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Cultural Studies for the End of the Century

Zeroing In on the Year 2000

THE FINAL EDITION

George E. Marcus, EDITOR

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Erratum to *Para-Sites: A Casebook against Cynical Reason* (Late Editions 7).—On page 11, Colin Richards was identified as a doctor working as a coroner at the time of Steven Biko's death in South Africa. In fact, Richards was at that time working as a medical illustrator at the medical school of Witswatersrand University.

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♾ The paper used in this publication meets the minimum requirements of the American National Standard for Information Sciences—Permanence of Paper for Printed Library Materials, ANSI Z39.48-1992.

CONTENTS

11. There are a diverse set of concepts and practices associated with uploading, but all entail the replication of human identity and/or "consciousness" in a computer environment. This concept was popularly articulated by computer scientist Hans Moravec in his 1988 book, *Mind Children* (Cambridge: Harvard Univ. Press), but has many literary, scientific and philosophical antecedents. For analysis on the rhetorical and affective practices of uploading, see my "Uploading Anticipation, or Becoming Silicon" at <http://www.personal.psu.edu/faculty/r/m/rmd12/uploading.html>. For speculative articulation of uploading's effects, see Robin Hanson, "If Uploads Come First: The Crack of a Future Dawn," <http://hanson.gmu.edu/uploads.html>.

12. Burroughs, William S. *The Adding Machine* (New York: Seaver Books), 135.

13. Spookier still, these entanglements have been played in by historian of science Michael Fortun and physicist Herb Bernstein, for whom they present a case study in the contingency and power of ongoing research. Ironically, Fortun and Bernestein's analysis highlights the role that such entanglements might play in cryptography, where they offer the promise of breaking all "public key" encryption technologies, perhaps aiding Merkle's plans for future decoding. At the same time, this "quantum computing" also promises to produce the holy grail of cryptography and a fearful event of cryonics—an unbreakable code.

14. Ettinger, R. C. W. *Man into Superman.* <http://www.cryonics.org/chapter5_1.html>.

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Mike A. Fortun

10

Fluctuating about Zero, Taking Nothing's Measure

I. Fluctuations about Fame: Lamoreaux, l'amour aux (0)

> For a half century, physicists have known that there is no such thing as absolute nothingness, and that the vacuum of empty space, devoid of even a single atom of matter, seethes with subtle activity. Now, with the help of a pair of metal plates and a fine wire, a scientist has directly measured the force exerted by fleeting fluctuations in the vacuum that pace the universal pulse of existence.
>
> Malcolm Browne, *New York Times*

That was the first I'd heard about it, in the *Science Times*. Every Tuesday, this special section of the *New York Times* fluctuates into and then out of existence. I succumbed to its hooks, as the *Times* wanted me to: pacing the universal pulse of existence, a seething sea of subtle activity about nothingness—this is clearly magnificent, awe-inspiring, newsworthy stuff. Forget the details of the "sensitive experiment" described in the epigraph, (as, indeed, the *New York Times* does)—this was physics at its most cosmically impressive. Even the *Economist* was attracted to the story:

> Nature, famously, abhors a vacuum. People are generally just bored by vacuums, because they believe them to be empty space in which, almost by definition, nothing ever happens. For nearly fifty years, however, quantum physicists have had a very different view. A branch of quantum theory known as quantum electrodynamics (QED) says that a vacuum, far from being static or empty, teems with transient "virtual" particles (especially photons, the particles of light) that keep popping weirdly into existence and then disappearing again. But for all their theoretical confidence, physicists have found it hard to demonstrate this.
>
> Until now, that is. Steve Lamoreaux, who works at the Los Alamos

> National Laboratory in New Mexico, has just done something peculiar. As he reported in a recent issue of *Physical Review Letters,* he has shown that if you take two electrically conducting plates and put them close together in a vacuum, they are pushed towards each other by a force conjured up out of the nothingness—the Casimir force. (*Economist* 1997: 84)

The same basic structure is at work in the openings of these two articles: (a) fifty years ago; (b) now. Fifty years of knowing, saying, having a view, confidently theorizing. Then, the dramatic, perhaps cathartic development: "something peculiar," now. Showing, measuring, demonstrating what was known, said, theorized. A very long-term fluctuation between theory and experiment. Other articles from the popular press reiterated many of these terms: the grandeur, the peculiarity, the delicacy, the long historical period:

> In 1948 Hendrik Casimir made the astounding prediction that if two parallel metal plates, both uncharged, are separated by a vacuum, then they should attract one another. Related Casimir effects have been observed in a number of experiments probing the long-range interactions of atoms with metal walls and atoms with atoms. Direct measurements of the wall-wall force have proved impossible, but recently Steve Lamoreaux of the University of Washington in Seattle has now confirmed Casimir's prediction for a related case, the attractive force between a conducting wall and a sphere. (Spruch 1997, 22)

> There's no such thing as a free lunch—except in quantum mechanics . . . A paper in the current issue of *Physical Review Letters* describes the first successful measurement of the ultimate quantum free lunch: the Casimir force, a pressure exerted by empty space.
>
> The measurement, by physicist Steve Lamoreaux of Los Alamos National Laboratory, confirms the strange picture of the vacuum conceived in the 1920s by pioneering quantum physicists Max Planck and Werner Heisenberg. (Seife 1997: 158)

> Von nichts kommt nichts, sagt der Volksmund. Doch in der Welt der Atome und Elementarteilchen scheint diese Weisheit nicht zu gelten; in ihr ist das Vakuum alles andere als gähnende Leere . . . Die Existenz solcher "Vakuumfluktuationen," eine grundlegende Voraussage der Quantenmechanik, hat nun Steve Lamoreaux, Wissenschaftler am Los Alamos National Laboratory in New Mexico, bestätigt. (*GEO* 1997: 169)

The fact that by definition—or "almost by definition," as the *Economist* reminds us, since it always seems to be a matter of the close approach, the fluctuation, the slimmest of departures from direct contact or naming—"nothing ever happens" in the vacuum will have turned out, in the always possible doubled reading, to have been almost exactly right: in the microworld of physics and physical phenomena, something named "nothing" happens all the time. "Nothing ever happens" will be able to be read as "eternal event-ing of zero."

This essay continues to approach, ever more closely, the subjects of the Casimir effect and its recent experimental measurement by Steve K. Lamoreaux. The crude outlines should have emerged by now: a long time ago, a Dutch physicist (who and where was he?) theorized that if two metal plates are carefully brought together (what apparatus could accomplish this?) until nothing but a vacuum remains between them (is a vacuum indeed nothing?), a force emerges from this nothing. What kind of force? A strange, weird, peculiar force—common adjectives frequently applied to quantum theory that signal a fluctuation from everyday logic—a minuscule force that may nevertheless be responsible for magnificent, even apocalyptic events.

That's if you listen to certain theorists. But we're listening here to a certain experimentalist, whose aims and narratives are more modest: to build and calibrate an apparatus that, with a great deal of patient, skilled, and demanding work, will measure the actual force of "nothing"—the eponymous Casimir force, the value of which had long been precisely established by theory. Grand theory makes for compelling reading, and the minutiae of refined substances, disciplined procedures, and carefully articulated mathematical relationships are best left off the table.

Even the genre of the scientific article can only admit the traced outlines of the painfully pleasurable craft of experimenting. Lamoreaux described his experimental apparatus for the readers of the *Physical Review Letters:*

> A schematic of the apparatus used in our measurement is shown in Fig. 1, and the details of the torsion pendulum are shown in Fig. 2. The Casimir force plates comprise a 2.54 cm diam, 0.5 cm thick quartz optical flat, and a spherical lens with radius of curvature 11.3 $\pm$ 0.1 cm and diameter 4 cm; each was coated (by evaporation) with a continuous layer of Cu of thickness 0.5 μm, on all surfaces. A layer of Au was then evaporated (0.5 μm thick) onto the faces which were subsequently brought together. As shown in Fig. 1, the flat electrode was mounted on one arm of the torsion pendulum, while the spherical electrode was placed on a micropositioning assembly. The adjustment screws and piezoelectric stack translators (PZTs) form a tripod . . .
>
> The Casimir force was measured by simply stepping the voltage applied to the PZTs up and down through 16 discrete and constant steps, and at each step, measuring the restoring force . . . required to keep the pendulum angle fixed . . . The PZTs give very accurate and reproducible *relative* changes in the plate separation; the absolute separation was determined by measuring the residual electrical

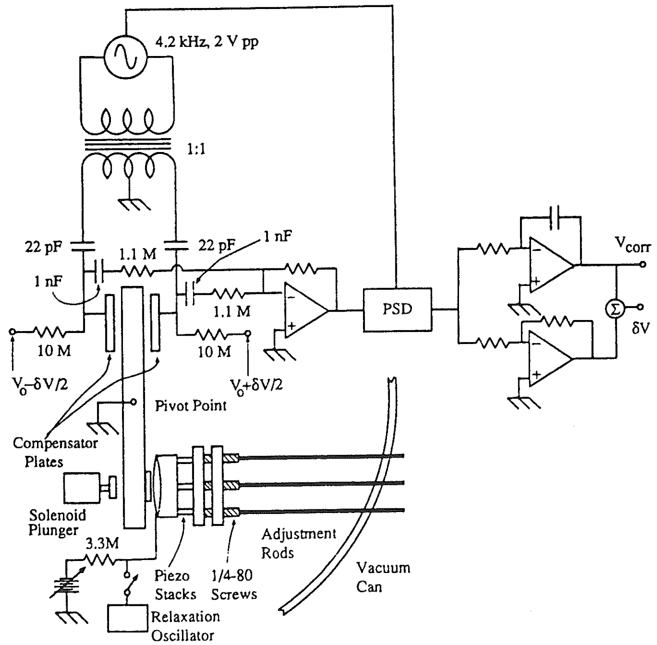

Fig. 1. Schematic of the apparatus. The vacuum vessel dimensions are 55 cm diam by 110 cm tall. The solenoid activated plunger was used to press the plates gently together (during alignment); after such pressing, the plates could be brought much closer. From Lamoreaux (1997); reprinted with permission of *Physical Review Letters.*

> attraction between the plates as a function of separation (the contact potential was intentionally not perfectly canceled). The electric force as a function of separation can be obtained by use of the PFT [proximity force theorem] . . . Finally, the electrical force is subtracted, giving
>
> $$F_c^m(a_i) = F(a_i) - \beta/a_i - b$$
>
> where $F_c^m(a_i)$ is the measured residual force (hopefully the Casimir force). (Lamoreaux 1997: 6–7)

It's the building and operating of the experimental apparatus schematized in figures 1 and 2 that allowed Lamoreaux to take nothing's measure in the world of microphysics while producing a sharp spike in Lamoreaux's fame-curve in the macroworld of physicists, science journals, and the popular press. We know that the passive voice—the modesty-inducing, agency-effacing technology of

writing that has marked the scientific report since the seventeenth century—is intended to erase or zero-out the seething sea of subtle (and not-so-subtle) activity that *is* experimentation.

But it leaks through. Photographs of the actual machinic assemblage might evoke a small fluctuation in your appreciation for the craft of experimenting (see figures 3 and 4). With some effort, you might imagine the slight heft of quartz optical flats that you've just coated with almost invisible layers of copper and gold, as you mount one on the piezoelectric stack and its adjustment rods and the other on a metal bar. You might conjure up the feel of a tungsten fiber; the smell as you solder it onto the brass rod from which you will suspend the metal bar with its coated quartz plate; the powerful sense of the invisible magnetic field that dampens all unwanted motions of the suspended bar; the sound of the "small oil (Fomblin) diffusion pump" echoing in a basement laboratory as it empties the enclosed space of the apparatus of almost everything; the slight pressure of pen on paper and the practically inaudible whisper of inked traces as the data go into the notebook; the numbers emerging in the small hours of the morning as you turn the adjustment screws and the plates approach each other, approach each other, approach each other . . .

