classroom, encouraged her to pursue war-related research, suggesting it would not only benefit her career but also make better use of her expertise.[26] To support her transition, he wrote a recommendation letter to John R. Dunning, head of Columbia's Manhattan Project team. Lawrence also completed a War Department investigation form rating her "Excellent" in all categories, including "Character," "Reputation," "Integrity," "Personal Habits," and "Family Background," with her "Loyalty" marked "Unquestionable."[27] In 1944, Wu was called for an interview by the Columbia's Division of War Research and hired immediately.

The Division of War Research must have known they were lucky to secure her. During graduate school, Wu had already made significant contributions to the emerging field of nuclear fission, which had to be kept secret for reasons of national security. In December 1938, Otto Hahn and Fritz Strassmann discovered that splitting uranium nuclei released immense energy. In early 1939, Lise Meitner and her nephew Otto Frisch explained and named the process "nuclear fission," borrowing the concept from biology. At Berkeley, Wu quickly began studying the lifetimes of noble gases emitted during fission. With World War II on the horizon, however, scientists agreed to keep certain research classified. Wu's findings were not published.

Behind closed doors at Berkeley, though, secrecy did not diminish the excitement.

[24]Howes and Herzenberg (1999, p. 35).

[25]Alumnae Quarterly, November 1942. Smith College Archives.

[26]Letter to Wu, March 23, 1943. Carton 18, Folder 36, Ernest O. Lawrence Papers, Bancroft, Berkeley.

[27]Confidential letter from the Internal Security Division, Investigation Branch. Carton 18, Folder 36, Ernest O. Lawrence Papers, Bancroft, Berkeley.

Recognizing the significance of her work, Oppenheimer asked Wu to present her findings at the department's Monday colloquium, held in the basement of what was then called LeConte Hall. She traced the mechanics of fission, hinted at the danger of a chain reaction, then closed with a wry remark: "Now I will stop here. I will go no further," drawing laughter from Lawrence and Oppenheimer.[28]

She did not stop. Her prolific post-war output speaks for itself. Between 1946 and 1949, she published over 20 papers. Wu's research on nuclear fission left a strong impression on the Berkeley community. Emilio Segrè, her professor and friend, borrowed her materials when preparing to speak on the subject.[29] Whenever discussions arose about nuclear fission, Oppenheimer always recommended involving Wu and said that she knew everything about neutrino cross sections.[30]

Why, then, was Wu never brought to Los Alamos? One possibility is that her status as a noncitizen complicated access to security clearance. But that cannot be the whole explanation: other foreign-born physicists, including Emilio Segrè and Otto Frisch, did work there. Another possibility is that Wu, as a Chinese woman in wartime America, was subject to forms of racialized suspicion. The answer remains elusive—precisely the kind of absence that, in Galison's terms, marks the inner circle of wartime secrecy.[31] In this calculus, secrecy was as much a tool of exclusion as of protection.

Excluded from Los Alamos, Wu nonetheless made significant contributions to the Manhattan Project. In March 1944, she joined the Columbia team led by John R. Dunning and Harold Urey.[32] They focused on developing the gaseous diffusion method for separating uranium isotopes (U-235 from U-238), a critical step in producing enriched uranium for the atomic bomb. Her expertise reached beyond Columbia and proved decisive at the Hanford site in Washington State, even though she never set foot there.

[28] Gloria Lubkin, "Chien-Shiung Wu, the First Lady of Physics Research," Smithsonian (January 1971).
[29] Chiang (2013, p. 125).
[30] Ibid.
[31] See Secrecy (USA: Redacted Pictures, 2008), by Peter Galison and Robb Moss.
[32] Wu, letter to Lawrence, April 19, 1946. Carton 18, Folder 36, Ernest O. Lawrence Papers, Bancroft, Berkeley.

Built in 1943 and modeled on Enrico Fermi's first atomic pile at the University of Chicago, the Hanford reactor suffered a puzzling shutdown soon after startup. The chain reaction faltered, then stopped entirely. Observing the timing of the failure, Fermi suspected a fission byproduct was capturing the neutrons needed to sustain the reaction.[33] "Ask Miss Wu," he was told.[34] Drawing on her earlier unpublished work at Berkeley, Wu identified the culprit: xenon-135, a neutron-absorbing isotope of the noble gas, acting as a reactor "poison." With this diagnosis, engineers adjusted the design, and the reactor returned to operation.[35]

After eight years without contact, the end of the war brought relief: her parents and brothers were alive. Yet fighting quickly reignited between the Kuomintang and the Chinese Communist Party, Wu's father urged her to stay away.[36]

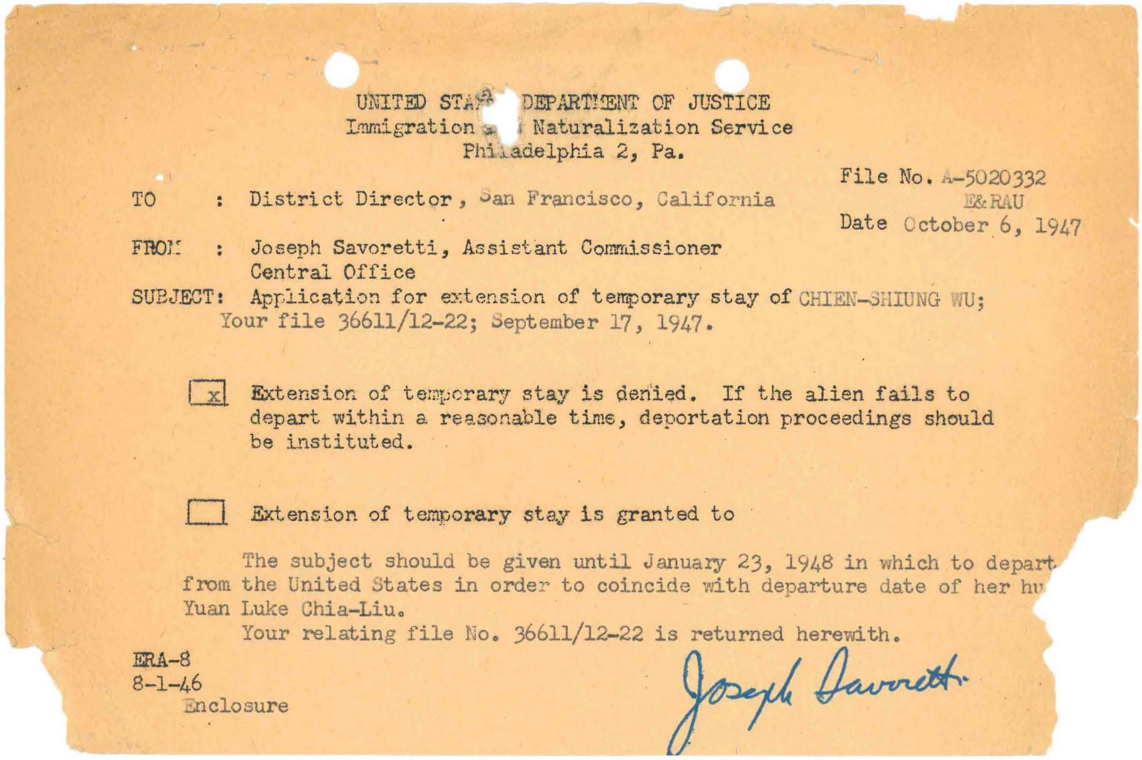
Wartime research became a turning point in Wu's career. It enabled her to break the glass ceiling in two significant ways. First, she could engage in research once more, a privilege predominantly reserved for her male peers at the time. After the war, physics departments in American universities experienced a significant influx of funding. Columbia asked Wu, as one of a few Manhattan Project physicists, to remain at the university to continue her research.

Second, her contribution to the Manhattan Project enabled her to prove her "loyalty" as a Chinese immigrant living in the U.S. during and after WWII. In December 1947, John Dunning addressed a letter "To Whom It May Concern," presumably to immigration authorities:

> If you will refer to previous correspondence regarding her case, you will find letters from General L. R. Groves, then head of the Manhattan Engineer District Atomic Bomb Project, Dean G. B. Pegram, Professor I. I. Rabi, and others, including myself, attesting to her past and ongoing contributions to this country.
>
> Dr. Wu is of the highest moral character and personal integrity...Retaining her service to this country is of vital importance.[37]

A year earlier, General Groves had written to the District Director of Immigration and Natural-

[33] Segrè (1970, p. 143).
[34] Lubkin, "Chien-Shiung Wu, the First Lady of Physics Research"; Chiang (2013, p. 130); Howes and Herzenberg (1999, p. 45).
[35] Ibid.
[36] McGrayne (1998, p. 266).
[37] John R. Dunning, December 19, 1947. C.S. Wu Papers, Box 7, Folder 6, CUA.



UNITED STA[illegible] DEPARTMENT OF JUSTICE
Immigration [illegible] Naturalization Service
Philadelphia 2, Pa.

File No. A-5020332
E&RAU
Date October 6, 1947

TO : District Director, San Francisco, California

FROM : Joseph Savoretti, Assistant Commissioner
Central Office

SUBJECT: Application for extension of temporary stay of CHIEN-SHIUNG WU;
Your file 36611/12-22; September 17, 1947.

[x] Extension of temporary stay is denied. If the alien fails to depart within a reasonable time, deportation proceedings should be instituted.

[ ] Extension of temporary stay is granted to

The subject should be given until January 23, 1948 in which to depart from the United States in order to coincide with departure date of her hu[illegible] Yuan Luke Chia-Liu.

Your relating file No. 36611/12-22 is returned herewith.

ERA-8
8-1-46
Enclosure

Joseph Savoretti

Figure 4.1. 1947 Record noting the denial of Wu's temporary-stay extension, National Archives at San Francisco

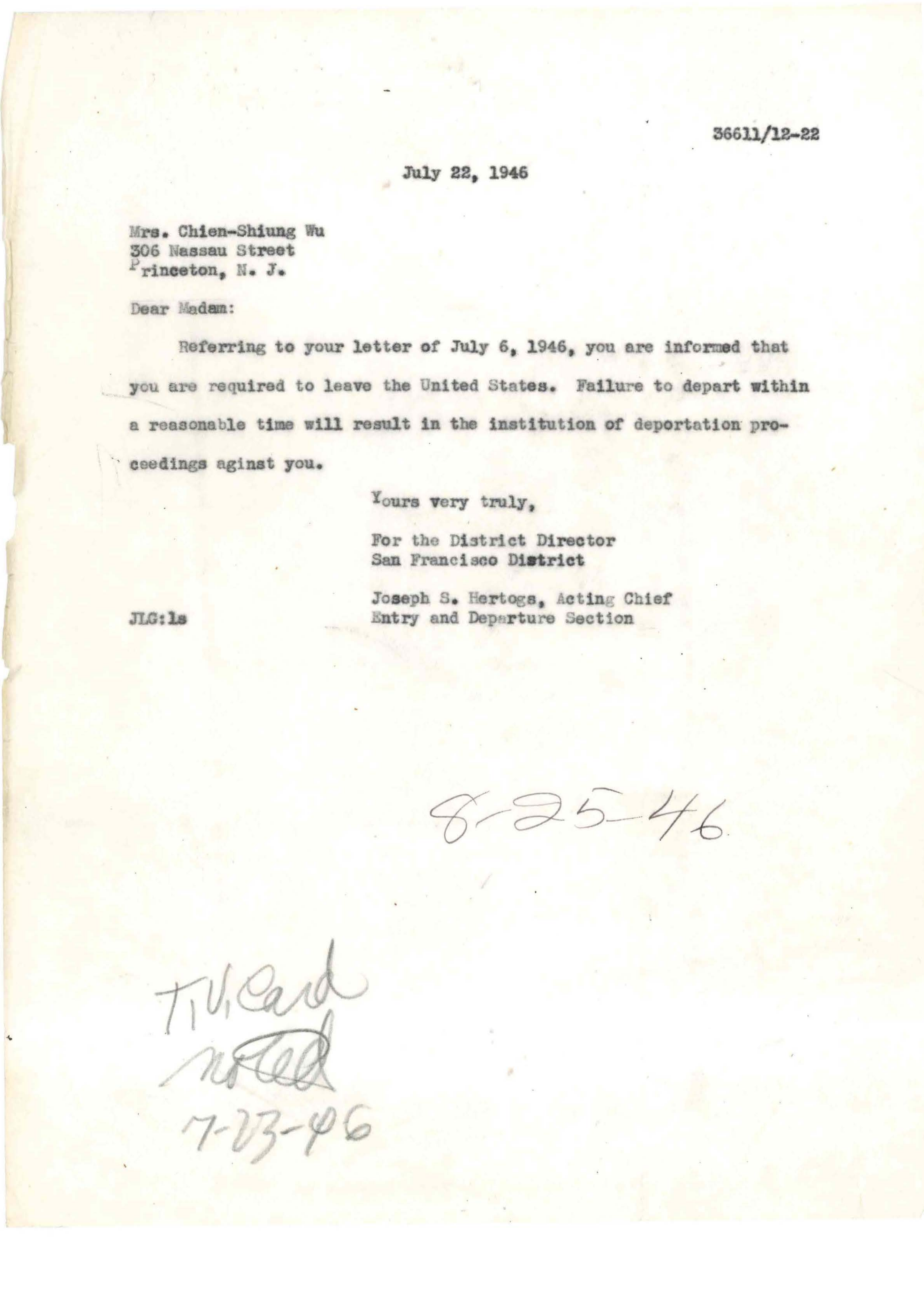
ization, calling Wu "a real asset to the United States" that "could not soon be replaced."[38] He further emphasized that "a serious loss to atomic research in this country will result if Dr. Wu is not permitted to stay here" (see Figure 4.3, 4.4)[39].