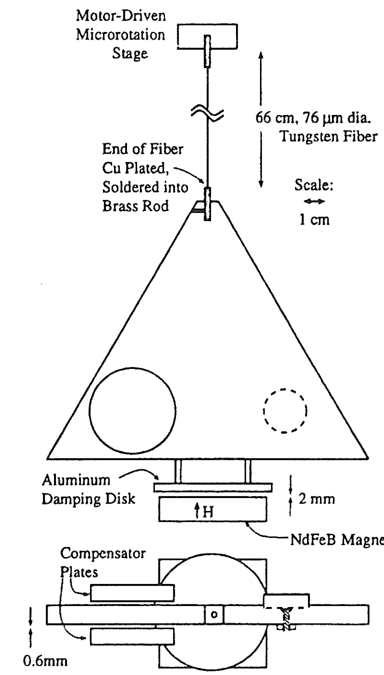

Fig. 2. Details of the pendulum. The body has total mass 397 g. The ends of the W fiber were plated with a Cu cyanide solution; the fiber ends were bent into hairpins of 1 cm length and then soldered into a 0.5 mm diam, 7 mm deep holes in the brass rods. Flat-head screws were glued to the back of the plates; a spring and nut held the plates firmly against their supports, and ensured good electrical contact. From Lamoreaux (1997); reprinted with permission of *Physical Review Letters.*

My approach here to the ethnographic description of nothing and its force in physical theory and experiment crudely mimes the structure of the Casimir force and Lamoreaux's experiment. Trying to grasp "nothing" directly, beginning with a necessarily reductive description of what the Casimir effect *is* and then explicating its theoretical and experimental subtleties, seems to me ill-suited to the fluctuating history of the Casimir effect, to Lamoreaux's experimental pursuit of its measurement, and

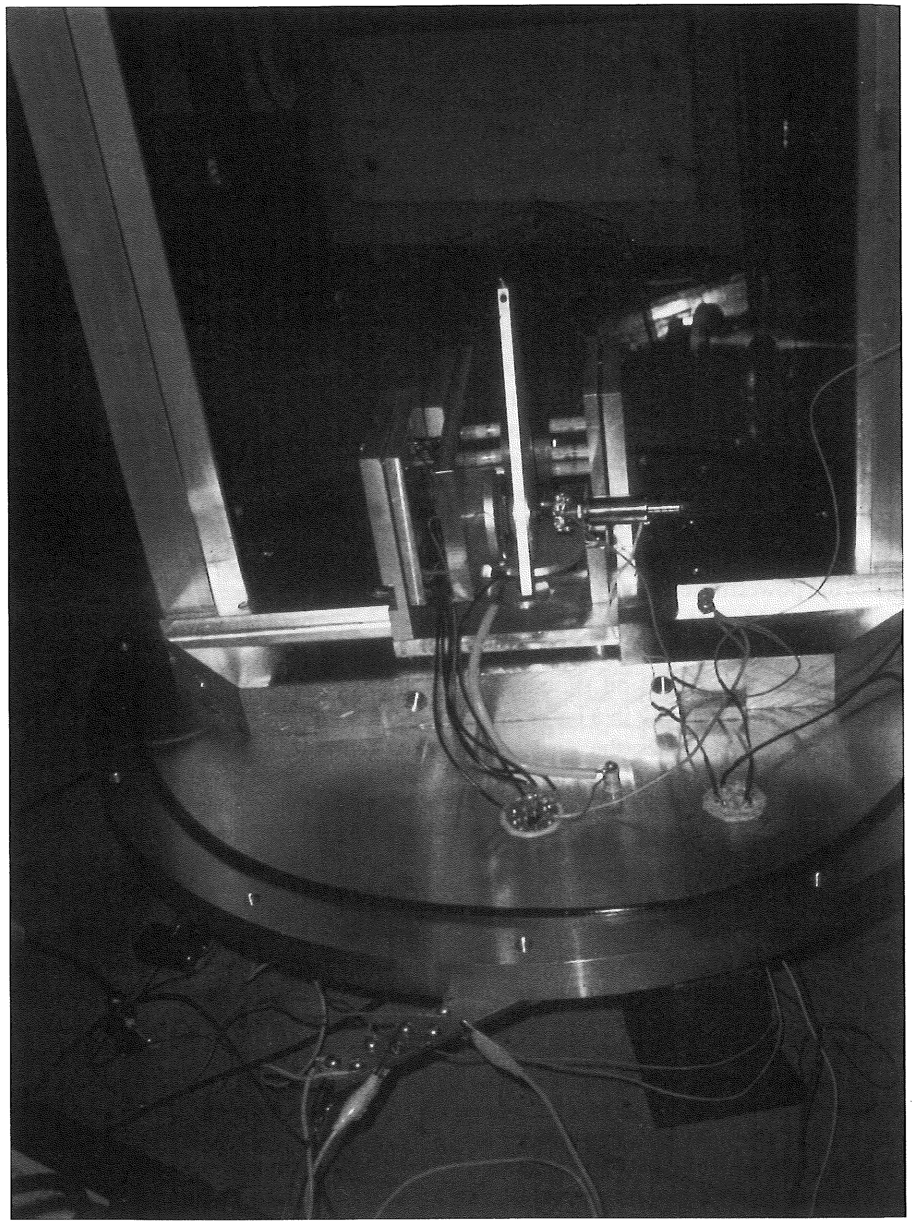

Fig. 3. Photograph of the apparatus; courtesy of Steve Lamoreaux.

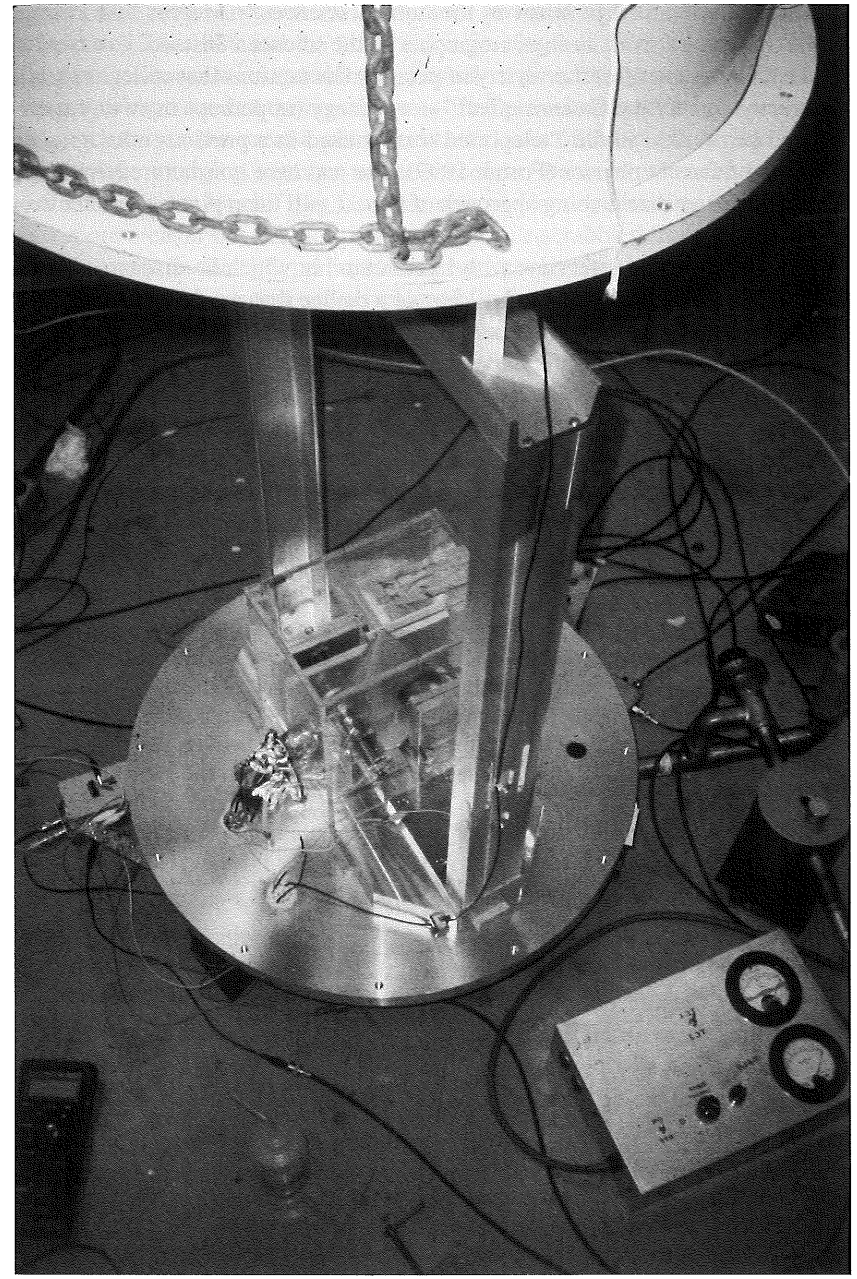

Fig. 4. Photograph of the apparatus; courtesy of Steve Lamoreaux.

to the "experimental moment in the human sciences" (Marcus and Fischer 1986) in which I work as an ethnographer of the sciences. Instead, I've tried to make this text manifest the variety of peculiar fluctuations that collectively but indirectly signal "the Casimir effect"—a strategy (or perhaps my own experimental habit) akin to the "teleported text" I used in a previous ethnographic account of current physics (Fortun 1999). The text here is structured as a slow, deliberate, ever-diminishing approach of two . . . call them plateaus, rather than plates.

One plateau is an interview with Lamoreaux, in which he discusses his interest in the Casimir force, the building of a device that would measure it, and the disciplined work of experimentation. This plateau assembles elements of the ethnography of scientific experimentation that anthropologists, historians, sociologists, and even philosophers of science have started to pursue. Since Lamoreaux referred to the work of physicist and historian of science Peter Galison during our discussion, I quote from his *How Experiments End* (1987) to summarize the stakes and interests of dense descriptions of experiment:

> For years the history and philosophy of science has relied on an image of experimentation viewed through theory. Experiments, when they were discussed at all, were reduced to observations, the psychology of observations, and theorists' use of observations. Ian Hacking has appropriately protested against this parody of experimental work: "Noting and reporting readings of dials—Oxford philosophy's picture of experiment—is nothing. Another kind of observation is what counts: the uncanny ability to pick out what is odd, wrong, instructive or distorted in the antics of one's equipment." It is to the twisting of wire, the shielding of chambers, the hoisting of thousand-pound steel plates, and the arguments over computer simulation that we now turn. Only there, in the laboratory itself, can one see how the miner sifts gold from pyrite. (Galison 1987: 19)[1]

My other plateau consists of excerpts primarily from the physics literature, which offer a different set of articulations of what the Casimir force is—or rather, what Casimir effects *are,* and the various speculations that have fluctuated around those articulations between 1948 and the ever-fluctuating present. Squeezing these two plateaus together produces, I hope, an indirect, ethnographic account of some aspects of how "nothing" is theorized, manipulated, and made meaningful by physicists—over time, and under the weight of certain theoretical and experimental assemblages.

Many other things are happening in this space of the "in between" as well. As some of the popular accounts cited already have suggested, when approaching Casimir effects we will be between the vernacular of the *Volksmund* and the expert dialect of the physicist; between famously common sense and received wisdom, and the fame that briefly attaches to "something peculiar" performed with uncommon talent and invented machinery; between the "knowing" of 1948 and the "directly measuring" of 1998; between the awe-inspiring, imagination-grabbing *gähnende Leere,* the yawning void of the quantum vacuum, and the bored yawns often provoked by metal plates and fine wires; between the precise plans and techniques of the engineer, and the opportunistic kludging together of the *bricoleur;* between what the *Economist* called the "free lunch," the fantastic returns from speculating in theory, and the more meager but still nourishing sustenance scrabbled from an experimental economy. In the end, perhaps, readers might have to reside in the unsettled conjunction of these opposed articulations rather than force a direct translation that collapses all differences—to respect the generative emptiness in between all these betweens, riding the fluctuations of language.

I'm pleased that I never thought to ask what the "K." in the between-space of Steve K. Lamoreaux stood for. The forced uncertainty allows the Kafkaesque possibilities to continue to fluctuate in my imagination. I did have to ask for directions to Lamoreaux's home in Los Alamos, and they bore the traces of what could be called the "ground zero" of American physics: stay on Trinity Drive through town, turn left on Oppenheimer, a quick left, and a quick right, just before the cul-de-sac . . .