Yet even with a letter from the Manhattan Project's Director—arguably as far inside the bureaucratic inner circle as one could get—Wu's extensions were far from assured. Like many Chinese immigrants, she was required to prove her worth repeatedly, enduring a cycle of denied and reapplied "temporary stay" renewals (see Figure 4.1, 4.2).[40]

[38]Leslie Groves, October 17, 1946. Immigration Arrival Investigation Case Files 1884 – 1944, NAID: 28883982, National Archives.

[39]Ibid.

[40]Immigration Arrival Investigation Case Files 1884 – 1944, NAID: 28883982, National Archives (with unrestricted use).



36611/12-22

July 22, 1946

Mrs. Chien-Shiung Wu
306 Nassau Street
Princeton, N. J.

Dear Madam:

Referring to your letter of July 6, 1946, you are informed that you are required to leave the United States. Failure to depart within a reasonable time will result in the institution of deportation proceedings aginst you.

Yours very truly,

For the District Director
San Francisco District

Joseph S. Hertogs, Acting Chief
Entry and Departure Section

JLG:ls

8-25-46

T.V. Card
noted
7-23-46

Figure 4.2. 1946 letter to Wu from the San Francisco District immigration office, National Archives at San Francisco

WAR DEPARTMENT
P. O. Box 2610
WASHINGTON, D. C.

REFER TO FILE No. WL-41

17 October 1946

District Director of Immigration and Naturalization
70 Columbus Avenue
New York 25, New York

Dear Sir:

On 27 June 1946 the San Francisco District Office of the United States Department of Justice Immigration and Naturalization Service forwarded a letter, a copy of which is enclosed as Enclosure (a) to Dr. Chien-Shiung Wu at Princeton, New Jersey.

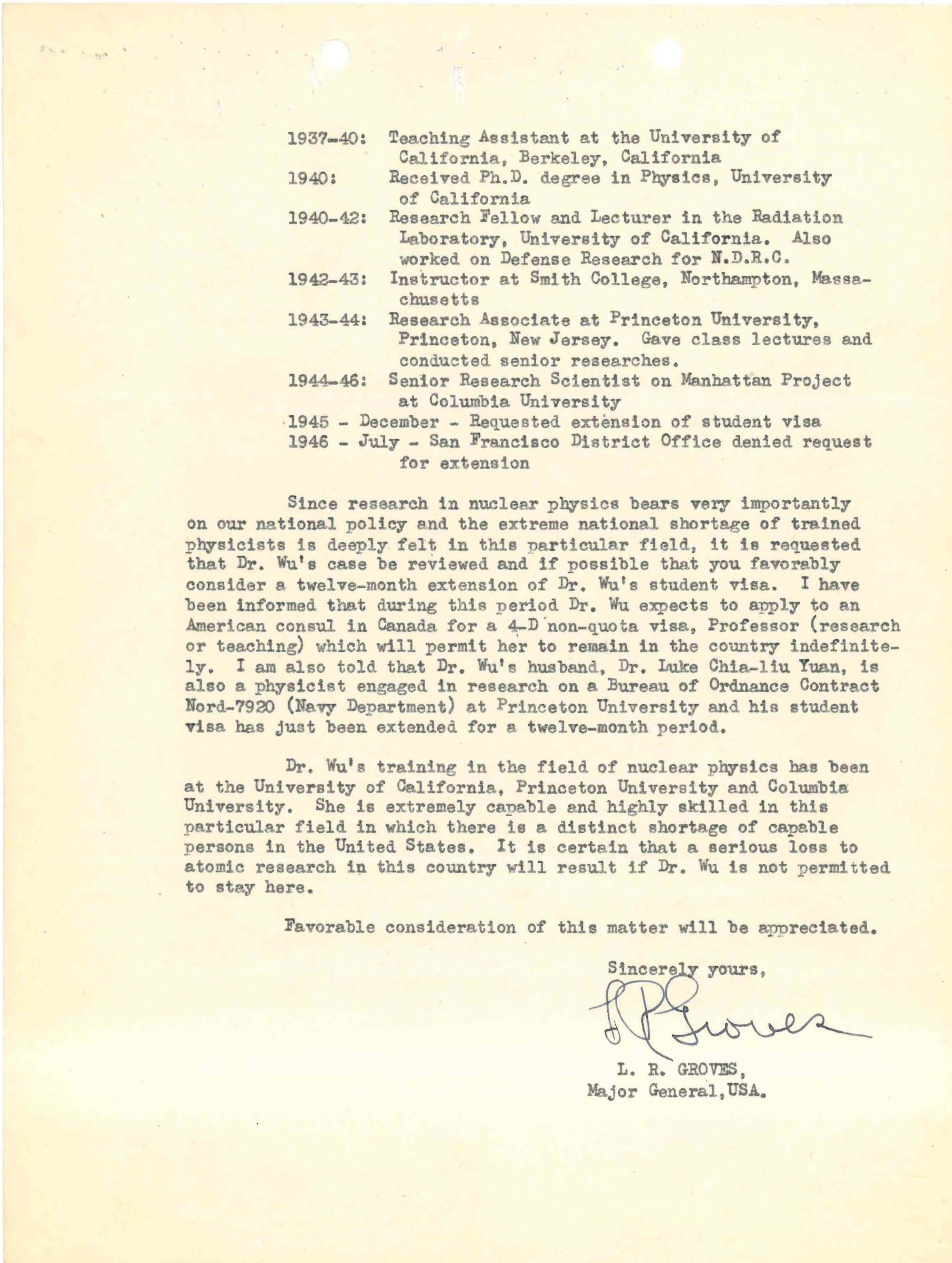
Dr. Wu is a Ph.D. physicist engaged in full-time research at Columbia University on a War Department Contract No. W-31-109-Eng-17, on the fundamental problems in nuclear physics related to the development of atomic energy. She has been actively engaged in such important investigations as neutron interaction with matter, the determination of nuclear energy levels by using beta-ray spectrometer in conjunction with the coincidence method and the development of radiation detection devices. Further information and refinement on these particular subjects are most essential for the real understanding of the nuclear core where most of the atomic energy is locked up. Dr. Wu also devotes part of her time to supervising research by graduate students of Columbia University in the field of nuclear physics, in cooperation with research programs of Columbia University. Her ability, training and experience are such that she could not soon be replaced. Her presence in this country is a real asset to the United States.

In brief, Dr. Wu's education and experience in China and the United States are outlined below. Conditions of her entry on a student visa and retention as an alien during the war are also outlined:

Chien-Shiung Wu   Alien Registration No. 5020332

1934: B.S. Physics, National Central University, Nanking, China
1935: Instructor at National Chekiang University, China
1936: Research Assistant at National Academia Sinica, China
1936: Applied for and received a student visa to enter the United States

Figure 4.3. 1946 Letter from Leslie Groves to District Director of Immigration and Naturalization (part 1), National Archives at San Francisco



1937-40: Teaching Assistant at the University of California, Berkeley, California

1940: Received Ph.D. degree in Physics, University of California

1940-42: Research Fellow and Lecturer in the Radiation Laboratory, University of California. Also worked on Defense Research for N.D.R.C.

1942-43: Instructor at Smith College, Northampton, Massachusetts

1943-44: Research Associate at Princeton University, Princeton, New Jersey. Gave class lectures and conducted senior researches.

1944-46: Senior Research Scientist on Manhattan Project at Columbia University

1945 - December - Requested extension of student visa

1946 - July - San Francisco District Office denied request for extension

Since research in nuclear physics bears very importantly on our national policy and the extreme national shortage of trained physicists is deeply felt in this particular field, it is requested that Dr. Wu's case be reviewed and if possible that you favorably consider a twelve-month extension of Dr. Wu's student visa. I have been informed that during this period Dr. Wu expects to apply to an American consul in Canada for a 4-D non-quota visa, Professor (research or teaching) which will permit her to remain in the country indefinitely. I am also told that Dr. Wu's husband, Dr. Luke Chia-liu Yuan, is also a physicist engaged in research on a Bureau of Ordnance Contract Nord-7920 (Navy Department) at Princeton University and his student visa has just been extended for a twelve-month period.

Dr. Wu's training in the field of nuclear physics has been at the University of California, Princeton University and Columbia University. She is extremely capable and highly skilled in this particular field in which there is a distinct shortage of capable persons in the United States. It is certain that a serious loss to atomic research in this country will result if Dr. Wu is not permitted to stay here.

Favorable consideration of this matter will be appreciated.

Sincerely yours,

L. R. Groves

L. R. GROVES,
Major General, USA.

Figure 4.4. 1946 Letter from Leslie Groves to District Director of Imigration and Naturalization (part 2), National Archives at San Francisco

4.3 Breaking Parity Against Disparity

Wu's postwar years in America were marked by scientific achievement, but also political turbulence. After the war, she and her husband welcomed their only child, Vincent Wei-Cheng Yuan. The couple had in fact planned to return to China to take up professorships at National Central University in Nanjing. But after the Communist victory in 1949, they decided not to raise their son under the new regime.[41]

However, they soon felt the chill of McCarthyism, also known as the Second Red Scare. Beginning in 1950, their friend Qian Xue-Sen, a brilliant Chinese physicist, became the target of the FBI's communist witch-hunt. For five years, despite the absence of any concrete evidence against him, Qian and his family lived under partial house arrest and constant surveillance, before he was deported to China in exchange for American pilots captured during the Korean War.[42] Even Albert Einstein, who visited Wu in hospital in Princeton after the birth of her son, was denounced by McCarthy as "an enemy of America" in 1953 for urging witnesses to refuse to testify in defense of academic freedom.[43] Wu was even more shaken when she learned of Oppenheimer's 1954 security clearance hearing, which probed his alleged communist ties. Reflecting on the event, she said, "Oppenheimer was the 'Father of the American Atomic Bomb.' Since even he was suspected, who else could escape being suspected?"[44]

As suspicions of communist ties spread through the physics community, Wu moved to protect herself—first by obtaining permanent residency, and later by becoming a U.S. citizen.