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> FORTUN: Tell me a little bit about your background: where you're from, where you went to school, who your teachers were.
>
> LAMOREAUX: I'm from Aberdeen, Washington, famous because Curt Cobain was from there. The kid that organized the band Nirvana lived across the street from us—this was when I was in graduate school that they were doing their stuff. Robert Motherwell, the founder of abstract expressionism, is from that town, too. It's the end of the world: there's no culture there, so people learn to be self-reliant. I went to the University of Oregon in Eugene for one year, then I transferred up to the University of Washington because I decided I wanted to go into physics.
>
> FORTUN: What made you want to do physics?
>
> LAMOREAUX: I was always interested in it. Probably when I was in eighth grade, I decided that being a physicist was something I really wanted to do. I got into that from the *Scientific American* book of projects for the amateur scientist. In the old days, there was this guy, C. L. Stong, who wrote the "Amateur Scientist" column; he told you how to make particle accelerators and all these other things. I just loved that stuff, and it was all physics, so I figured that was what I really wanted to do. Later in high school, I decided I wanted to go into chemistry, but when I got to college I decided to go back to the

physics. I transferred up to the University of Washington and got my bachelor of science degree in 1981. I went to graduate school—curiously enough, one year back at the University of Oregon—I got a master's degree, and then I transferred back up to the University of Washington and finished my Ph.D. in 1986.

For my Ph.D. I did two experiments. One was testing one aspect of the theory of relativity: whether or not one can determine your direction through space. It's like the Michelson-Morley experiment, but a little different. This one could be interpreted as, if the velocity of light was different for light versus material particles, then the speed of light *c* in relativity theory, the maximum velocity, could be different in different domains. So the idea was, if there was a different *c* due to coupling to the motion through space, you would get a distortion of the atomic nucleus with a nuclear magnetic resonance experiment, looking for a shift in frequency. Originally it was called the Hughes-Drever experiment. So we proved the limits to something like a factor of 10,000.

My other Ph.D. experiment was looking for an electric dipole moment, and that would be evidence for time-reversal symmetry. But that's a different story.

FORTUN: Who did you study with there?

LAMOREAUX: Norval Fortson. He was Norman Ramsey's student. Fortson is very smart and knowledgeable, but with more of a theoretical bent. So I learned how to think about experiments from him. I learned how to do the mechanics of experimental work more from the technical support staff at the University of Washington.

After my Ph.D. in 1986, I went to Grenoble, France, and started in neutron physics—ultracold neutrons in particular. Those are neutrons with such low energy that they can be stored in material bottles for times approaching their beta-decay lifetime. I worked with Norman Ramsey there, my teacher's teacher.

FORTUN: How did you come to make that transition?

LAMOREAUX: It was a postdoc position. And it was an electric dipole experiment, so I knew that from my atomic work and decided it was a nice overlap. After that I went back to the University of Washington and was there for the next ten years.

FORTUN: In the much-discussed divide between theorists and experimentalists, where do you put yourself?

LAMOREAUX: I could have gone into theory, since I was usually in the top three or four students in all my graduate courses in physics. A funny thing happened, though: one of the professors said at that time that there was no point in going into physics, because we understand everything, we have the Grand Unified Theory, and it's just a matter of mopping up, so you'll have a boring life if you become a theorist. Then, of course, the proton didn't decay as they had theorized. So I just got a little jaded about theory and decided to stick with experimental physics.

FORTUN: When you say that Fortson taught you how to think about experiments, how did he do that?

LAMOREAUX: Just being able to do very solid calculations associated with an experimental system.

FORTUN: To put it crudely: is it a hand-brain thing? Do you like building equipment?

LAMOREAUX: Yeah, most of the time. Before I started to build something like the Casimir experiment, we didn't have any real plan. We just started putting stuff together. I'd think, we could make it sort of like this, or sort of like this, or this might work. And then we'd do the tests. So it's very geometrical. I don't sit down and draw up a pile of engineering plans and turn it over to the shop. It's much more . . . intimate, I would say.

FORTUN: I ask because there's been more attention being paid to experiment and the work of experimenting in the history and philosophy of science, where the emphasis has more traditionally been more on theory.

LAMOREAUX: Yeah, the first book along that line that I read was *How Experiments End* by Peter Galison. It was really an amazing book; I'd never seen anything like it.

FORTUN: Yeah, it's a very nice book. He's a really good practitioner of that kind of attention to experimental detail. You'd really enjoy his latest book, *Image and Logic.*

So let's turn to your Casimir experiment, then. You were quoted in one of the articles I read as saying, "This experiment was a bit out of my line." I've been thinking a lot about fluctuations, and so I rather like the fact that you go "a bit out of your line" to do this very beautiful experiment. So, tell me how you got interested in the Casimir effect, why you wanted to do this experiment that was obviously very difficult, and that no one had done for decades.

LAMOREAUX: I first learned about it when I was in graduate school, in an advanced quantum mechanics course. And at that time, when we were students learning about this, we were really excited about the question, would it be possible to measure it? At that time, I was working with the "fifth force" people: they were looking for these new long-range interactions, and they had built torsion pendula. In fact, I built the first torsion pendulum at the University of Washington.

So I was thinking about torsion pendula at that time, and when we learned about the Casimir force, I thought it would be really easier to measure that force with a torsion pendulum. I hadn't really thought through what it would mean to keep things positioned to one-micron accuracy and all that. But I did start trying to think it through as a grad student. It was always in the back of my mind, and of course I had my real professional research that I had to do, and carry on with my career. Measuring the Casimir force isn't something you can build a career on. Maybe you could, I don't know.

FORTUN: From reading the review articles, where people talk about its relevance to everything from colloid chemistry to biology to cosmology to the nucleon bag model, it seems like you might be able to.

LAMOREAUX: Oh, I know. Colloid chemistry is an application of the calculational technique; the nucleon bag model represents an "analogous" system. The force is a well-understood consequence of quantum electrodynamics. What I meant is that my experiment doesn't have any direct bearing in those situations. I don't think it would be honest to say that measuring the Casimir force is the greatest thing in the history of science. It's just not the case. But it's very, very compelling intellectually, and that's another use for an experiment: to stimulate people to think and talk about it.

I had a student come along in 1993 or 1994, Dev Sen. He had learned about this from the professor in his undergraduate electromagnetism course. He had a really kind of funny creativity, sort of a genius. So he said he'd like to do this, and I told him I had thought about this before. The first step was making a torsion pendulum with a magnet to damp the swinging. That would give you a very stable rotation: if you disturb the system, it would come back to equilibrium very, very quickly. Otherwise, if you had a torsion pendulum and you bumped it, it would just swing forever. You could never control it. So we had these really powerful magnets—in fact, the pendulum was so well damped, if you let it swing in there, it would just come to its final position instantly.

So the first trick was getting the torsion pendulum and its damping. And we did quick tests on the bench: I set up a ring stand and showed him how to draw a quartz fiber, put a disk on the end of the quartz fiber, put a magnet underneath it, shine a laser on it, and look at the fluctuations on the wall. You could just see, sitting there, that you could get the kind of sensitivity necessary to measure the Casimir effect, no trouble.

Dev Sen built the first apparatus, and that summer I went to Germany for most of the summer. I left him there to do the machining of the parts, and he did a very lovely job of machining it all. I came back and we put it together: I did all the electronics, and told him what he should build and how it should look. So we just built up this apparatus. Our first experiments were with two flat plates, and when we tried to get the plates closer, we couldn't do it. That's because you have to squeeze the air out; but with the type of forces we could apply to the apparatus, to squeeze the air out so that the plates were one micron apart would take about a month. You can do the calculations, it's a simple problem in hydrodynamics. So we were going to have to do it in a vacuum. We built the apparatus so it was modular, and I happened to have an old vacuum can from an old cryogenic experiment. So we had this apparatus that consisted of a torsion pendulum on a frame that stood about waist high, two mirrors, controls for very fine screws for the piezoelectric stacks, and we had this big can that came down over it, and you could pump the air out.

We did all that, and Dev worked and worked, and we tried to work out protocols for getting the plates parallel—that's really the trick. You have two flat plates, but if you can't see them, how do you know if they're parallel? There are a lot of old experiments with dielectric forces, and they could use optical interferometric methods to line them up. So we were faced with this problem of lining up the plates, and Dev just got more and more frustrated. He graduated, and just finally quit the project. It was too much for him. I started to get after him to use one flat plate and one curved plate, and he said he didn't want to do that because that wouldn't really be *the* Casimir effect. Which is not a good way to understand the Casimir effect.

FORTUN: I can understand the reasoning, though. There was one article that said there is no good dictionary definition of *the* Casimir effect. Spruch says that *the* Casimir effect is the parallel plates one, but then there's the van der Waal's retardation forces, there's various ones.

LAMOREAUX: Any time you have two metal objects, I think you can say it's a Casimir effect. If you have a curved plate, you just rethink it as one little tiny plate, and it's sort of the same physics. That's the model, and to do the calculations you make successive approximations.

II. Fluctuations about a Historical (Non-) Origin: Out of the Nether Lands

In the 1940s, J. T. G. Overbeek at the Philips Laboratory in the Netherlands carried out experiments on suspensions of quartz powder used in manufacturing. The results indicated that the theory of the stability of colloids which he had developed with E. J. W. Verwey might not be entirely correct, and that the interparticle interaction might fall off more rapidly at large distances than originally thought. Overbeek suggested that this had to do with the finite propagation velocity of light, and this prompted H. B. G. Casimir and D. Polder at Philips to reconsider the van der Waals interaction. They found that Overbeek's suggestion was correct; as a consequence of retardation, the interaction energy varies as r^{-7} rather than r^{-6} for large intermolecular separations r. This could have been nearly the end of the story, except that Casimir was intrigued by the simplicity of the result which he obtained with Polder and sought a physical explanation for it. This led him to propose a remarkable new effect that has been of interest ever since. Casimir recounts as follows how he arrived at a new way of thinking about the Casimir-Polder result (Casimir 1992):

> "Summer or autumn 1947 (but I am not absolutely certain that it [was] not somewhat earlier or later) I mentioned my results to Niels Bohr, during a walk. 'That is nice,' he said, 'That is something new.' I told him that I was puzzled by the extremely simple form of the expressions for the interaction at very large distance and he mumbled something about zero-point energy. That was all, but it put me on a new track.
>
> I found that calculating changes of zero-point energy really leads to the same results as the calculations of Polder and myself . . .
>
> On 29 May 1948 I presented my paper 'On the attraction between two perfectly conducting plates' at a meeting of the Royal Netherlands Academy of Arts and Sciences. It was published in the course of the year . . . " (Milonni and Shih 1992: 313)

An obscure manufacturing problem encountered at a famous industrial laboratory suggests that theory is "not . . . entirely correct," that the truth fluctuates about a resistant world. Casimir and Polder reconsider the theory that accounts for the van der Waals force (named after another Dutch scientist), the force exerted between atoms or molecules that are very close together. A small but important adjustment is made: the force varies according to the inverse seventh power of the radius, rather than the sixth. The resistant world responds to such demands for precision. Industry exacts this kind of exactitude. This correction of the theory could have been the end of the story, or at least "nearly the end," and inquiry would have been stilled, silenced, zeroed.

Except.

Except something swerved. Except Casimir fluctuated. Except Casimir wavered from this near end, in response to simplicity. The intrigue of simplicity, the intrigue of a certain kind of purity. Mathematical beauty. Casimir fluctuates, seeks. Seeks a physical correlate to this beautiful still simplicity, and proposes, proposes "a remarkable new effect that has been of interest ever since." He presents, the Academy publishes, in Dutch, which few people read.

Casimir hardly ever appears in the histories of quantum mechanics, quantum electrodynamics, or quantum field theory. Casimir's name is a fluctuation, enjoying a sporadic existence in the rare index entry.

Decades later, Casimir remembers, not in a published account, but in personal correspondence to other physicists interested in Casimir effects and their history. The personal correspondence is excerpted in their review article. Casimir remembers a fluctuation, or perhaps Casimir's memory fluctuates: summer or autumn 1947, maybe earlier, maybe later. History quivers, if not time itself. Who can be "absolutely certain" about these things? We can be absolutely certain that Niels Bohr mumbled. Bohr was always mumbling, never moving his lips far from their zero point, where a pipe was often clasped. Bohr mumbled "something" to Casimir, a murmur about zero-point energy, and "that was all." Nothing more. But enough to make Casimir jump the rails and lay down a new track, twin rails of theory and experiment extending into a fluctuating future.

III. Fluctuations about a Definition: *Was Ist das Ding?*

> A definition of "Casimir effect" cannot be found in a dictionary, nor is there universal agreement among physicists as to its meaning, but it is often used in connection with retarded interactions between pairs of systems and changes in the energy of the vacuum produced by the imposition of boundary conditions. We begin with a study of the force per unit area F/A between two parallel metallic walls separated by a vacuum, often referred to as *the* Casimir effect. (Spruch 1996: 1453)

> The Casimir effect takes its name after the Dutch prominent physicist H. B. G. Casimir, who in 1948 published a paper in the Proceedings of the Royal Academy of Sciences of the Netherlands where a rather remarkable property, namely, the attraction of two neutral metallic plates, was predicted theoretically. In all the research papers and reviews about the Casimir effect that have been published in the last years, this paper by Casimir is taken as the undubious beginning of a whole branch of research, which aims nowadays at answering very profound questions about the vacuum structure of quantum field theory (QFT).
>
> However, it is very difficult to get a clear idea from these—on the other hand, excellent review papers—of what the contribution of Casimir was precisely or of what was the specific physical context in which his paper appeared . . . Nowadays, when dealing with the Casimir effect itself, a particular emphasis is usually put on its own spectacularity, that is, on the fact that two noncharged plates do attract themselves in the vacuum. One needs to understand that this is actually much more mysterious *today* than it was in 1948. (Elizalde and Romeo 1991: 711)

> [W]hy is the Casimir effect less understood now than it was 40 years ago? Why did it become a subject of more and more interest as

decades went by? . . . Unlike the van der Waals forces, which are always attractive, the ones appearing in the Casimir effect can be either attractive or repulsive . . . One speaks nowadays of different (generalized) Casimir effects, as due to (1) the existence of a background field in the vacuum; (2) the geometry of the boundary; (3) the dimensions of the space-time; and (4) the possible curvature of the space-time. Summing up, just by closing up in a trivial way the configuration considered by Casimir, we obtain a repulsive pressure, which can in no way be explained by a kind of van der Waals–like force. On the other hand, for the multiple generalizations of the Casimir effect (to different fields, boundaries, dimensions, and space-times), the dependence of the force sign on them is anything but trivial. So is the *mystery* of the Casimir force born. (Elizalde and Romeo 1991: 714)

During [the early 1930s] it became clear that in a relativistic quantum field theory the vacuum is no longer a simple entity. Fluctuations in the charge-current densities as well as in the electromagnetic field strengths gave the vacuum a complex structure. These phenomena also destroyed the correspondence with classical theory. (Schweber 1994: 86)

The commonsense notion of "vacuum" is surely just that it is a region without matter, without any *things*. Here we are at once confronted with the question: what is to count as a thing? What are the basic things of which the non-vacuous world is made? The conceptual framework within which answers to these questions are offered must also be one within which we create a corresponding understanding of the vacuum. This framework is relativistic quantum field theory.

Please don't stop reading now! This theory is indeed an esoteric discipline, but our present ideas about matter and the vacuum are formulated in this language, and there is no escaping the need to attempt some introduction to it . . . To do this, I shall need to rely heavily on analogy, both physical and mathematical. This is valid and useful, I think, as a way of getting the ideas across, but it must not be mistaken for the real thing: the article will not enable the reader to do any actual professional calculations. (Aitchison 1985: 333)

The big bang was an act of creation. Was it a singular, unique event, or is the creation of matter a natural occurrence? And what existed before this event? Was the universe created out of nothing? To better understand how to answer these questions, it is necessary to consider what is meant by nothing, or more precisely, by a vacuum.

[A] vacuum, even the most perfect vacuum devoid of any matter, is not really empty. Rather, the quantum vacuum is a sea of continuously appearing and disappearing particles. However, these particles are "virtual," as opposed to real, particles. Virtual particles are not directly observable. They exist thanks to the uncertainty principle, and the very act of observation would make them real. Energy is "borrowed" from the vacuum to create the particles, and repaid almost instantly . . .

The influence of virtual particles is . . . measured in what is known as the Casimir effect. Cool a gas down to a very low temperature, so that all noise and thermal motions are suppressed, and insert two parallel conducting plates. Outside the plates, all possible fluctuations and virtual pairs can exist, but between them, only certain kinds of pairs are present. If quantum motions are represented by waves, then only those pairs are present whose quantum motions expected from the uncertainty principle can allow exactly a whole number of wavelengths to somehow "fit" in the distance between the two plates. There must therefore be fewer waves inside the plates than outside; the result is a net pressure that tends to drive the plates together. (Silk 1994: 67–68)

Over time, the "mystery" of the Casimir effect becomes articulated in different ways, but most forcefully with new understandings of the vacuum as it is structured in quantum field theory. "Nothing" comes to count within quantum field theory as a kind of thing, albeit a ghostly, virtual thing whose essence is a fluctuating one. "Nothing" is "no longer a simple entity," but something with a "complex structure" that includes the precise concepts and formulae of an "esoteric discipline," coupled with less precise but no less productive physical and mathematical analogies, coupled with the boundary conditions imposed by certain material structures, coupled with "actual professional calculations." The picture of "nothing" that emerges in these articulations is not a "commonsense" one yet can never be entirely free of everyday language and notions. Sometimes these lead to seductive, grand speculations on originary acts of creation, fantastic seas of virtual particles, answers to the biggest of questions. The Casimir effect becomes a trivial detail, a mere measurement. "Cool a gas . . . and insert two parallel plates"—nothing could be easier.

Nothing would be more demanding. Taking nothing's measure will mean accounting for almost everything.

IV. Fluctuations about Midnight: Experimental Performances

LAMOREAUX: After Dev Sen left, the apparatus was just sitting there, but I figured the sensitivity was good enough to do this experiment. I had accepted a new position at Los Alamos, and so I said to myself, well, this is the last chance I'll have to do anything like this until—maybe I could come back to it in my dotage. I had about a year left at the University of Washington, and I didn't really want to start any new projects. I decided to put the effort into getting this to work. So I started working on it, and it was one of those things that just kind of snowballs: one thing after another works, and all of a sudden you have this big complicated experiment that's working.

The trick was to put the curved plate in and improve the vacuum. It turned out the vacuum that Dev was using wasn't really good enough.

FORTUN: You decided to use a portion of a sphere, rather than a cylinder, which I would understand as a kind of intermediate case between a flat plate and a sphere. Why a sphere rather than a cylinder?

LAMOREAUX: Because if you had a cylinder, you'd still have one more axis to align. When you have a sphere, there's really no alignment angle at all. The point of closest approach, and the radius of curvature, geometrically defines the system. So it's really very simple, in fact—once I got the air pumped out, I could get the plates arbitrarily close with no trouble.

FORTUN: Did you make the plates yourself, and coat them with copper and gold?

LAMOREAUX: Yeah, I did. The coating is done with an evaporator, which I built myself, too. It was for putting coatings on laser diodes, but I used it for a lot of stuff. I really miss it. It was one of my pieces of personal professional equipment back in Seattle. I built it all. The way I'd coat the plates was put down a layer of copper by evaporating it. You just get a tungsten electrode or something like that, put a big current through it so it glows, almost like an incandescent lamp filament, and when the metal's hot enough that it evaporates, you can put on tens of microns with no trouble at all. So what I would do is put down a layer of copper, because that adheres well to the glass, and then on top of the copper I'd put down a layer of gold. Gold's a very good substance because it doesn't exhibit something called "patch-effect": electrostatic potential differences between crystal boundaries, sort of like contact potentials, all across the surface. That could give a false signal and possibly contaminate the experiment. So the gold is a very good material for eliminating that.

FORTUN: Is this part of the reason why Sparnaay's 1958 experiment, which used chromium plates, didn't work?

LAMOREAUX: No, but chromium still doesn't work very well. Chromium has the advantage of being very hard, but every experiment that tried to use chromium really hasn't worked. Also, it seems that Sparnaay had some trouble controlling the plate positions.

FORTUN: You had also said that you had first used a quartz fiber to suspend the torsion pendulum, but then you switched to tungsten.

LAMOREAUX: The way we did this experiment, with a capacitive feedback system, the fiber has to be electrically conducting, so that's why we used tungsten.

FORTUN: Why were the piezoelectric stack translators important?

LAMOREAUX: It's just a way that you can electrically displace something. Quartz and materials like it are piezoelectric, so when you apply stress, they generate an electric field. But the converse is also true: if you put an electric field across it, you get a stress in the material. And the stress, according to the Young's modulus, turns into a displacement. Some materials really have a high displacement versus voltage—I think the ones I had were on the order of 14 microns. You apply 100 volts to these things, and they expand to 14 microns. These piezoelectric stacks are made of a lot of very thin wafers, maybe one hundred, stacked up together, all wired in parallel, so you can put a fairly low voltage on and get a big displacement. That's sort of newish technology, probably fifteen years old. They're generally used for lasers and optical experiments.

FORTUN: And how were you actually measuring the distance between the plates—by laser interferometry?

LAMOREAUX: I did that once. See, the piezoelectric stacks, when I put a voltage on, would displace a given amount. And that was very accurately reproducible. So if there were 10 volts on, I knew that the plates would displace 1 micron.

FORTUN: And did you have to do those calibrations, or are they part of the literature?

LAMOREAUX: No, I had to do them myself. The piezoelectric stacks were made in Japan, and there's not much specification on them. It might be fun just to see it. [*Sorts through file box.*] This was the actual calibration of the system. [See figure 5]

FORTUN: It's amazingly clean.

LAMOREAUX: Oh yeah, it's very nice. There's no noise or anything. You could turn it up and down all day and it would do exactly the same thing. I think I did it once or twice just to make sure, and it was always absolutely reproducible. But it was important to always go through the same sequence of voltages. Some people asked, when the plates touched, why didn't you immediately start ramping down? If you did that, then you'd ramp up to here [*finger tracing up the lower portion of curve*], then you'd start the backward downsweep here [*tracing down the upper portion of curve*], and you'd be on a different part of the hysteresis curve. So it's really important to keep the step sequences of the voltages the same.

FORTUN: How long did it take you to do all 216 runs with the apparatus?