[41] McGrayne (1998, p. 266).

[42] Ironically, Qian was likely anti-communist at the time he came under suspicion. In 1949, after the Chinese Communist Party took control of the mainland, he applied for American citizenship, and his wife was the daughter of a prominent military strategist associated with Chiang Kai-shek's Nationalist government. Only after his forced return to China did he publicly align himself with the new regime; if he was to continue doing science at all, he had little choice. He later became known as the father of China's missile program. See Chang (1996) and Geppert and Liu (2025).

[43] See the *New York Times* article, "Einstein Criticized: M'Carthy Says Giver of Such Advice Is 'Enemy of America," June 14, 1953, p.30. See also the Institute for Advanced Study History Working Group document, "A Refuge for Scholars," by Bardawil et al. (2017). Einstein's visit to Wu was confirmed both by Wu's husband in an interview by Zhu (2001, p. 438) and by Wu's granddaughter in Yuan (2021).

[44] Zhu (2001, p. 479-480).

This decision was also pragmatic: international travel for conferences proved particularly challenging with a Chinese passport, a difficulty that many Chinese scholars still encounter today. At the time, the Chinese Exclusion Act had been replaced by the Magnuson Act of 1943, which limited Chinese immigration to a quota of just 105 individuals per year. Washington was then divided between deporting Chinese students and scholars or forcing them to stay, especially those with access to classified knowledge. Wu perhaps fell into the latter category. According to a letter signed by Franklin D. Roosevelt Jr., her permanent residence status was approved by the Senate on May 5, 1953.[45] Wu officially became a naturalized citizen on June 28, 1954.[46]

Political pressures weren't the only obstacles Wu faced. At Columbia, where she had remained after the war, her path to tenure was anything but smooth. Despite her standing as one of the most accomplished experimentalists, her promotion faced resistance. Isidor Rabi, a Nobel laureate with considerable influence in the department, held the belief that women were biologically unsuited to excel in science.[47] In a 1951 faculty meeting, when Willis Lamb proposed Wu's advancement to tenured associate professor, Rabi objected, remarking that "women tend to become argumentative as they mature".[48] He added that Columbia would "always have Chien-Shiung," suggesting that no other institution would hire a woman and that she was too settled in New York to leave.[49]

Lamb's disagreements with Rabi reportedly contributed to his decision to leave Columbia for Stanford in 1951.[50] His protest may have carried some weight, however: in 1952, Wu became the first woman in Columbia's physics department to secure tenure, after eight years on the faculty with more than thirty publications. She continued to focus on beta decay, the subject of

[45]Franklin D. Roosevelt Jr., letter to R.W. Siegel, May 6, 1953. C.S. Wu Papers, Box 7, Folder 6, CUA.

[46]R.W. Siegel, letter to U.S. Atomic Energy Commission, December 16, 1954. C.S. Wu Papers, Box 7, Folder 6, CUA.

[47]Rabi said, "I'm afraid there's no use quarreling with it, that's the way it is. Women may go into science, and they will do well enough, but they will never do great science" (Gornick, 1983, p. 36). See also Rigden (2000, p. 117).

[48]Zhu (2001, p. 435).

[49]Julia Steinberger, email correspondence with the author, January 11, 2025. Julia Steinberger recounted stories her father, Jack Steinberger, Wu's friend and colleague at Columbia, shared about Wu.

[50]Ibid.

her dissertation. By the spring of 1956, when Tsung-Dao Lee consulted her on the possibility of parity violation, she was already recognized as the authority on the subject.

4.3.1 The Experiment(s)

This section traces the history of the parity violation experiment, later known as the Wu experiment. Given space constraints, the technical details of the experiment are treated extremely briefly; these aspects have already received extensive coverage in the existing literature.[51] The emphasis here is instead on the less visible behind-the-scenes negotiations and power dynamics.

Roughly speaking, a parity transformation is mathematical operation that simply flips the signs of spatial coordinates, effectively mirroring the physical system. For instance, a position vector r becomes $-r$ under parity transformation. The same applies to linear momentum ($p \rightarrow -p$), and acceleration ($a \rightarrow -a$) etc. However, angular momentum $L = r \times p$ does not change its sign, since as the double reversal of r and p cancels out. Similarly in the quantum case, spin angular momentum does not change under parity transformation. Parity symmetry, or the symmetry between the left and the right, means that the laws of physics remain the same under the parity transformation. Newton's second law of motion $F = ma$ is invariant under parity transformation (yielding $-F = -ma$), which means the law does not distinguish right-handed coordinate systems from left-handed coordinate systems.

Unlike continuous symmetries, which yield conserved quantities such as energy and momentum via Noether's first theorem, parity symmetry is a discrete symmetry that lacks a well-defined conserved quantity in classical mechanics. In quantum mechanics, however, parity can be formally defined through a parity operator and corresponding eigenvalues, allowing it to be treated as a conservation law. As a result, the terms parity symmetry and parity conservation are often used interchangeably. For many years, it was assumed that all fundamental physical processes conserved parity. This view of parity as an absolute symmetry was challenged and ultimately refuted by a series of experiments in the 1950s that showed parity violation in weak

[51] See, for example, Franklin (1979).

interactions.

The story of parity violation began with the so-called $\tau - \theta$ puzzle, a perplexing problem in particle physics. In the late 1940s and early 1950s, cosmic ray experiments revealed the existence of spin-0 mesons. These particles appeared very similar in most respects, particularly in their mass and lifetime. Based on their decay products, these particles were named: θ^+ (theta-plus), which decayed into two pions (one π^+ and one π^0) and τ^+ (tau-plus), which decayed into three pions (two π^+ and one π^0). Both θ^+ and τ^+ were believed to be the same particle but with different decay modes. However, a problem arose when trying to assign their parity values. Based on what was known about pions and angular momentum conservation, the θ-decay mode implied that the particle had positive parity, but the τ-decay mode implied that the particle had negative parity.[52] How could a single particle have two different parities?

In 1956 Tsung-Dao Lee and Chen-Ning Yang contemplated a bold hypothesis as a solution to the $\tau - \theta$ puzzle: parity conservation might not hold in weak interactions. Lee was Wu's colleague at Columbia. In early spring of 1956, he visited Wu's office and discussed the possibility of experimenting parity violation. Wu suggested polarized Cobalt-60 as beta-source and lent Lee a book on beta decay edited by Kai Siegbahn. After surveying existing evidence based on the book, Lee and Yang found that the law of parity conservation was experimentally supported in the electromagnetic and the strong interactions, but there was an absence of experimental data in weak interactions.[53]

Skepticism towards parity violation was universal, however. Yang later admitted, "I was not betting on parity nonconservation; Lee was not betting on parity nonconservation."[54] Wolfgang Pauli thought testing it was a waste of time, claiming that he would bet any amount of money on parity conservation.[55]. He later expressed relief at not formalizing the bet, "I can afford to lose some of my reputation, but not my capital."[56] Richard Feynman, however, did,

[52]See Dalitz (1954) and Fabri (1954).
[53]Lee (1988, p. 162).
[54]Yang (1982, p. 450).
[55]Weisskopf (1989, p. 164)
[56]Weisskopf (1989, p. 165)

losing a $50 bet to Norman Ramsey.[57] Experimentalists were unwilling to test the idea—one even dismissively joked that he would do the experiment if he found "a very bright graduate student to be a slave."[58]

To understand why so many physicists were confident in the conservation of parity, it is useful to consider the historical context. Nearly two decades before the parity experiment, a major theoretical development reshaped the understanding of beta decay: the birth of the neutrino. Early studies of beta decay showed that the energy and momentum of the emitted beta particle didn't add up to the initial energy and momentum of the decaying nucleus. This discrepancy prompted two competing responses. Niels Bohr was willing to entertain the radical idea that energy and momentum might not be conserved at the atomic scale or that conservation laws might only hold statistically. Wolfgang Pauli, in an attempt to rescue conservation laws, "invented" a new particle in 1930: a neutral, (nearly) massless particle with half-integer spin, emitted alongside the electron during beta decay. The idea was then developed in more detail by Enrico Fermi in 1934, who renamed Pauli's proposed particle (which Pauli initially called "neutron") "neutrino", after the name "neutron" was taken by the particle James Chadwick discovered in 1932.

The situation bore some resemblance to the later $\tau - \theta$ puzzle, where physicists faced a choice between abandoning the conservation of parity or positing that θ^+ and τ^+ were distinct particles. In the case of neutrino, one could either give up the conservation of energy and momentum or invent a new particle. It turned out that Pauli was right, and the conservation law was saved. Neutrino was discovered in 1956, more than twenty years after Pauli's initial proposal and right before Wu's experiment. Wu, an expert on beta decay and a longtime acquaintance of Pauli, was well-versed in the history of beta decay. When Lee approached her about the possibility of conducting an experiment on parity conservation, she was skeptical that parity could be violated. However, she recognized the value of the experiment. She thought it would be

[57] Feynman et al. (1985, p. 248).
[58] Yang (1982, p. 31).

worthwhile even if it confirmed parity conservation in weak interactions.

Moreover, she somehow sensed the urgency of the opportunity—she cancelled a long-planned trip to Europe and then China, which would have marked her first return to China since 1936.[59] "Our passages were booked on the HMS Queen Elizabeth before I suddenly realized that I had to do the experiment immediately, before the rest of the Physics Community recognized the importance of this experiment and did it first," she said.[60] Her husband who understood the time-sensitive nature of her experiment went alone.

The theoretical principle underlying Wu's experiment is straightforward. The key was to measure something that changes under parity transformation, such as linear momentum, relative to something that does not, such as the direction of spin. Cobalt-60 turns out to be a great beta-decay source to test parity violation. Roughly speaking, if parity were conserved, electrons should be emitted both in the direction of the Cobalt-60 nuclei's spin and opposite to it. But that is not what Wu found. The observed distribution was anisotropic: electrons were statistically more likely to be emitted opposite to the direction of the Cobalt-60 nuclei's spin than along it. More exactly, the experiment revealed a nonzero correlation between nuclear spin and electron momentum, indicating that the weak decay of cobalt-60 is not invariant under spatial reflection.

In June 1956, recognizing that Columbia's facilities could not reach the extremely low temperatures required, Wu initiated a collaboration with Ernest Ambler at the National Bureau of Standards (NBS). Ambler readily accepted but was away on vacation for two weeks in August, a delay that perhaps left Wu quietly frustrated with the slower pace at NBS.[61] By September, however, the project began to gather momentum. Ambler introduced her to his supervisor, Ralph P. Hudson, who soon decided to join the experiment. The team expanded further when Raymond

[59] It is unclear whether Wu and her husband had planned to visit Taiwan, mainland China, or both. Taiwan may, however, have been the more likely destination. They had left China when the mainland was still under Kuomintang rule, and although Wu still had family there, many of their intellectual associates were by then in Taiwan. Her husband's itinerary also suggests that Taiwan was in the original plan. In any case, travel to mainland China would have been politically fraught during the McCarthy era.

[60] Wu (1973, p. 104). Wu referred to the ship as "HMS Queen Elizabeth," but it was likely the RMS Queen Elizabeth, as the HMS Queen Elizabeth was Royal Navy's warship, an aircraft carrier.

[61] Wu (1973, p. 110).

Hayward and his research assistant, Dale Hoppes joined and contributed essential equipment, including a 10-channel pulse-height analyzer.[62]

The experiment was elegantly designed. It involved first polarizing Cobalt-60 nuclei (i.e., aligning their spins) in a cerium magnesium nitrate crystal using a strong magnetic field at low temperatures.[63]The beta decay of Cobalt-60 nuclei undergoes a transition to (an excited state of) Nickel-60 by emitting an electron and an antineutrino. This is a weak-interaction process (which involves turning a neutron to a proton, or equivalently, a down quark to an up quark):

$$^{60}_{27}\mathrm{Co} \rightarrow {}^{60}_{28}\mathrm{Ni}^{*} + e^{-} + \bar{\nu}_{e} \tag{4.1}$$

where $^{60}_{27}\mathrm{Co}$ is cobalt-60, $^{60}_{28}\mathrm{Ni}^{*}$ is the excited daughter nucleus nickel-60, e^{-} is the emitted electron, and $\bar{\nu}_{e}$ is the electron antineutrino. Cobalt-60 possesses a spin of 5, while Nickel-60 has a spin of 4. Consequently, to uphold the conservation of angular momenta, the emitted electron and neutrino must each possess a spin of 1/2 (given that electrons and neutrinos are fermions). Under a parity transformation, the linear momentum flips the sign, but the spin does not. Thus, whereas in the "real world", the electron emission was emitted in the opposite direction as the spin direction of Cobalt-60, in the "mirrored world" (one would expect that) the electron emission would be in the same direction as the spin direction of Cobalt-60. If parity were conserved, the mirrored image should have been as likely as the real-world image. But in the experiment when a parity transformation was applied by reversing the direction of the magnetic field (and thus the alignment of the Cobalt-60 spins), electrons exhibited a preference for orientations opposite to that of the nuclei spins.

It is worth noting that the direction of electron emission was not the only observable in the experiment. The excited nickel-60 nuclei emitted gamma rays as they transitioned to the

[62]Wu (1973, p. 110-111).

[63]The particular method is called the Rose-Gorter method, first carried out by Ambler et al. (1953).

ground state. This is an electromagnetic process (known to conserve parity):

$$^{60}_{28}\text{Ni}^* \rightarrow {}^{60}_{28}\text{Ni} + \gamma \tag{4.2}$$

where $^{60}_{28}$Ni is nickel-60 in its lower-energy state and γ is the emitted gamma ray. The emission of gamma rays was important because it serves as a reference for evaluating the distribution of emitted electrons. Since gamma rays are photons, parity conservation in electromagnetism ensures that the gamma rays released have an approximate isotropic distribution (i.e., a relatively uniform dispersion across all directions). The extent to which their distribution deviates from perfect uniformity, known as "anisotropy," provides a method for assessing the alignment of Cobalt-60 nuclei, specifically the alignment of their spins. The emission pattern of electrons can be compared to the emission pattern of gamma rays to determine if electrons emitted have a genuine preferred direction.

To summarize, the experiment tracked two key observables: the direction of electron emission and the angular distribution of the gamma rays, the latter functioning as a readout of the nuclei's orientation. The design was ingenious because it exploited two processes at once. The first was a weak-interaction process, the beta decay itself, which emitted electrons and antineutrinos. The second was an electromagnetic de-excitation process, which emitted gamma rays and provided an internal standard for the degree of nuclear alignment. By measuring the relation between the nuclei's spin orientation and the electrons' momenta, Wu and her collaborators could test whether the beta particles were emitted symmetrically. Any systematic asymmetry would constitute direct evidence of parity violation in beta decay.

The timeline of the discovery unfolded rapidly during the Christmas break in 1956.[64] Between the 19th and the 22nd of December, Wu's team observed promising but inconsistent beta-asymmetry signals. On December 27th, the first consistent beta asymmetry was detected,

[64] The discussion in the following two paragraphs is based on "One Researcher's Personal Account" by Chien-Shiung Wu, Richard Garwin and Valentine Telegdi in Adventures in Experimental Physics, Gamma Volume (p.101-144), as well as a chronology organized by Zhu (2001, p. 264-281).

though Wu, commuting between New York City and Washington, D.C., was not present. Ambler shared the news with Wu, who immediately informed Lee and Yang. On the 4th of January 1957, the asymmetry was confirmed, this time with Wu present to collect gamma-ray data. That same day, at Columbia's physics department luncheon, Lee announced that Wu's results indicated a substantial parity violation. Wu herself had not wished to publicize the finding so quickly; she was wary of premature disclosure.[65] After all, extraordinary claims required extraordinary evidence. Perhaps she was also guarded because of the competitive environment and the micropolitics of priority and prestige in science—and with good reason.

Upon learning the scale of the violation, far greater than anyone had expected, Leon Lederman realized that his ongoing muon experiment (otherwise difficult to test) could be adapted into a parity measurement. Within hours he contacted Richard Garwin and started their $\pi - \mu - e$ experiment. Using Garwin's advanced "electronic techniques" from IBM and apparatus built by Lederman's graduate student Marcel Weinrich, the team completed the experiment with remarkable speed.[66] By January 8, it demonstrated parity violation with a statistical certainty of 22 standard deviations. Lederman, Garwin and Weinrich drafted their Letter to the Physical Review.

Lederman then called Wu. It must have been a stressful moment for her, because her team still faced technical challenges with the experimental apparatus and needed more data for publication. Wu told Lederman that she needed a week to check her data. "It would not, however, have been appropriate for our results to precede in print the work of Professor Wu and her collaborators," Garwin later recalled.[67] Recognizing that their experiment had relied on Wu's preliminary results to begin with, the $\pi - \mu - e$ team waited. On January 12, 1957, Wu and her collaborators at NBS submitted a joint Letter to Physical Review, with the draft prepared in advance by Wu. On February 15, 1957, both the cobalt-60 and the $\pi - \mu - e$ experiments were published in Physical Review back-to-back, with the same submission date.

[65] Chiang (2013, p. 187)
[66] Lippincott (2007, p. 446).
[67] Garwin (1973, p. 127)

Interestingly, another group led by Valentine Telegdi and Jerome Friedman at the University of Chicago also attempted to publish their findings on parity violation in the same issue but were rejected due to insufficient data; their work appeared in the following issue.

4.3.2 The Prestige Politics of Science

At first glance, the Wu experiment seems a textbook example of Popperian falsification: a single, decisive experiment topples a long-standing law of physics. Yet the neat Popperian narrative—the lone experiment overthrowing a fundamental symmetry principle—was constructed *ex post*. In reality, three independent experiments converged on the same conclusion almost simultaneously. What complicates the Popperian picture is the accumulation of convergent results across multiple sites, each reinforcing the credibility of the others.

Lederman, Garwin, and Weinrich began their $\pi - \mu - e$ experiment only after hearing preliminary results from Wu's cobalt-60 measurements. By contrast, Telegdi and Friedman had embarked on a similar $\pi - \mu - e$ test soon after reading Lee and Yang's paper. Their progress, however, was slowed by their reliance on nuclear emulsions and by weak institutional support.[68] During Christmas 1956, Telegdi returned to Italy for his father's funeral; when he came back, Garwin told him of Columbia's results. Telegdi and Friedman immediately wrote a Physical Review Letter based on their pre-Christmas data.[69]

The editors rejected it. Telegdi suspected interference from the "Columbia University gang."[70] But Physical Review editor Samuel Goudsmit later explained that the decision rested on statistical grounds: their initial effect was "only a little larger than two standard deviations," insufficient for "such an important and subtle experiment."[71] Telegdi's frustration was understandable though—timing is often decisive in the prestige politics of science.

Columbia's own publicity strategy likely intensified those frustrations. On 15 January

[68] "Their experiment was also difficult and met with little enthusiasm from their colleagues," Yang (1982, p. 31) recalled.

[69] Telegdi (1973, p. 133-134).

[70] Chiang (2013, p. 195).

[71] Goudsmit (1973, p. 137).

1957, the university staged a press conference in Rabi's office to announce the discovery of parity violation. Lee, Wu, Ambler, Hudson, Hayward, Lederman, and Garwin attended. Rabi had initially wanted Ambler alone to represent the NBS scientists, but Deputy Director Robert Huntoon insisted all NBS participants be included.[72] Absent was Yang, who declined on the grounds that "the idea of a press conference to announce a scientific development was distasteful."[73] Columbia's strategy was effective: The New York Times ran a full-page story the next day before the papers had even been peer-reviewed, and many readers assumed Wu's experiment had been conducted at Columbia rather than at the NBS.[74]

The news quickly reached Pauli, who wrote to Wu twice in January 1957 with technical questions, remarking in the second letter: "I still do not understand why the strong interactions are then reflection invariant."[75] That puzzle—why strong interactions conserve parity while weak interactions do not—remains unsolved to this day.

Rapidly publicized, the result soon came to be cast as a "crucial experiment" in the Popperian sense—a single, decisive falsification of a universal law. According to Popper, the logic of scientific inference distinguishes falsification from confirmation. Universal statements can never be deductively confirmed, since no finite set of instances entails them, but they can be deductively refuted by a single counterinstance. One apparent anomaly is therefore sufficient, in principle, to overthrow a law. Yet history suggests that such distinction is not so clear-cut. Popper's account of falsification has long faced familiar objections: scientists seldom act as ruthless falsifiers, and theories are always tested alongside auxiliary assumptions (i.e., the Duhem–Quine thesis). Historical episodes such as the discovery of Neptune, where an apparent anomaly was explained away, and the non-discovery of Vulcan, where an anomaly prompted theory change, already show that Popperian falsification cannot be generalized. "Crucial experiments" like Eddington's 1919 eclipse observations and Wu's parity-violation experiment are often treated as

[72] Zhu (2001, p. 281).
[73] Yang (1982, p. 36).
[74] See the *New York Times* article, "Tex of Columbia Report on Physics Experiments," January 16, 1957, p. 24.
[75] Letter from Pauli, January 21, 1957. C.S. Wu Papers, Box 3, Folder 14, CUA. In the letter, Pauli also refers to an earlier letter "from two days ago".

rare vindications of Popper's view. Yet even these depart from the textbook picture.

In 1919, one of Eddington's telescopes recorded values closer to Newtonian predictions, which was dismissed as unreliable. In 1957, three parity experiments ran in parallel, each lending what might be called a Bayesian boost—incremental increases in confidence that weak interactions violate parity, with each result making the others more credible. Wu likely would not have published so quickly without knowing Lederman's findings supported hers, and Lederman would not have begun without word of Wu's early results. Interestingly, the tendency to see the overturning of parity as a vindication of Popperian falsification—of a single lone experiment over-turning the laws of physics—may have been reinforced by Rabi and Oppenheimer's efforts to link Wu's name indelibly to the result.

In October 1957, Wu was lecturing in upstate New York when she received a call from Oppenheimer. He waited on the line until she finished speaking, then broke the news. Lee and Yang had won the Nobel Prize.[76] What Wu said in reply is unknown. That she was both pleased and disappointed is almost certain, though the balance of the two emotions can only be guessed. Oppenheimer later hosted a banquet in Princeton to celebrate the achievements of Wu, Lee, and Yang. He arranged Wu's seat next to him, pointedly emphasizing her role in the discovery.[77] Oppenheimer was not alone in publicly emphasizing Wu's contributions. In the years that followed, she received multiple Nobel nominations from fellow physicists and was often cited as an example of the Matilda effect—Margaret Rossiter's term for the systematic erasure or downgrading of women's scientific achievements in favor of male colleagues.

Lee and Yang's receipt of the Nobel Prize was not itself surprising. What is striking is that they received it in 1957. The deadline for nominations was February 1, yet the experimental confirmation of parity violation was not published until February 15, 1957. Without empirical confirmation, it is unlikely that anyone would have nominated Lee and Yang solely on the basis of their theoretical proposal, especially given how controversial and initially unpopular

[76] Chiang (2013, p. 198).
[77] Ibid.

it had been. Even if Columbia's press conference in mid-January drew attention to the result, it would still have been unusual to base a nomination on a press announcement rather than a peer-reviewed publication. As the Nobel archives now reveal, however, John Simpson, a physicist at the University of Chicago, did nominate Lee and Yang before the experimental papers appeared in print. Since both Lee and Yang had taken their doctorates at Chicago, their work may well have been more visible there. In any case, Simpson evidently knew enough to submit a nomination letter in January that reached Stockholm in time.