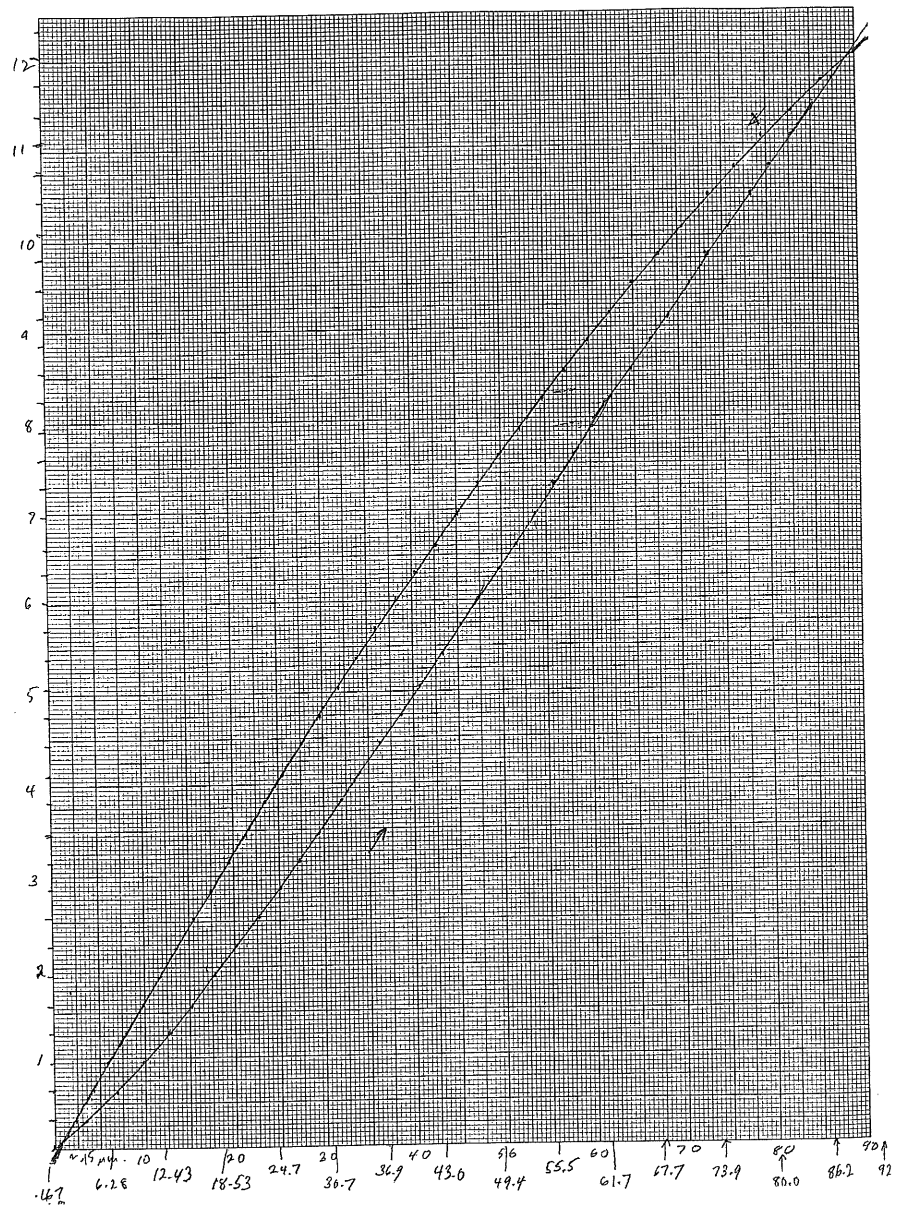

Fig. 5. Calibration curve for PZT stacks from Lamoreaux's lab notebook; courtesy of Steve Lamoreaux.

LAMOREAUX: I started in June 1996. I have the notebook here—it's really sort of a messy notebook. If I go back through and look, I couldn't go back and analyze the data from this, because I was really working at high speed. But at the time, all the calibrations are here, and I went through and double-checked them. [*Flipping through notebook.*] Probably June 20, I really started it, going through mid-July.

FORTUN: Did the runs get progressively better or easier?

LAMOREAUX: Yeah, they did—on both counts.

FORTUN: How do you account for that?

LAMOREAUX: One of the problems was that the pendulum was very long, and the two plates had to be positioned to a sensitivity of less than a micron. It turned out it didn't really matter so much. If they touched during the course of a run, that was easy to tell in the data, because it would just throw off the signal so badly. When the plates would separate again, the signal would come back and it would restore. So if they were close enough it was fine. There was about a fifteen-minute time constant on the response. So I'd work and work and get it set up, I thought, perfectly. I'd measure the capacitance between the plates, these very delicate measurements, spending hours to get it all tuned up. And it would look like it was running perfectly, and I'd come back and look through the window in the lab, and it was off. All of a sudden one day it just occurred to me what was happening. It turned out that when I stood on the floor where I was adjusting it—I was on this big can, I'd have to reach down to the floor and turn these little screws from the outside, to get the plates close. It was on a concrete floor in the basement of the physics building, but my weight on the floor was enough to distort the floor, so the apparatus didn't stay straight. The whole apparatus would tip like the Leaning Tower of Pisa, and when I was standing on the floor, I'd tune up the plates and get them close enough, and then when I'd walk away they'd restore back and be too far apart. And the data would be useless, because they would never get close enough to get a discernible signal. They were about 10 microns apart at their closest; there was no signal.

The best data came when the plates touched during the course of a run. Because even over the course of a day, it would drift by a few microns. So if it was drifting around, there would be different runs, and I could always get the contact points from when the feedback system went haywire.

The other trick that I hadn't realized, even before I started it, was getting the absolute separation of the plates. Because I'd move the plates in roughly 1 micron steps, and I needed to know the relative separation of the plates to better than a tenth of a micron sensitivity. So from the point where the plates contacted, that would only give the separation on the order of a micron. And it occurred to me that there was always this $1/r$ separation force, and that's what you would expect from electrostatic potential between the plates. I was very careful to tune that out. But it turned out that the wires on the outside of the apparatus were picking up the local radio and television stations, so there was a signal of about 10 millivolts there.

FORTUN: Probably playing Curt Cobain . . .

LAMOREAUX: Yeah, could be something like that. It was enough of a signal. It turned out to be the trick to make the experiment work. The raw data would look like this: there would be a $1/r$ relationship and a $1/r^4$ relationship. The $1/r$ would give the absolute separation of the plates, and then that could be subtracted out. At least 20 percent of the $1/r$ signal would be the Casimir effect, so you subtract that off and you get this nice curve.

So I had 216 of these runs. Each one of these took about twenty minutes. Over the course of a day, I'd tune it up, and it would always take a while before it would get nice. So in the middle of the night, the data would be good. Then in the morning, when everybody came in to work at the machine shop, it would go bad. So I got probably five hours a night, because after I got it tuned up, it was usually midnight, and then I would do a couple hours of runs, and then I went home. I didn't go through and catalog when it was bad and when it was good. It was just absolutely obvious. It's one of those things where you have internal consistency in the data, and you know when it's right, when there's not noise. And when the guys in the machine shop are at work, it's just noisy. You couldn't see anything.

FORTUN: The *Economist,* predictably, was the only article that mentioned that it was a very cheap experiment: "Also gratifying was the cost of the experiment. Dr. Lamoreaux's set-up (which began life as a student project) cost only a few hundred dollars. Not quite something for nothing, but in the world of physics, a close approximation." (*Economist* 1997: 84)

LAMOREAUX: Yeah, it was all junk around the lab. The piezo stacks we had for our laser work. In some sense, though, it's not fair to say it's cheap, because the resources existed at the University of Washington. But at the same time, it was at about zero cost. Shows the advantage of having a physics department with a lot of junk around. It's part of the creative process: going to look at the junk pile and making an experiment out of it. I probably couldn't have done it otherwise. I have this very geometrical approach to problems.

When I first started using the curved plate, there was a real quandary about how to make a nice, succinct calculation with a spherical plate and a flat plate. It turns out that the people who did some work on dielectrics knew about this—there's something called the "Derjaguin approximation." I didn't know about it, because I didn't really know the literature of the Casimir stuff that well. In fact, it was a good thing I didn't, because if I had read those papers, I probably would never have been able to build an experiment that works.

FORTUN: Why?

LAMOREAUX: Because I would have attempted to duplicate what they did. And I would have been thinking along a track that would have never been possible. They were all making little mechanical cantilever balances, and there was never really enough intrinsic sensitivity in the systems. One experiment used this trick with a loudspeaker, and you can put things on it, and displace it very small distances by putting a little bit of current through the coil. But somehow there was just never enough force sensitivity to any of these.

FORTUN: So how does the experiment end?

LAMOREAUX: I just ran out of energy to take any more data. I started analyzing the data before the experiment was over, but this business of using the electric force to get the absolute separation between the plates only occurred to me much later. That was on roughly August 1. And you can see on the paper that the receipt date was 28 August. So it was four weeks from when I had the intellectual breakthrough to use the electric force to get the absolute separation.

So I had enough data and didn't have any more energy to take any more. It was really very difficult to spend hours there day and night. I'd end up walking home at three in the morning, and I had to start thinking about going to Los Alamos, and I was just getting worn out. Plus, the data weren't so good anymore; it took a special effort to get the apparatus tuned up so the data were very nice. I can probably tell you when the last data run was. [*Flips through lab notebook.*] July 30.

FORTUN: There's a rather well-known article by Gerald Holton on the Millikan oil drop experiment. He went back to Millikan's lab notebooks and showed how Millikan, because he was committed in advance to the theory that the electron had a unitary charge, knew which data could be thrown out because it messed up the results that supported that theory. But this certainly wasn't fraud or anything so simple as that. Millikan knew when his equipment was working well; he knew when *he* was working well. So he had a sense of when he had a good experimental run and when he had a bad run. And he had no compunction about making the judgment: bad run, bad data, I'm not going to include it.

LAMOREAUX: It's just like in this Casimir experiment. I knew when the data was good and when it was bad. If I just gave somebody my raw data set, I think they'd get the same answer I did, but it might not look quite as good, because I knew when it was really right and when it was off. But they'd get the same answer, but a little more noisy. It's really hard to second-guess somebody who actually knows the apparatus.

FORTUN: It's this kind of craft knowledge that makes the work of experimentation so interesting to historians or anthropologists of experiment. The language isn't perfect, but there's a kind of "feel" for the equipment, or craft knowledge, or tacit knowledge—a hard-to-articulate sense of "this is right" or "things are meshing." There's a lot of different ways to try to say what it is, and they all miss the mark a little. They fluctuate, if you like—but that misses it, too.

LAMOREAUX: Well, there's a certain intimacy with your apparatus.

FORTUN: Some people don't like that language of intimacy, or "feeling for the equipment," either.

LAMOREAUX: Yeah. In some sense, we're not allowed to have subjective feelings about our apparatus. But it's really an extension of the body.

V. Fluctuating about Everything: Where Zero Provokes Infinity

The boundary of a boundary is zero. This central principle of algebraic topology, identity, triviality, tautology though it is, is also the unifying theme of Maxwell electrodynamics, Einstein geometrodynamics, and almost every version of modern field theory. That one can get so much from so little, almost everything from almost nothing, inspires hope that we will someday complete the mathematization of physics and derive everything from nothing, all law from no law. (Wheeler 1990: 10)

There will be a state of minimum energy—the ground state, the state of stability; the other states are "excited" states. *The vacuum is, in fact, precisely the ground state of the fundamental many-field system.* By contrast the excited states can be described as containing quanta—elementary quanta of excitation, as they are called. These quanta are the particle aspect of the field. Thus the vacuum is not a substance, but a state . . . Here we connect to the more intuitive ideas. (Aitchison 1985: 334)

The first divergence of the quantum theory of fields was encountered by [Pascual] Jordan in his quantum mechanical treatment of the vibrating string in the *Dreimännerarbeit* . . . Jordan discarded this infinity by dropping it, thereby performing the first infinite subtraction, or renormalization, in quantum field theory . . . The fact that the vacuum expectation value of the energy could be renormalized to zero does not imply that the mean square value of the energy vanishes. These mean square fluctuations have observable consequences . . . However, in 1931 Heisenberg showed that Jordan's derivation was incorrect: the energy fluctuation was in fact infinite. The infinity stemmed from the fact that the fluctuation had been calculated for a sharply defined region. The fluctuation in the energy in a volume v averaged over a time interval $(t, t + T)$ is infinite if the volume v has sharp boundaries. Only by smoothing the boundary of the volume v will the fluctuations be finite. The recognition that in quantum field theory only smeared operators—that is, only operators suitably averaged over small regions of space-time—make sense was a central idea in Bohr and Rosenfeld's [1933] paper on the measurability of the electromagnetic field. The detailed history of how that important paper came to be written has not yet been told. (Schweber 1994: 108–111)

In rough outline the story is as follows. The quantization of a field à la Heisenberg and Pauli implies that there will be a limit to the accuracy with which one can measure such a field. Just as the quantum rules . . . for a particle imply that the uncertainties in the momentum and position coordinates of the particle must be such that $\delta p \, \delta q \geqslant h$, similarly there will be uncertainty relations limiting the accuracy of certain field measurements. There is, however, a further complication in the case of field measurements: the uncertainties in the field variables at a given *point* in space are infinite . . . In his 1929 Chicago lectures Heisenberg demonstrated that the best one can hope to do is to measure an average over a small region . . . Hence as the region gets smaller the fluctuations become larger. (Schweber 1994: 111)

The basic idea is very simple: the electric and magnetic field vectors of a monochromatic wave of frequency ω undergo harmonic oscillations and, when the field is described quantum mechanically, it has the same allowed energy levels $E_n = (n + \frac{1}{2})\hbar\omega$, $n = 0, 1, 2, 3, \ldots$, as any other harmonic oscillator. In the case of the field the integer n corresponds to the number of photons. The term $\frac{1}{2}\hbar\omega$ is a zero-point energy and implies fluctuations of the electric and magnetic fields even when there are no measurable photons in the field. (Milonni and Shih 1992: 314)

FORTUN: What's the difference between your experiment and the one of Hinds, which received attention a few years ago as a measurement of the Casimir effect?