Would Simpson have thought to nominate Wu as well? Probably not. As a Chicago colleague of Telegdi's, he was unlikely to view the result through the "Wu experiment" framing that later became familiar. However, unlike the other experimentalists who helped confirm parity violation, she had been involved at an early stage in shaping the experimental test itself, including suggesting cobalt-60 to Lee. She was also the only experimentalist acknowledged by name in Lee and Yang's original paper. Perhaps Wu's exclusion from the 1957 nomination was not simply a matter of timing. It also reflected the way scientific credit was already being distributed through professional and institutional networks.

Had Simpson's letter not reached Stockholm in time, Wu would at least have received two nominations (from Polykarp Kusch and Willis Lamb) the following year alongside Lee and Yang. Indeed, the Nobel archives show that Lee and Yang were also nominated for the 1958 prize, on the basis of nominations submitted during 1957, after the Wu experiment had appeared in print. Many had later come to believe that Wu, too, deserved the prize. More than three decades later, on his way to Stockholm to receive the 1988 Nobel Prize in Physics, Jack Steinberger wrote to Wu (Figure 4.5):

> I am off for Stockholm, and I am thinking of you. You should be going there, not me. Nobody has deserved that prize more than you and nobody has done more beautiful experiments.[78]

Wu never publicly said much about not having received a Nobel Prize, but she physically

[78] Letter to Wu, December 6, 1988, Carton 15, folder 17, Emilio Segrè papers, Bancroft, Berkeley

forwarded Steinberger's letter to his former teacher and her dear friend Emilio Segrè, who replied:

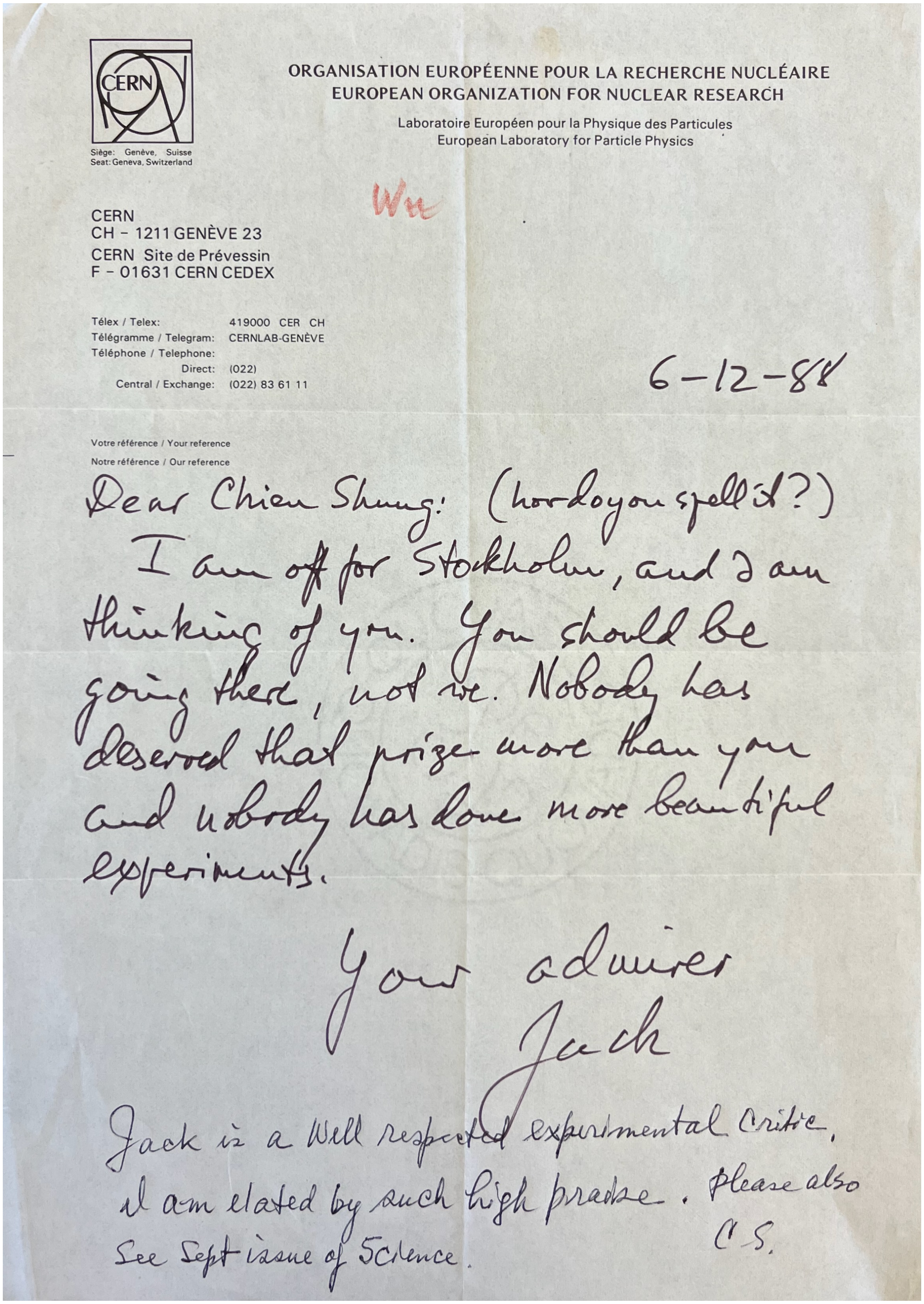
> Nobel Prizes or other honors do not improve past work. The asymmetry in Co decay is your monument and will last forever; nothing can add to or subtract from it. It will be reported in all physics books for many, many years to come, Nobel Prize or not. The people that count, i.e. those whom you respect and admire, know it all. Most of the rest is vanity.[79]

Segrè received the Nobel Prize in Physics in 1959, and by the time he wrote this letter, he had already nominated Wu for the Prize twice.

Not everyone, however, accepted the narrative of the "Wu experiment." Low-temperature physicist Nicholas Kurti—who had trained Ambler and Hudson at Oxford—and particle physicist Christine Sutton argued that the experiment should have been called the "NBS experiment" instead of the "Wu experiment". Regarding Wu being the first author of the paper, they said, "the chivalrous suggestion was made that as Wu was the only woman she might sign first. (One wonders whether 40 years on such a suggestion would be regarded as an early example of affirmative action or a sexist remark!)".[80] Telegdi appears to have shared this view, later referring to the result as the "NBS–Columbia" finding.[81]

From the NBS perspective, their scientists worked full-time on the experiment, bringing essential expertise in low-temperature physics and the specialized apparatus without which the project could not have succeeded. Wu, in contrast, split her time between Columbia and NBS and was absent for the first consistent detection. From Wu's perspective, she had initiated the collaboration only because Columbia lacked the necessary facilities—a notable departure for someone who preferred to work independently with her own graduate students. The collaboration also appears to have been marked by differences in work style. Wu, treating the experiment as urgent and time-sensitive, cancelled travel and skipped breaks. The NBS scientists, in her view, proceeded more leisurely, taking lunch breaks and playing bridge—Ambler took a two-week

[79]Letter to Wu, December 25, 1988, Carton 15, folder 17, Emilio Segrè papers, Bancroft, Berkeley
[80]Kurti and Sutton (1997, p. 575)
[81]Telegdi (1987, p. 438)



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Votre référence / Your reference
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Dear Chien Shung: (how do you spell it?)
I am off for Stockholm, and I am thinking of you. You should be going there, not me. Nobody has deserved that prize more than you and nobody has done more beautiful experiments.

Your admirer
Jack

Jack is a well respected experimental critic. I am elated by such high praise. Please also See Sept issue of Science.
C.S.

Figure 4.5. Letter from Jack Steinberger to Chien Shiung Wu, 1988, Carton 15, folder 17, Emilio Segrè papers, Bancroft, Berkeley

holiday in August 1956, in the middle of preparations.[82]

Wu's competitiveness reflects the structural obstacles she faced: as a woman in a male-dominated discipline, she had to work harder to receive equivalent credit and visibility. Yet the Wu experiment also shows how her work was shaped by processes of institutional inscription and the micropolitics of prestige. Rabi, despite his own prejudices about women in science, acted to anchor the discovery at Columbia rather than NBS. Compared to Lee and Yang, Wu did not receive the Nobel Prize; compared to other experimentalists, she emerged from 1957 as a recognized physicist.

4.4 Chien-Shiung Wu, the Activist

Following her Nobel-worthy experiment, Wu was finally promoted to full professor in 1958, a significant milestone in her career. Yet it was also a year of deep personal loss. Early that year, she received the devastating news that her older brother, Jian-Ying, had died in China from poor health during the Great Leap Forward.[83] In December, she faced another profound loss with the death of Wolfgang Pauli. At the time, Wu was preparing an article about neutrinos for a collection ("Festschrift") in honor of Pauli's sixtieth birthday; it would instead appear in the memorial volume published after his death. Pauli's sister, Hertha Pauli, wrote to Wu, thanking her for the intellectual comfort she had brought to him in his final days when he "needed two morphine injections to subdue the pain":[84]

> To think, that on this unforgettably horrible day he had at least one good hour while writing to you, to be at least in thought close to a kind person whom he venerated, this is everything to me.[85]

Just two months later, during the third day of Chinese New Year in 1959, Wu's father also passed away. Hertha Pauli conveyed her heartfelt condolences, noting how "unbearable" it must have

[82] McGrayne (1998, p. 274).
[83] The Great Leap Forward (1958–1962) was a campaign led by Mao Zedong aimed at rapidly industrializing China and collectivizing agriculture, which resulted in widespread famine.
[84] Letter from Hertha Pauli, May 24, 1959. C.S. Wu Papers, Box 3, CUA.
[85] Ibid.

been for Wu to be far away when her father died.[86] McCarthy-era restrictions had barred her from visiting her family in China in the 1950s. In Chinese tradition, to see one's parents on their deathbed carries significant cultural meaning. That she failed to do this likely weighed on her in later years—a sorrow she carried quietly alongside her professional triumphs.

Wu's relationship with her father was central to her life and work. She often spoke of his influence on her worldview, mentioning his teachings in letters and conversations. Their bond remained a source of strength throughout her career. She kept a photograph of them together in her office—a photograph that still hangs on the 13th floor of Pupin Hall at Columbia today, a quiet testament to her admiration for him.

Wu's father was a feminist and an activist. In what follows, I turn to Wu's own feminism and activism in her later years, with particular attention to her efforts to expand opportunities for women and minorities in science. Feminist scholarship on emotional labor and hermeneutic labor has highlighted how care labor often remains invisible and unacknowledged by those who benefit from them.[87] A similar phenomenon exists in academia, where more visible contributions, such as publications and awards, overshadow less visible yet vital contributions, such as administrative work and activist endeavors that transform the structural and cultural norms of academia. This section aims to shed light on these overlooked aspects of Wu's legacy, showing her lesser known yet equally significant contributions to the scientific community.

4.4.1 Feminist Activism

In the 1960s, Wu led several different experimental groups. One worked on double beta decay in a salt mine 2,000 feet underground near Cleveland, Ohio; one used Mössbauer spectroscopy to study sickle-cell disease; one investigated time-reversal invariance; another probed exotic atoms at Brookhaven's Alternating Gradient Synchrotron.[88] In 1963, Wu and

[86] Ibid.
[87] See, for instance, Lazzarato (1996) and Lanoix (2013).
[88] Georgia Papaefthymiou-Davis, interview with the author, February 25, 2025; Evelyn Hu, interview with the author March 18, 2025.

her students also confirmed the conserved vector current hypothesis in beta decay, originally proposed by Richard Feynman and Murray Gell-Mann in 1958.

The 1960s were also a transformative period for American universities, shaped by the civil rights and feminist movements that demanded structural and administrative reforms. Among these institutions, Columbia University stood out as a site of significant upheaval and change. In April 1968, only weeks after the assassination of Martin Luther King Jr., Columbia became the focal point of student activism with campus-wide protests. The demonstrations were driven by two issues: the university's involvement in the Vietnam War, and its plan to build a gymnasium in Morningside Park—a project criticized for exacerbating racial divides in the Harlem community. The resulting chaos forced Columbia to overhaul its administration: in 1969, the University Council was quietly dissolved and replaced by a newly minted "University Senate", designed to include broader representation with shared governance. The following year, William McGill—formerly Chancellor at the University of California, San Diego, and noted for his ability to manage campus unrest—was appointed president of Columbia.

McGill took the helm of Morningside Heights around the same time Richard Nixon settled into 1600 Pennsylvania Avenue. Nixon's administration, in an uncharacteristic nod to feminist demands, established the Task Force on Women's Rights and Responsibilities in 1969 and appointed Anne Armstrong as Counselor to the President in 1972, the first woman to hold the post. This marked a shift from the quieter and often ambivalent attitudes toward women's rights during the Kennedy and Johnson years.[89]

Sex discrimination had been only belatedly and somewhat haphazardly addressed by the White House in the 1960s. Title VII of the Civil Rights Act of 1964—which prohibits employment discrimination based on race, color, religion, sex, and national origin—only gained its "sex" clause through an amendment by Representative Howard Smith, reportedly added to sabotage the bill's passage. Meanwhile, President Johnson's famous Executive Order 11246 in 1965 mandated affirmative action for race but made no mention of sex.

[89] See Graham (1990).

In what might be described as an academic echo of Nixonian politics, McGill established a President's Advisory Committee to help steer Columbia through its post-1968 turbulence, though with considerably less fanfare and arguably more substance. Wu was one of 13 members appointed to the committee, which also included one Black member, another Asian member, and four other women.

Though she didn't initiate it, Wu soon took the lead in an informal subcommittee of the women members of the President's Advisory Committee, focused on gender equity at the university.[90] She even kept a custom stamp in her office labeled "Women Members of President's Advisory Committee on Affirmative Action". She brought to the advisory committee the same intellectual rigor she was known for in the laboratory. She oversaw the collection of demographic data from every department and rank, compiling comparative tables that positioned Columbia alongside peer institutions such as Harvard, Berkeley, and the University of Chicago.[91] The work culminated in Columbia's 1972 affirmative action program.

A major focus of Wu's subcommittee was exposing salary disparities between male and female faculty of the same rank. Among the many tables Wu meticulously compiled, one shows that in the physics department, 16 male professors earned an average salary of $\$25,344$, while the sole female professor earned $\$23,000$.[92] Adjusted for inflation, this $\$2,344$ gap in 1972 translates to approximately $\$17,500$ today. That female professor would have been Wu herself. At the time, the retirement age for both men and women was 65, a standard that remained in place until federal changes in 1983. Wu was 60 years old and had been a faculty member at Columbia since 1944, making her one of the department's most senior, yet worst-paid, professors.

This disparity was not surprising. In 1953, after she was promoted to associate professor with tenure, her salary was described as "below the minimum set for her grade" in a letter

[90] The group included Nina Garsoïan, Patricia Graham, Ann Hirsch, Frances Hoffman, and Wu herself. Garsoïan, Graham, and Hoffman initially began meeting informally to discuss concerns about the status of women at Columbia. Upon learning of these gatherings, Wu asked to join. (Patricia Graham, interview with the author, March 25, 2025)

[91] C.S. Wu Papers, Box 6, CUA.

[92] Ibid.

from William Havens who advocated for a raise.[93] Decades later, when advocating for equal pay in the 1970s, Wu and her colleagues still encountered much resistance—not only from administrators but also from male colleagues who argued that women's salaries were merely "secondary incomes" to the household, implying that men, as primary providers, deserved higher wages.[94]

She also campaigned for improved childcare, calling on universities to provide professional day care centers so that mothers could "get away from the monotonous household chores and work in their chosen field."[95] At an APS panel in 1971, she asked pointedly: "Why shouldn't educational institutions lead the nation in such bold social improvements? Why shouldn't husbands and wives share the work as well as the parental joy?"[96] Wu had relied on her husband and a "lovely lady" who looked after her son as an infant—her work on the parity experiment would not have been possible without her husband taking on primary childcare responsibilities. Aware that many women had no such support, she emphasized the need for institutional reform.

In March 1972, her recommendations appeared in Columbia University's Affirmative Action Plan to End Sex Discrimination at Columbia, which committed the university to creating childcare facilities accessible to staff, faculty, and the broader Columbia community.[97] The plan outlined a commitment to providing childcare services for children from infancy to age 12, structured around flexible schedules to meet the needs of working parents. It also specified that the facility should remain operational whenever the university was open.

Between 1971 and 1972, the women members of the President's Advisory Committee put forward numerous policy recommendations at Columbia. These included calls for equal pay for women and minority faculty members, job-protected maternity leave for both tenured and non-tenured women, the establishment of childcare facilities, and an increase of the representation of

[93]William W. Havens Jr., letter to an unidentified recipient, April 23, 1953. C.S. Wu Papers, Box 7, Folder 6, CUA.

[94]Jonathan R. Cole, interview with the author, January 30, 2025.

[95]C.S. Wu Papers, Box 9, Folder 26, CUA.

[96]Ibid.

[97]C.S. Wu Papers, Box 6, Folder 31, CUA.

women and minorities in both tenured and non-tenured faculty positions.[98] Their proposals were backed by demographic surveys and detailed accounts of individual cases of discrimination.[99]

The work did not go unnoticed. Sinologist William Theodore de Bary, the Executive Vice President for Academic Affairs and Provost, wrote to Wu:

> I cannot tell you how much I appreciate your help, even though it may seem to you that I am resisting it much of the time. You are very good to give so much to this task and to be so conscientious in fulfilling it. When I recommended your names to the President, I knew you would not be 'patsies' for the administration, and although I am sorry to have put you in a difficult and delicate position (to say nothing of myself), I don't regret the recommendation at all.[100]

Indeed, Wu was far from being a "patsy." Jonathan Cole, a Columbia sociologist who served with her on the committee, described her as "calm" and "gentle." Though she was not the most vocal member of the group, her quiet persistence and refusal to settle for token gestures made her a formidable presence.[101] She would not accept superficial reforms—she insisted on real, structural changes.

She also later recommended Ruth Bader Ginsburg—then a rising legal scholar, later a Supreme Court Justice—for a position on the advisory committee.[102] It is unclear whether President McGill acted on the recommendation; Ginsburg did not ultimately join the committee.[103] Even so, the two likely encountered each other in their overlapping advocacy for equal pay and equal retirement benefits.

4.4.2 The First Woman President of the American Physical Society

Wu's commitment to equity extended beyond gender. In the 1970s, she turned her attention to sickle-cell anemia, a chronic blood disorder disproportionately affecting Black communities. "Living and working near the edge of one of America's largest black communities

[98]Ibid.

[99]The individual cases include Carolyn R. Denning, Ruth C. Harris, and Anneliese Sitarz, all of whom raised concerns about stalled academic advancement and discriminatory salary practices.

[100]Letter from William Theodore de Bary, April 10, 1972. C.S. Wu Papers, Box 6, Folder 31, CUA.

[101]Jonathan R. Cole, interview with the author, January 30, 2025.

[102]Letter to William McGill, May 30, 1972. C.S. Wu Papers, Box 6, Folder 31, CUA.

[103]Patricia Graham, interview with the author, March 25, 2025.

[Harlem], we were gravely concerned when we learned of the plight of those who suffer from sickle-cell anemia," Wu explained.[104] Collaborating with John Bertles from St. Luke's Hospital Center, Wu led a team of graduate students and postdoctoral associates from the physics department to investigate the transformation of normal doughnut-shaped blood cells into sickle-shaped blood cells.[105] They examined the electronic structure of Fe^{2+} in normal human hemoglobin and the Mössbauer spectra of Fe^{2+} in aggregated and anhydrated sickle-cell hemoglobin.

Wu also preserved extensive materials related to the status of underrepresented communities in physics. Among them were "Physics in the Black Colleges", the proceedings of a 1970 conference in Atlanta, and Fermi Lab's "Minority Physics Conference," which she helped plan.[106] Her handwritten corrections, suggestions, and edits on these documents, often made with pens of different colors, demonstrate her quiet but deliberate dedication to these efforts.[107] When Wu became the first woman to serve as president of the APS in 1975, she used her position to advance this work. One of her first acts was to nominate Carl Spight, a Black physicist and civic activist, as chair of the APS Committee on Minorities.[108] Under her leadership, the APS Committee on Minorities organized a "Workshop on the Pre-Graduate Preparation of Black Students in Physics" in Atlanta, Georgia. The Committee on Women in Physics and the Committee on Professional Concerns expanded their initiatives, fostering greater diversity within the field.[109]

Being APS president was no small task. Physicists are rarely shy about voicing their opinions, particularly on politics and social issues. Wu had to navigate several thorny debates. She was called upon to help Soviet Jewish scientists facing repression, including those who had resorted to hunger strikes. Wu advocated for their right to leave the U.S.S.R. and condemned the

[104] Lubkin, "Chien-Shiung Wu, the First Lady of Physics Research," p. 56.
[105] The group includes C.S. Yen, B.H. Huynh, G.C. Papaefthymiou, P.H. Swerdlow, and J.L. Groves. (Georgia Papaefthymiou-Davis, interview with the author, February 25, 2025)
[106] C.S. Wu Papers, Box 4, CUA.
[107] Ibid.
[108] Ibid.
[109] Wu, Presidential Address, February 3, 1976. C.S. Wu Papers, Box 5, Folder 35, CUA.

Soviet government for withdrawing research support from these scientists. This, however, drew criticism from some who accused the APS of overlooking “the domestic front” while criticizing others abroad.[110]

Wu did, in fact, address domestic concerns. As APS president, she wrote to U.S. President Gerald Ford, addressing concerns about federal support for basic research.[111] The Mansfield Amendment of 1969 had prohibited the Department of Defense from funding basic scientific work, cutting off a major source of support for theoretical physics. Recognizing how vital federal funding was to the scientific community, she labored over her message, producing multiple drafts filled with careful edits and rephrasing—a humbling reminder that English was not her first language.

Wu never forgot her own journey from China, nor the barriers she had faced. She regularly mailed Columbia’s application materials to prospective Chinese students, many of whom later sent heartfelt letters of thanks after being admitted. For those whose visa applications were denied, she often became their first, and sometimes only, point of contact for assistance. With her own graduate students, she held her Chinese mentees to particularly high standards, believing that anything less than outstanding performance would be insufficient to overcome the prejudice against them.[112]

Her ties to the Chinese academic community endured even through moments of deep personal sorrow. During the Cultural Revolution, her younger brother, Wu Jian-Hao, and her uncle Wu Zhuo-Zhi died after being persecuted. When Wu left China in 1936, Jian-Hao was still only a middle-school student. Nearly three decades later, she learned from newspaper reports that C. N. Yang had managed to reunite with his family in Hong Kong in 1964, at a time when U.S.–China relations remained deeply strained. Encouraged by this, Wu arranged a secret meeting with her brother and her uncle in Hong Kong in 1965. By then, Jian-Hao was Chief Engineer at the Zhengzhou Waterworks in Henan Province, and he and his wife, Jiang Han-zhen,

[110]Open letter from Vladislav Bavc, Janurary 27, 1976. C.S. Wu Papers, Box 4, CUA.
[111]C.S. Wu Papers, Box 4, Folder 23, CUA.
[112]McGrayne (1998, p. 270).

flew to Hong Kong to see her. It is difficult to imagine what that reunion must have felt like: when Wu had last seen her brother, he was still a schoolboy and both of their parents were alive; now he was a husband and father of two, and their parents were gone.[113] At the time, there was still hope that Wu and her husband might soon be able to return to China. But the following year the Cultural Revolution began. Unbeknownst to Wu, the meeting in Hong Kong would be the last time she saw her brother. He died in 1968.