LAMOREAUX: In the Hinds experiment, if you have a single atom and you put it by a mirror, you get an attractive force, because there's zero-point fluctuations around the atom, and it sees an image of that in the mirror, and it's attracted to itself because of that. That's called the Casimir-Polder force. His experiment was really the first accurate one of one of these Casimir-type effects. Some people were against the idea of zero-point fluctuations, because if this is real energy that gravitates, something like a volume in space the size of the earth would be a black hole. So in a certain sense it's not real energy. There's a whole calculating procedure that one puts on it: arbitrary cutoffs in integrals, subtracting infinities. In the Casimir effect, that's the trick.

FORTUN: It's a really hard thing to get your head around.

LAMOREAUX: Yeah, it is. If you took it at face value and said, "Oh, that's energy, that's the ability to do work, and there's a lot of it there"—it's really inspired a lot of people to say this is a source of power for the future. But it really isn't. Casimir was head of research at Philips for forty years, and if there was an energy source there, the guy who thought up the Casimir effect certainly would have been able to make that easy extrapolation.

But the Casimir force is really just a precursor of the chemical bond, in my opinion. That's the way I think about it. The van der Waals force is really a precursor of the chemical bond, in that—

FORTUN: Wait—when you say precursor, what do you mean by that?

LAMOREAUX: It's the first attraction that the atoms see when they form a molecule, and then when they get closer, the forces get stronger and stronger. They go from the van der Waals limit to more of an electrostatic, dielectric effect. So that's why people call it a precursor to the chemical bond.

FORTUN: Since this was one of the things that I wanted to ask about, maybe we can flip-flop or fluctuate back and forth between the very practical and the very theoretical. I really like the fact that it's Overbeek at Philips Laboratory who's working with a quartz colloid, that starts this ball rolling. So there's this manufacturing problem at the origin of this story. And throughout, scientists have talked about how the Casimir effect might have very practical consequences in terms of colloid chemistry, biology, and so on and so on. That kind of linkage between the very practical and the very theoretical is something that interests me.

LAMOREAUX: Casimir is really a genius. Somehow he got taken over by the "dark side of the force," some might say. Philips really paid him a lot of money. He'd go to physics conferences, and ride in a chauffeur-driven limousine, and all the other guys were streaming off the public transport all wet and drenched and muddy. He had a very good life, and he was able to be creative for a lot of it, too. But at the same time, I think he strayed away from "mainstream" physics questions, the type of studies that would make him really famous, like Schrödinger. However, it's clear that Casimir had a much better understanding of physics and its issues than did many of the "famous" people from that era, about 1930 to 1950 or so.

FORTUN: I was quite struck by the way he has been kind of zeroed-out of the history. I went through a whole stack of books—the ones that happened to be on the shelf at Hampshire College—on the history of quantum mechanics and quantum electrodynamics. But Casimir isn't even in the index in the vast majority of them. I found a funny story by George Gamov, about him and Casimir and Niels Bohr climbing the wall of a bank in Copenhagen after a night of drinking. But otherwise, Casimir is almost not in the history at all. He doesn't ap-
pear in Sam Schweber's massive and thorough *QED and the Men Who Made It.* And I guess it is because he worked at Philips.

LAMOREAUX: He wasn't in with the crowd, so to speak. But somehow I think that coming up with these forces and the Casimir effect was an intellectual catharsis that had effects on the entire field. I was in Germany and staying with a friend who had the Schweber book, and I was looking in it for Casimir, too.

FORTUN: Yeah, he has a nice section on the zero-point energy, almost all of which was way beyond me, but I think it was mostly in terms of the debate around renormalization theory, and Casimir doesn't appear at all.

LAMOREAUX: But it does show indirectly that Casimir was really on the cutting edge of physics at the time.

VI. Fluctuations about the Cutting Edge: Cosmological Speculations

> Fluctuations . . . play a dominant role in the interaction of distant atoms or electrons with surfaces and of distant surfaces with one another. It is in these areas that high-precision confirmations of retardation effects have been made. Ultimately, Casimir effects may be of much greater importance in biology, where basic systems, such as cells and surfaces, can be large . . . There also have been hints that truly dramatic Casimir effects may be present in astrophysics and cosmology. (Spruch 1996: 1452)

> We shall deliberately slant our account towards what is certainly the most revolutionary possibility—the feasibility of *creating* the universe out of the *vacuum!* There could hardly be a more remarkable interconnection than this between "something" and "nothing." (Aitchison 1985: 385)

The quantum vacuum . . . inevitably contains fluctuations, although they are on an infinitesimal length scale. Inflation captures these fluctuations and amplifies them up to scales that correspond to those of galaxies, galaxy clusters, and beyond . . . As for the details of the inflationary particle physics, any general prescription for inflations seems unachievable. There is an infinity of initial conditions for inflation, almost all of which do not result in a universe compatible with what is observed.

The moral behind this is that we have only one universe to try to comprehend. It is often thought desirable in particle physics to avoid both small and large dimensionless numbers, since these are often considered unnatural, and, by the same token, to avoid any seemingly special initial conditions. The attitude of cosmologists is somewhat different: for one thing, there is only one universe to study, which makes it difficult to argue about the desirability of "naturalness." Maybe apparent "fine-tuning" is not too bitter a pill to swallow. Ultimately, we may hope that a theory of quantum gravity will emerge to provide the possibly unique initial conditions that led to inflation. (Silk 1994: 183–185)

Dr. Michael S. Turner, a cosmologist working at Fermilab, has speculated about some sinister possibilities. [He and Dr. Frank Wilczek] hypothesized the possible existence of a false vacuum. After the Big Bang . . . the young universe might have allowed for various different energy states, and might have settled into one that was not the lowest possible energy . . . If the bottom were to drop out of the present vacuum, they wrote, "without warning, a bubble of true vacuum could nucleate somewhere in the universe and move outwards at the speed of light, and before we realized what swept by us our protons would decay away," annihilating all atoms.

This disquieting possibility was later discounted by Dr. Piet Hut . . . [and] the notion of a deadly vacuum collapse has been undermined . . .

But it remains a possibility, [Turner] and Dr. Wilczek said. Another possibility, he said, is that the universe might have been created to contain several vacuum "domains," not just the one we know. These domains might have the same vacuum energy, but within them, "the realization of the laws of physics might be very different," he said. The wall between domains would contain enormous energy, "and you certainly wouldn't want to be in that wall."

"The energy of the vacuum remains one of the deep mysteries of science," Dr. Turner said. (Browne 1997: C6)

It must be admitted that all of the above is pure conjecture, and is only a possible "way in which things might have been." Inflationary models are, however, remarkably successful in dealing with a number of serious problems which faced the previous standard hot big-bang cosmology . . . For this reason they continue to dominate contemporary cosmological discussion. Apart from this, [they] offer the fascinating and profound possibility of understanding the uncaused origin of the universe from, in some sense, nothing. We put in the qualification for the following reason. The reader will surely, by this stage, not need any convincing of the point that in quantum field theory "nothing" is far from being a featureless, quiescent, property-less void. Perhaps the main message of this article is that quantum theory teaches us that the "classical vacuum" state, empty of all matter and free of all fluctuations, is not physically realizable; matter cannot be relied upon to be absolutely non-existent in any region of space. Of course, this is initially only true on the microscopic scale . . . but the inflation idea allows the microscopic fluctuation to expand into the whole cosmos. At the quantum level, that "nothing" out of which the whole universe might have fluctuated into existence is indeed a most volatile and fecund medium. (Aitchison 1985: 390)

FORTUN: You've been quoted as saying "it was the most intellectually satisfying experiment I've ever done." Can you tell me what the intellectual satisfaction was?

LAMOREAUX: Measuring this quantum effect directly on a macroscopic apparatus. Of course, whenever you do an experiment in quantum mechanics you have to have a macroscopic apparatus, but this is at a more subtle level, subtle in that it was direct. It's a funny play on words there.

I've gotten a lot of letters from people saying, oh, everybody's measured this before, they've had all these hundreds of dielectric measurements. But the force between the conducting plates is really different. And I finally came to some clarity on this and worked it through in my own mind about six months ago, what the difference is. Even the Lifshitz theory says that there's two contributions to the force: one is the atom-atom interaction in the materials. In a dielectric material, every atom in each plate sees every other atom on the other plate. So you just do a sum over every pair, and you get the force. And when you do it right, you put in boundary conditions, which modify the zero-point fluctuations. The Lifshitz theory says that in addition to the atom-atom interaction, there's the modifications in the zero-point fluctuations of absolute space. He doesn't use exactly that terminology, but he says there's two contributions. One is zero-point moments of space, the other is atom-atom interaction.

Now when you have conducting plates, the atom-atom interaction is completely gone. It's just really the free space that is affecting the plates. And you can see that in the formula with the $\hbar c$. [*Flips* *through his* Physical Review Letters *article, points to equation (1) there:* $F(a)/A = \pi^2/240\ \hbar c/a^4$.] The only constant—there's no material properties there; the charge of the electron doesn't appear. For the dielectric case, there's always an index of refraction, or the permittivity of the material in there; there's an implicit charge of the electron in all that. So the Casimir force, in my opinion, is a property of absolute space. It's not a question of whether it's a retarded van der Waals force or whatever. It's really something different. Because when you do the van der Waals case, you just add up atom-atom interactions. But when you have a conducting material, an electromagnetic wave can't propagate in the material, so it would just be a thin layer of atoms on the surface that would contribute to the force. So in my opinion it's really completely different from the dielectric case and the van der Waals force.

One letter was a little bit angry, saying this has all been done before, and you didn't reference all the papers properly, on and on. I wrote back, but I wasn't sure what the guy was getting at. It turns out I missed a bunch of papers that were—

FORTUN: —his.

LAMOREAUX: No, not of his, but of the Dutch group—the Overbeek group. They continued on with their work, and there was some paper in the *Transactions of the Faraday Chemical Society,* where they made a Casimir balance, sometime in the 1970s. The reference is probably in that Melonni and Shih article. But I didn't see it. But anyway, their experiment didn't work so well; it wasn't much better than the Sparnaay experiment, in my opinion.

FORTUN: What's your line at cocktail parties, or to distant family members, about what you did? The *New York Times* puts it in those very catchy terms, "physicist measures the force of nothing," the "universal pulse of existence," and so on. All of this language is wrong, but it's also right in some sense. And I have to admit that it was the *New York Times* article that first caught my eye, with all those grand words and ideas. Even when I was preparing to come out here, my one-liner to friends and family members was, I'm going to interview this physicist who measured nothing. And they would be really interested. But then to try to explain it, both the theory of it and your experiment, becomes very difficult. So how do you think about the relationship between what you did and all these grander concepts?

LAMOREAUX: I'm a bit of a ham, so I like to play it up at a cocktail party if somebody asks me. I give them the spiel that even in the absence of light, there's always photons. I try to say what a photon is, and I say that there's always half a photon around. And if you do something to limit the size of those photons, you get a force from the outside. And that's the easy explanation, and I think it's fairly correct.

FORTUN: What about the cosmological stuff about inflationary universes? Or the "death by vacuum" disaster scenarios that the *New York Times* scared up for its readers?

LAMOREAUX: I don't really get into that. I could talk about the fact that these photons supposedly have mass, and if you add up all the mass in the universe, it's a rather large value, and we haven't really come to grips with that yet.

It's been a while since I talked to members of the lay community about this. I talked to an engineer recently, and he asked a lot of detailed questions about the apparatus: how did you keep it stable, how did you control it down to 1 micron, things like that.

FORTUN: So your intellectual satisfaction really comes from making the experimental connection of the quantum to the classical, and not from the big ideas?