Officially, his death was described as a suicide, but some accounts suggest that he was tortured to death.[114] When his letters suddenly stopped, Wu could not understand why; only in 1970 did she learn that he had died under horrific circumstances. In a letter concerning Jian-Hao's rehabilitation memorial after the end of the Cultural Revolution, she recalled through tears the family members they had lost.[115] Towards the end of the message, Wu wrote, "I remember when my father was still alive, he taught us to think of the 'big self' and not to dwell on the 'small self.' Being able to live this way is something to be truly proud of."[116] In Chinese culture, the "big self" refers to society or the nation; the "small self" refers to oneself.

Understandably, Wu's stance toward the communist government was, to say the least, critical. But even in grief, she chose to focus on the "big self". Following the thaw in U.S.–China relations after President Nixon's 1972 visit, Wu returned to Mainland China in 1973 for the first time since leaving at the age of 24. She visited Ming-De School, the institution her father had founded, and left a note in his honor: "My father was an educator who transcended his time."

[113]Details of this meeting were recounted by Jiang Han-zhen, Jian-Hao's wife, in an article published in "上海灘" (*Shanghai Tan*), no. 4 (2012).

[114]Jiang Han-zhen, Jian-Hao's wife, did not describe his death as a suicide in *Shanghai Tan*, no. 4 (2012); Yuan (2021), Wu's grand-daughter mentions the deaths of her uncle and brother by torture. According to an unverified account by Li Xueshu, Wu Jian-Hao was targeted because his sister had helped "American imperialism" build the atomic bomb. Although the official line was that he had taken his own life, Li reports that his family rejected this account after seeing signs of torture on his body. If Li's account is true, Jiang may have chosen not to tell Wu the full circumstances of his death.

[115]"Rehabilitation memorial" is my translation of "平反昭雪追悼會," a term referring to posthumous political rehabilitation ceremonies that became common after Mao Zedong's death and the conclusion of the Cultural Revolution. The referenced letter, written in Chinese is undated but likely from 1979, as Wu mentioned her return from Washington D.C. after Deng Xiaoping's U.S. visit. C.S. Wu Papers, Box 4, Folder 3, CUA.

[116]"記得父親在世時教導我們的為大我著想，不要把小我放在心上，能做到這樣是很可驕傲的。" Translation mine. C.S. Wu Papers, Box 4, Folder 3, CUA.

Shortly after, she began organizing scholarly exchange programs to strengthen academic ties between the U.S. and China.[117] In June 1978, she welcomed a group of 12 Chinese plasma and nuclear physicists upon their arrival in Washington, D.C.[118] Six months later, during Deng Xiaoping's historic visit to the U.S., she witnessed the formal establishment of U.S.–China diplomatic relations and collaborations in science and technology.[119]

Her experiences of personal loss, above all the deaths of her parents and brothers, may have deepened the humanitarian commitments that would later find public expression in her activism. On July 15, 1979, Wu took part in a rally at the United Nations' Dag Hammarskjöld Plaza, advocating on behalf of the Vietnamese "boat people" fleeing persecution. She was one of four representatives of the Chinese community at the event, joining a diverse group of speakers, including Bayard Rustin, a prominent Black civil rights and gay rights activist and close advisor to Martin Luther King Jr., as well as Martin Begun and Rabbi James Rudin, representatives of the Jewish community.[120] Together, they urged the U.N. to "rescue the boat people" and called on the U.S. government to "open its borders" to those escaping what they described as genocide by the Vietnamese government.

The plight of the boat people stemmed from the aftermath of the Vietnam War. Following the fall of Saigon in 1975 and the consolidation of communist rule under North Vietnamese leadership, political repression was widespread, particularly against those in the South who had been aligned with the U.S.-backed Republic of Vietnam. American troops had first arrived in Vietnam in 1965 and withdrew in 1973. By 1978, the Vietnamese government also intensified persecution of its ethnic Chinese population, who made up approximately three-quarters of the boat people.[121] Wu kept a handwritten letter in traditional Chinese, which reads: "Tens of thousands of Vietnamese Chinese are drifting at sea, many already dead, their bodies swallowed

[117] Letter from Pierre M. Perrolle, May 24, 1978. C.S. Wu Papers, Box 3, Folder 28, CUA.

[118] Letter in Chinese to "Ambassador Chen," possibly Chen Chu, and "Madame Chen", June 20, 1978. C.S. Wu Papers, Box 3, Folder 26, CUA.

[119] Undated letter in Chinese. C.S. Wu Papers, Box 4, Folder 3, CUA.

[120] Committee Against Genocide by Vietnam (制止越南迫害華人行動委員會), Press Release, July 15, 1979. C.S. Wu Papers, Box 9, CUA.

[121] See the *New York Times* article, "4,000 March for Rescue of Refugees," July 16, 1979.

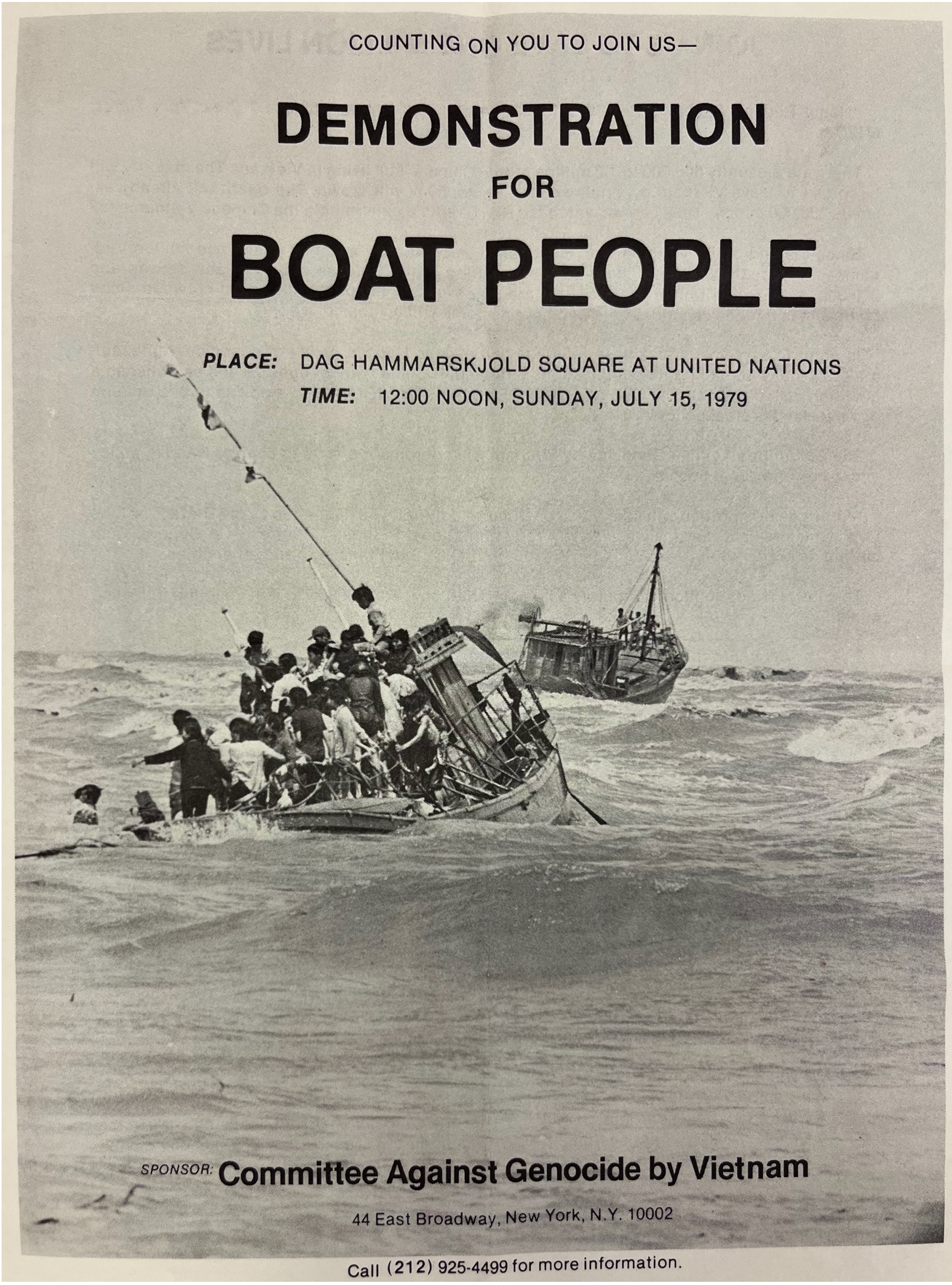

Figure 4.6. Poster for the 1979 demonstration on behalf of the boat people, preserved by Wu

by the fish. The rest are helpless, desperately fighting against the ocean and death...all a result of the Vietnamese government's racial discrimination. How can we sit still and watch?"[122] Facing political and economic marginalization, many Vietnamese Chinese saw escape as their only option. Yet escape itself was often deadly: they set out in overcrowded and unseaworthy boats, exposed to storms, pirate attacks, and starvation.

Many countries were reluctant to accept large numbers of refugees at the time. The rally at the U.N., attended by an estimated 4,000 people, helped bring global attention to the crisis. The U.N. then successfully pressured the United States, Canada, and other nations to increase refugee admissions. After the passage of the U.S. Refugee Act of 1980, many Vietnamese refugees were allowed to settle in the U.S.

Clare Boothe Luce, former U.S. Ambassador, once remarked that when Wu "knocked off the principle of parity, she established the principle of parity between men and women."[123] In fact, Wu's impact went far beyond scientific achievement. She actively worked to improve the status of women in academia, always stood in solidarity with marginalized communities, advocated for equal pay and pensions, increased the employment of women and minorities at Columbia University, and pushed for broader institutional reforms to promote equity at a time when there was much resistance.

4.5 On the Possibility of Standpoint Epistemology in Physics

Can Wu's remarkable accomplishments be understood as a case study in feminist science? If so, what interpretation of that concept might fit her example? There is no doubt that her career was shaped, both constrained and enabled, by her identity as a woman. Her story motivated many young girls to pursue physics; her survival strategies, forged in a male-dominated discipline,

[122] "成千上萬的越南華僑正在海上漂流，其中很多已經葬身魚腹，其餘的正孤立無援地，絕望地和大海，和死神作殊死鬥...我們能坐視嗎?" Translation mine. The letter was collectively signed by the Columbia University Chinese Human Rights Association, the Columbia University Taiwanese Students Association, Columbia University Song-She, and the Columbia University Hong Kong Students Association. C.S. Wu Papers, Box 9, CUA.

[123] Hartford Courant, October 5, 1958.

remain instructive for other women scientists. Perhaps the more challenging question lies in feminist epistemology: can Wu's life and work illuminate the claims of feminist standpoint theory?

The heart of feminist standpoint theory is what Emily Tilton (2024) refers to as *the Situated Knowledge Thesis*: epistemic agents are socially situated, so their knowledge is also socially situated, shaped by their histories, resources, and values, etc. Standpoint theory as a whole is then perhaps best understood as a program that critically explores the implications of the Situated Knowledge Thesis. Dominant groups often fail to critically examine how their privileged social positions shape their beliefs, leaving them epistemically disadvantaged to certain facts. If you want to know whether a particular physics department is sexist, you will likely get a more accurate assessment from a woman in that department because she might have better epistemic access to the relevant evidence. In the sciences, women primatologists and biologists have brought forward interpretations informed by their situated knowledge, challenging entrenched assumptions.