LAMOREAUX: Yeah, building up this apparatus, and really doing it all myself, seeing it work, and getting the answer out. And it came out so cleanly. It's such a fundamental principle; it's the electromagnetic stress of empty space. Being the first one to measure that, and really see it, and have a good experiment—it was very, very intellectually satisfying. In a certain sense, it's not going to change the world. Everybody knows that quantum electrodynamics is correct. The magnetic moment of the electron is calculated with such high precision, for example, that there's just no chance that QED is wrong. The fact that zero-point fluctuations are responsible for the decay of excited states of atoms: we absolutely know that these fluctuations are around. To take Casimir's prediction from such a long time ago, and set up an experiment to measure that—it's something I dreamed about off and on for a long time. Fifteen years, maybe.

FORTUN: You said that it's only recently that you've come to really understand this at some satisfactory level. These kinds of theories of fluctuations about the zero-point energy seem not to have been so important to you for thinking about your own experiment.

LAMOREAUX: I would say not at all. We just had a real prediction that there should be this force between conducting plates that would go as the distance of separation to the fourth power—when one plate's curved, it's as the separation to the third power, but that's a technical detail. I just set out with a monomaniacal goal in mind, and then when it was done, it started sinking in what it all meant.

FORTUN: So the kinds of philosophical conundrums about nothingness really meant nothing in terms of your dream of doing this work?

LAMOREAUX: It was in the back of my mind, certainly. When I first learned about it as a graduate student, I thought it was really amazing that empty space had this property. Then the first thing I thought was, wouldn't that be fun to measure? Later on, I went through all the papers, because I really didn't know the theory so well, and learned how to do the calculations. And I came to grips more with what it really means.

FORTUN: In fact, QED doesn't really appear in your paper at all.

LAMOREAUX: No, not really. I quote this result here, then this temperature correction is a bit of a tricky calculation. Then there's actually a typographical error: this should be a 1 minus this term in equation (5), rather than 1 plus.

FORTUN: That always happens.

LAMOREAUX: I know. Once you make it, you never see it.

FORTUN: It always amuses me when I sit in on a colleague's physics class, and he'll be writing equations all across the blackboard, and then he looks back about ten equations previously and says, oops, that should be a minus, sorry.

LAMOREAUX: Yeah. If you ask me, I can tell you what it should be. I understand it. You just don't see it happening.

VII. Fluctuations about Uncertainties: Dreams and Nightmares

FORTUN: In theory, these are asymptotic curves, right? [See figure 6 below.] This might get us back to the relationship between zero and infinity: if you actually kept approaching a separation of zero, the force would go to infinity?

LAMOREAUX: In principle, it would. But if you go back to this equation here, you see this correction. Eventually that takes over. This is just a first-order correction, but after a while, when the plates are so close that the frequency of the electromagnetic waves of the zero-point fluctuations that you're affecting is so high, they're like X-rays and just go right through the material and no longer have any effect on the apparatus. So this is all very approximate for a real material, and you have to make what's called a plasma frequency correction. In fact, this is a very crude model, because if you use that formula to predict the correction on the point of closest approach, it would be a 20 percent correction. But if you take the real properties of gold, it turns out to have no more than a 5 percent correction. That's my experimental accuracy. When I first finished the experiment, I thought, did I do something wrong? I put my results in that formula, and I thought they should be off by 20 percent. But I looked a little more

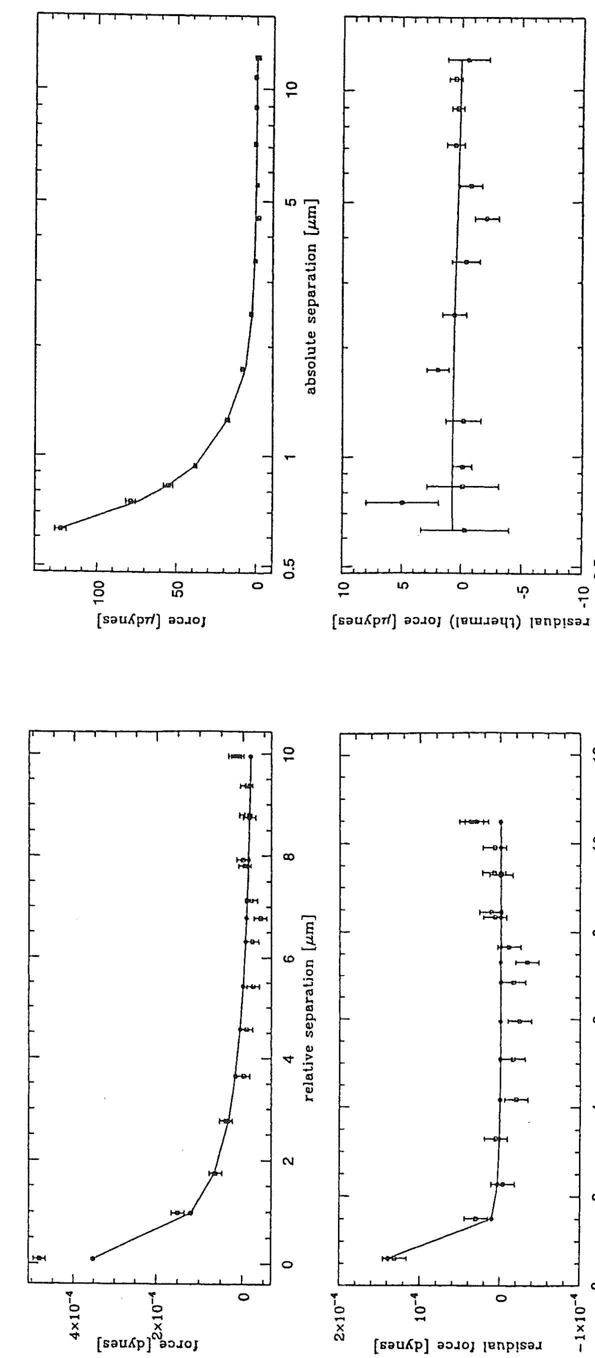

Fig. 6. *Top left:* Measured force as a function of relative position and least-squares fit to $F_c^T \times (a_i + a_0) + \beta/(a_i + a_0) + b$: the two points at closest approach were not used in the fit. *Bottom left:* Measured force with electric contribution subtracted; the points connected by lines are as expected from the Casimir force. *Top right:* All data with electric force subtracted, averaged into bins (of varying width), compared to the expected Casimir force for an 11.3 cm spherical plate. *Bottom right:* Theoretical Casimir force, without the thermal correction, subtracted from top [right] plot; the solid line shows the expected result. From Lamoreaux (1997); reprinted with permission of *Physical Review Letters.*

closely at the theory of electrical properties of gold, and I convinced myself that the correction should be much less than 20 percent. I was really dismayed at first. I could only imagine doing some sort of funny mistake, some error in the computer program generating fake data. But there's really no way that could be possible. Still, you're wondering: I thought I should be off by 20 percent. And then I just forced myself to look through the theory. It's not so easy, but by some back-of-the-envelope calculation, I convinced myself there should be this smaller effect.

In fact, a lot of people are interested in doing this sort of proper calculation for a real material. Claudia Eberlein tries to explain sonoluminescence by the Casimir effect. In sonoluminescence, you have an ultrasonic bath, and you get little bubbles which collapse and give off flashes of light. One hypothesis is a dynamic Casimir effect. She's interested in doing this calculation for the correction of gold electrodes.

FORTUN: Are you following what people are trying to do on the experimental plane?

LAMOREAUX: Not really. I think theorists really like to do this finite conductivity calculation for a real material, because they're a good proving ground: give a student a problem like that and they can develop experimental skill and learn technique. But in a certain sense it's not such compelling work. But it could be that this experiment is wrong, and somebody really does the calculation, and there's a 15 percent correction. And my experiment could be off by 10 percent.

FORTUN: I was going to ask if you have nightmares that in ten or fifteen years, somebody's going to find that you didn't account for X, or this part of your apparatus could be an error source, or something like that.

LAMOREAUX: Well, this is my experimental result. That's all I can say. I explained how I got it. If there's something different, it would have been an artifact of the experiment, but that's the data that came out. My apparatus exists. If someone wants to take it and do it themselves, I'd be happy to give it to them. I've actually given it away a few times. I've given a few colloquia on this, and people are so taken in by it, they say they'd really like to have the apparatus, maybe put it in a student lab. So I've given it away a few times now, but no one's ever claimed it. It's one of those things that sounds great when you first hear it, but then you think about what a nightmare it would be setting it up.

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The bottom line, as it were. Block out the theorists' grand narratives—not because they're unimportant, unproductive, or uninteresting, but only because these meticulously produced and plotted points here are a better sign of the

experimentalist's "intellectual satisfaction." Read the graphs in figure 6—especially the top right graph, which Lamoreaux (1997: 8) considers to be "the most striking demonstration of the Casimir force"—from right to left. This is the distillation of 216 trials, the remarkable residue of the experimenter's patience, craft, solitude, frustration, boredom, passion, calculation. The two plates begin 10 microns apart; there is nothing between them, and the force between them is zero. A small voltage applied to the piezoelectric stacks pushes the plates a micron closer together: still nothing, still zero. Again and again and again: zero, zero, zero. When the plates are finally less than 2 microns apart, the nothing between them begins to become something: the suspended bar moves imperceptibly. Departure from zero. When the plates are separated by less than 1 micron of vacuum, the swerve is clear: nothing has fluctuated, and fluctuated measurably, registering on the bulky equipment the force of nothing but absolute space within these boundaries.

Sheer graph of nothing's trace.

Except . . .

As fond as I am of that exquisite phrase, it goes just a bit too far. Too close to the phenomenon it names, perhaps, it would cause this ethnographic account to collapse into a grandeur similar to that so greedily invoked by the popular accounts of Lamoreaux's experiment. It's better to close by snubbing the clean marks of the published work, and fluctuate one last time by returning to another kind of writing, a different set of traces: the pen strokes of Lamoreaux's lab notebook.

Lamoreaux himself had to return to these all-important marks long after they had been transformed into "results" and published, as he wrote in a letter to me:

> Thanks for sending the draft of your article for me to proofread. It's really very good and I enjoyed it immensely. I have just a few suggested changes, and I'd like to tell you about my recent work in the field (which corrects an error in my measurement, and the story might amuse you).
>
> . . . I recently did the proper calculation for the finite conductivity effect. I found that the correction for my experiment should be around 10–15%, outside my quoted accuracy of 5%. I wrote an erratum (copy enclosed) which was recently published in *Physical Review Letters.* I traced the disagreement between theory and experiment to my radius of curvature measurement; on pp. 108–109 of my notebook (copy enclosed) [see figures 7 and 8], I have 11.3 (used in my original paper) and 13.54 cm; in the notebook I have written "take 12 ± 1 cm for now and do better later." [See figure 9.] Well, later didn't come until quite recently, and I can't find the analysis that led to 11.3 ± 0.1 cm as used in my original paper. I think the 0.1 comes from the measurement that gave 13.54 cm, and at some point I assigned that