Some aspects of standpoint epistemology fit comfortably within the practice of physics. Women may, in virtue of their social position, have access to different, and in some contexts better, evidence about their professional environments, and have stronger incentives to gather and act on such evidence. Wu's career illustrates this kind of epistemic advantage. Through her service on Columbia's advisory committee and the APS, she drew on her situated knowledge to advocate reforms such as institutional childcare. Wu herself endorsed the view that greater inclusion of women in science would benefit both the discipline and society because "women's vision and humane concern" would contribute to addressing science-relevant societal issues such as environmental degradation, chemical pollutions, and drug safety:

> Men have always dominated the fields of science and technology. Look what an environmental mess we are in. Women's vision and human concern may be exactly what is needed in our society. If there were more women like Rachel Carson to warn us of chemical pollution, Dr. Alice Hamilton to call attention to occupational health hazards, Dr. [Frances Kelsey] in the Food and Drug Adminis-

> tration who stubbornly worked to safeguard us from drugs like Thalidomide, the world would be a more pleasant and safe place in which to live.[124]

A stronger and more controversial thesis would hold that women possess an epistemically privileged standpoint on physical reality itself. If physical reality refers to atoms and nuclei, the thesis that women are better informants seems implausible. Wu herself was skeptical about this stronger thesis:

> Bringing a womanly point of view may be advantageous in some areas of education and social sciences, but not in physical and mathematical sciences where we strive always for objectivity. I wonder whether the tiny atoms and nuclei or the mathematical symbols or the DNA molecules have any preference for either masculine or feminine treatment.[125]

Wu's assertion of gender-blindness in the physical sciences, of course, served a strategic function: it implied that she was just as able as any man. Still, her remark points to a difficulty in extending standpoint epistemology, which developed largely in relation to socially embedded subject matters, to the physical sciences. In fields such as primatology or psychology, the social positioning of the knower has an evident role in shaping research. By contrast, physics appears, at least superficially, more resistant to such influence. Sandra Harding (1992) has challenged this resistance by arguing that the social and natural science divide is a false dichotomy: atoms, trees, and rocks are "social objects" insofar as all knowledge about them is socially situated. Even if Harding is right, however, the fact that all scientific knowledge is socially situated would not by itself show that women, *qua women*, have an epistemic advantage in investigating atoms or rocks.

Wu's intellectual biography nonetheless supports a more modest and defensible application of standpoint epistemology to physics. Feminist values—understood broadly as values arising from the experience of marginalized positions—need not manifest as stereotypically "feminine treatment". Rather, they can shape the organization of work, the choice of research

[124] Written material and cassette based on Dinah L. Moche's interview. C.S. Wu Papers, Box 9, Folder 20, CUA. The original document reads "Dr. Kelly," but this is likely to refer to Dr. Frances Kelsey. I thank Cathy Gere for pointing this out.

[125] Mattfeld and Van Aken (1965, p. 45)

questions, and the willingness to explore alternative or neglected hypotheses (Wylie and Nelson (2007) and Kourany (2010)). On this interpretation, Wu's career offers multiple instances of such influence.

She often gravitated toward low-budget, independent projects, joining or leading small teams where she could shape the work directly, rather than participating in the large-scale projects. At Berkeley, she spent "a fair amount of time" at the cyclotron but was not part of Lawrence's "cyclotron crew."[126] August Carl Helmholz, a fellow graduate student, recalled that she was "very well acquainted with the theory" compared to other students of Lawrence, and when asked whether Wu had wanted to be on the cyclotron crew or had been excluded, Helmholz replied, "I hadn't really thought about it until you brought it up."[127]

If this absence was voluntary, it was a notable decision. Lawrence won the Nobel Prize in 1939 for inventing the cyclotron, which, in the words of the Rockefeller Foundation, became "a mighty symbol, a token of man's hunger for knowledge." (Galison, 1992, p. 3) He was the chief architect of "big science," a model for scientific inquiry soon validated by its wartime achievement. Nuclear physicist Maurice Goldhaber recalled: "The first to disintegrate a nucleus was Ernest Rutherford, and there is a picture of him holding the apparatus in his lap. I then always remember the later picture when one of the famous cyclotrons was built at Berkeley, and all of the people were sitting in the lap of the cyclotron. Roughly speaking, that gives you an idea of the change."[128] The photograph Goldhaber described—twenty-two people perched atop the cyclotron's massive horseshoe magnet and another twenty-one gathered below—became an emblem of a new scientific era (see Figure 4.7). To join such a project was to claim both prestige and proximity to the very center of nuclear physics.

If Wu's distance from the cyclotron crew was in part deliberate, it may have been an early recognition of big science's bureaucratic style: securing resources, managing large teams, and courting public attention. If the traditional image of the scientist was of a solitary

[126]Helmholz (1993, p. 58).
[127]Ibid.
[128]Hiltzik (2015, p. 5).

Figure 4.7. 1939 Photograph of Lawrence Berkeley Laboratory's scientific and technical staff

genius, "big science" inaugurated an image of the scientist as manager, boss or CEO. In this environment, women not only faced stereotypes about their scientific ability but also about their managerial competence. Wu may have seen limited room for herself in such a structure, and she may have preferred the intellectual autonomy of small-scale work. This choice, or constraint, proved advantageous. Instead of spending nights repairing the cyclotron, she developed as an experimentalist with a strong theoretical foundation, becoming an "asset to the United States" who could not easily be replaced.

Her choice of research questions and her willingness to consider hypotheses others dismissed also set her apart. Wu's doctoral research on beta decay and uranium fission products established her reputation in the physics community in the 1940s and 1950s. She often carved out small, precise questions within larger, fashionable fields, becoming the leading authority in those niches. When most physicists considered the parity experiment a waste of time, she grasped both its significance and its urgency. Even later, as physics shifted toward high-energy accelerators, large rockets, expansive budgets, and vast collaborations, Wu remained committed to modest apparatuses and carefully framed experiments.

Her preference for small projects may also have been shaped by the insecurities of her peripheral position. She was famously cautious and protective of her data, warning students never to show results to visitors. When guests pressed for information, she slipped into a tangled and convoluted Chinese-English, speaking softly, endlessly, and leaving the question unanswered.[129] As one former student put it, "Maybe she felt that she was in enemy territory."[130] In a country where she had to prove herself irreplaceable to remain, she may have concluded that she could not succeed as a minor contributor to a massive project. Small science offered a trustier path to securing credit for her achievements.

Taken together, these patterns illustrate a modest standpoint-epistemological point: diversity in social positions can foster diversity in scientific styles, which in turn can enrich the

[129] McGrayne (1998, p. 269).
[130] Ibid.

discipline. In Wu's case, the interplay of personal inclination and the constraints of marginalization produced a distinctive research style that thrived outside the growing big science model. In an era when big science now claims the lion's share of funding and attention, Wu's career shows that small projects, too, can lead to transformative discoveries. This is one of the central benefits of standpoint epistemology as applied to the sciences: the epistemic value of sustaining a plurality of research styles. We need, as much as ever, both the flamboyant Richard Feynman and the solitary Barbara McClintock.

4.6 Conclusion

It is impossible to distill Wu's life into a single narrative. Her journey from China to America marked the beginning of an extraordinary career as a physicist. At the same time, she was disheartened by the discriminations she experienced, which seeded her activism. Wu had not intended to remain in America, but successive wars shaped her decision to stay. She became a "perfect immigrant," though she never fully reconciled with her American identity. When she passed away in 1997, her ashes, as per her wishes, were returned to her hometown Taicang, Jiangsu, China. She was buried beside the "Zi-Wei" (crepe myrtle) tree her father had planted for her at Ming-De School, the institution he founded to advance girls' education after the fall of the Qing dynasty (Figure 4.8). Wu had grown up with that tree, and beneath it was inscribed: "It takes ten years to grow a tree, but a hundred years to educate a generation."[131]

She never received a Nobel Prize, but she lived among those who did. She maintained close friendships with Lawrence, Lamb, Lee, Oppenheimer, Rabi, Segrè, Yang, and many others; Einstein visited her in the hospital after the birth of her son; Pauli wrote to her in the final days of his life as he suffered from pancreatic cancer. Yet unlike Lee and Yang, whose success as Nobel laureates was celebrated in China and taken to disprove assumptions of Chinese intellectual inferiority,[132] and unlike Qian, whose forced return to China was appropriated and

[131] 十年樹木，百年樹人。

[132] Yang repeatedly said in interviews and public speeches that his greatest accomplishment was helping to dispel

Figure 4.8. The Zi-Wei tree at Ming-De School, Taicang, photograph by the author

instrumentalized as a symbol of national achievement, Wu remained less well known and less easily folded into patriotic mythmaking. There was something more elusive, and in some ways more human, about her place in history. Her life reveals not only scientific brilliance, but also the fractured experience of the Chinese scientific diaspora in the twentieth century.

There is no shortage of remarkable stories from Wu's life. But this paper closes with one of the least known and perhaps most telling episodes. After Pearl Harbor, anti-Japanese hysteria swept across the U.S. It was fueled by the kind of wartime imaginary analyzed by (Galison, 1994, p. 230), in which the "Enemy Other" was both racialized and dehumanized—"to the Americans, British, and Australians, the Japanese soldiers were often thought of as lice, ants, or vermin to be eradicated." This rhetoric was not confined to the war front; it rationalized the incarceration of Japanese Americans and echoed in newspapers, radio broadcasts, and everyday conversations.

At the time, Wu had no way of knowing whether her family were among the millions killed in China during the Japanese invasion. And yet, when she left her job at Smith College, she recommended Dr. Shuichi Kusaka, a Japanese physicist from Berkeley, as her replacement. The appointment sparked outrage, first on campus, then in the town of Northampton, and eventually in national headlines.[133] But Wu stood by her endorsement, and so did Smith.[134] The college proceeded with the appointment.

In a moment shaped by fear and hatred, Wu acted with clarity and courage. She saw not an enemy, but a scientist, a colleague, a human being. At a time when science again faces political hostility, her story reminds us that scientists still have the power, and perhaps the duty, to stand by one another. Scientists protect scientists. Sometimes, that is the work.

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the Chinese people's sense of inferiority.

[133]See, for instance, *Pacific Citizen* article "Smith College Backs Japanese Professor Despite Threats", September 18, 1943, Vol.17; No. 11.

[134]The story was told by Imra Buwalda to Kay Haugaard in 1974. John P. Buwalda and Imra Wann Buwalda Papers Collections, Box 15, Folder 5. Caltech Archives and Special Collections.

Acknowledgments

Chapter 4, in part, is currently being prepared for submission for publication. The dissertation author was the sole author of this paper.

In addition to those already mentioned in the dissertation acknowledgment, I would like to thank Cathy Gere and Yao Lin for their helpful comments on an earlier draft of this chapter. I am grateful to Jonathan Cole, Gerry Dugan, Patricia Graham, Evelyn Hu, Georgia Papaefthymiou-Davis, and Shu Chien for agreeing to be interviewed, and to Julia Steinberger and Harriet Zuckerman for their email correspondence. I thank the archivists at Berkeley, Caltech, CERN, Columbia, and Smith College for their assistance, as well as a student at Southeast University in China who went out of his way to help me navigate bureaucratic obstacles. Finally, I thank the UC San Diego International Institute for its travel award, and the organizers and audiences of the First Feminist Philosophy of Physics Workshop at the University of Michigan, Ann Arbor, the NYU Shanghai Philosophy Speaker Series, the Institute for Science in Society at Radboud University, and the Science Studies Colloquium Series at UC San Diego for their thoughtful engagement with this project.

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