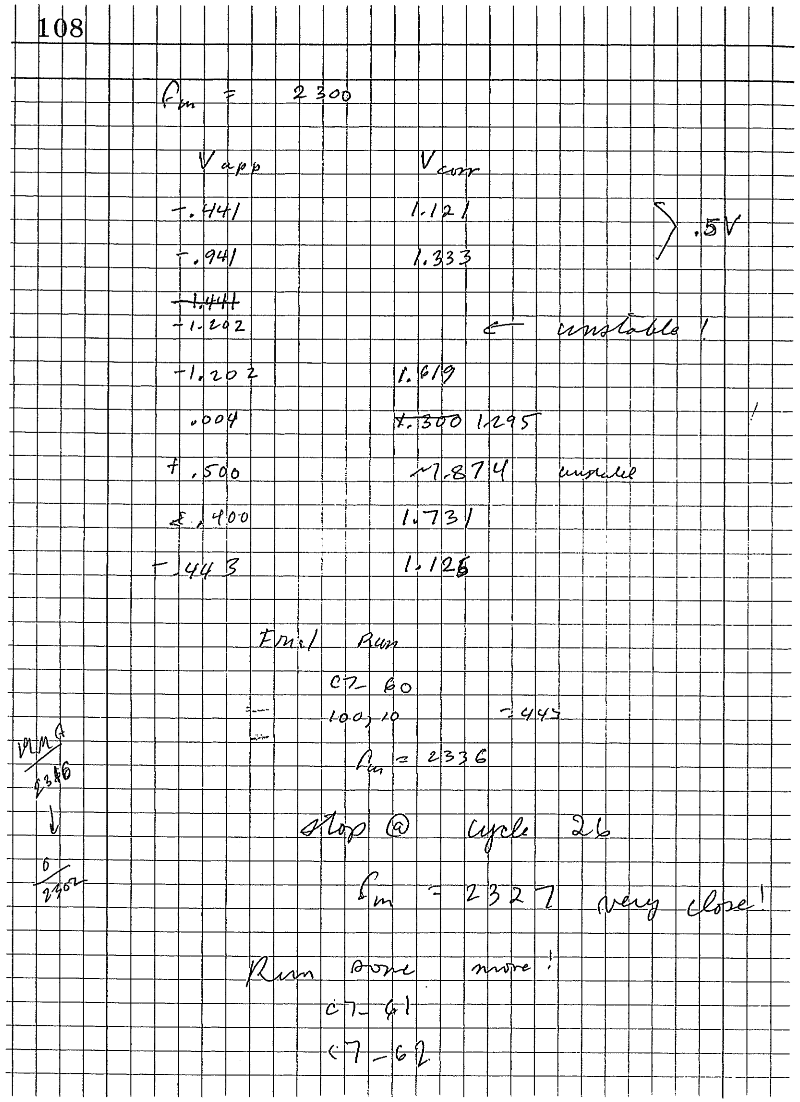

Fig. 7. Experimental data from Lamoreaux's lab notebook; courtesy of Steve Lamoreaux.

error to the 11.3 cm measurement. Anyway, the possibility that something was wrong was in the back of my mind, and I brought the curved plate with me to Los Alamos; I redid the curvature measurement, getting the result reported in my erratum. I really feel now that the experiment is complete; if you read between the lines on p. 34 of your

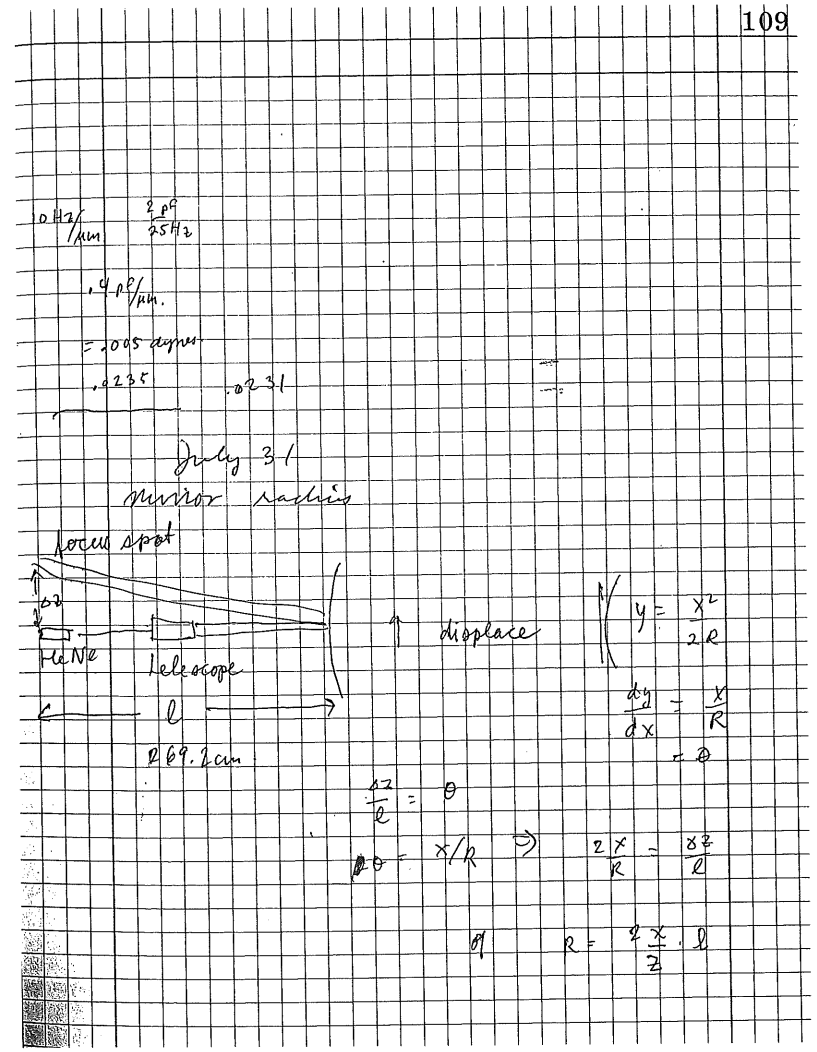

Fig. 8. Experimental data from Lamoreaux's lab notebook; courtesy of Steve Lamoreaux.

paper [pp. 152, 154] above, my comments indicate a certain dissatisfaction with my result and with the field. Anyway, I now feel that the experiment has really ended.

What really prompted me to work through this was a request by *Phys. Rev. Lett.* to referee a manuscript concerning the measurement of the Casimir force by use of atomic force microscopy. I originally agreed that the paper should be published but only after doing a better

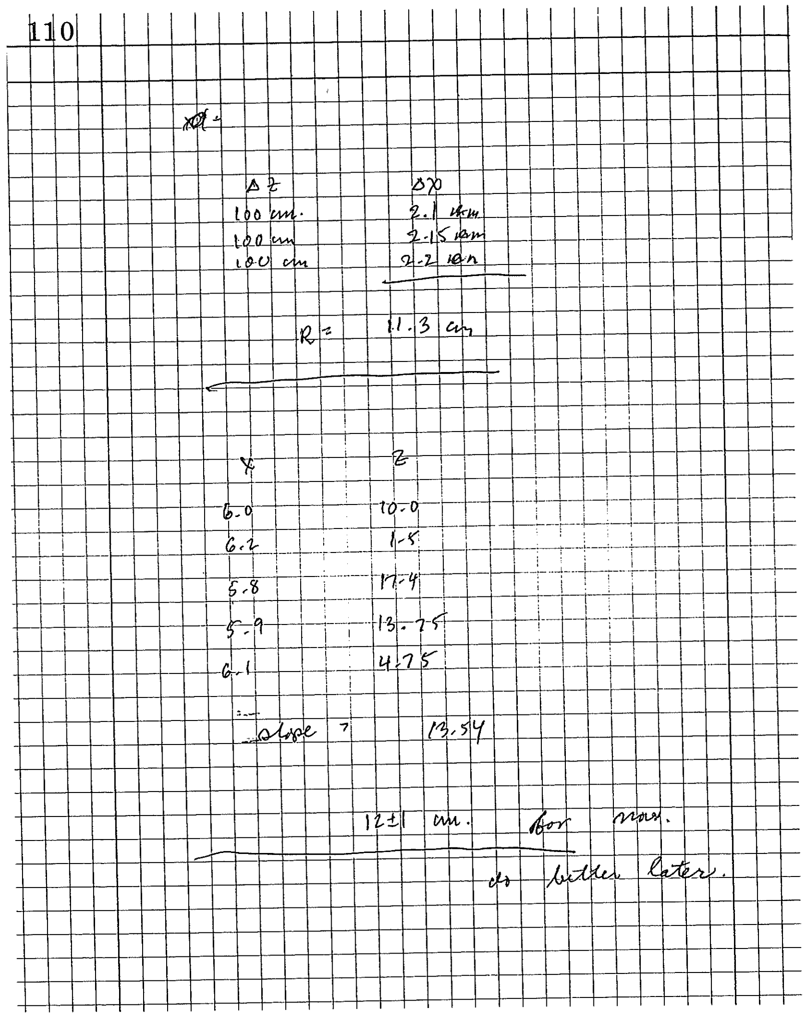

Fig. 9. Experimental data from Lamoreaux's lab notebook; courtesy of Steve Lamoreaux.

job on the theory . . . The authors refused and I rejected the paper, but three other referees thought it should be published, and *PRL* did so (I guess to *PRL* science is a democratic process). This paper also has some reference to my work, and why they think it is wrong. Anyway, all this prompted me to do a proper theoretical calculation for all the effects, and the 1% measurement claimed by the authors has a theoretical uncertainty of over 50%. Under usual circumstances I would accept their work at face value, but the authors included my suggested

theoretical corrections which amounted to 30%, but they maintained the 1% agreement to the same data. I wrote a comment (enclosed), and depending on what their reply to it is, I might conclude that their work is invalid.

As you can see, I still have a bit of fun with the Casimir force, but I now consider my work in the field complete.[2]

The truly dedicated grammatologist of science would run down all of the writing and reading effects in play here, the signs of how science works through all manner of fluctuations: lapses of memory, passages of time, refinements of theory, and occurrences of errors. Such inescapable fluctuations require more writing mechanisms to accommodate them—the erratum, the comment, the referee report, the "(copy enclosed)." A sociality coheres in the process. Other fluctuations can't be directly remarked upon, although they can be gestured toward with those handy phrases "read between the lines" and "in the back of my mind."

The laboratory notebook is of course a most utilitarian writing technology: a space for recording, calculating, and diagramming. The relative permanence of its marks counteracts a scientist's fluctuating memory, and permits the checking and rechecking so essential in the event of error or the simpler event of time itself. But there's an aesthetic quality to its pages as well, an accidental balance of lines, figures, emptiness: the columns of numbers on the march, the occasional θ or δ summoning up a distant and opaque heritage, the equations succeeding each other according to rules that are not present on the page, and of course the rules that are there—the gridded Cartesian background that is indeed background, and *must* be written over, in a handwriting that is neither precise nor imprecise, for science to happen. And the markings here that read "unstable!" "very close!" "Run some more!" and, of course, "[take] for now . . . do better later"—isn't there, in these few blotches of ink, an entire philosophy of science? They are at least signs that among the many marks that experimentation inevitably produces, are exclamation marks.

So, as lovely as "sheer graph of nothing's trace" would have been as a closing set of marks before the white space washes in again, a more appropriate epigram would be the simple statement on Lamoreaux's experiment made by fellow physicist and Casimir buff Larry Spruch: "It is pleasing, interesting and exciting that Casimir effects are at long last not only conceptually grand but also measurable." (Spruch 1997: 23)

Notes

1. Galison is quoting Ian Hacking, *Representing and Intervening* (Cambridge: Cambridge University Press, 1983), p. 230. One could hack Hacking's protest and redirect its message: an ethnography of experiment takes us from nothing to the uncanny, from absence to oddly informative fluctuations.

2. Lamoreaux to the author, December 22, 1998. Lamoreaux's "Erratum" was submitted to *Physical Review Letters* on November 22, 1998; his "Comment" on December 22, 1998. The other paper in question is U. Mohideen and Anushree Roy, *Phys. Rev. Lett.* 81, 4549 (1998).

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DOUGLAS R. HOLMES

11

SOCIETY LOST, SOCIETY FOUND

"There is no such thing as society." It was Margaret Thatcher who first articulated the negation in her now-famous utterance. Her reasons for making the pronouncement are still not entirely clear; it is far more likely that she made the assertion out of pique and rancor than as a broad ideological premise let alone a radical theoretical postulate. Nonetheless, in the more than a decade that has passed since her emphatic observation, its meaning has been vigorously contested by others. Marilyn Strathern comments on this much-debated disavowal of society, "What is breath-taking is that the leader of an elected political party [Margaret Thatcher] should have chosen the collectivist idiom to discard. What vanishes is the idea of society as either a natural or an artificial consociation. What also vanishes, then, are the grounds of class dialogue (the naturalness or artificiality of social divisions) that has dominated political debate and reform for the last two centuries" (Strathern 1992: 144). What is ironic is that as Baroness Thatcher declared this evacuation of the social, she began to perform the cultural labor that would refill it, that would instill a new rendering and a new substance of society.

In this text, I examine the exploits of two European politicians, the French nationalist Jean-Marie Le Pen and the British Prime Minister Tony Blair, as they address from radically opposed perspectives the eclipsing of the modernist project of society—the science, political economy, and metaphysics of solidarity. I argue that the work of these politicians depends on restoring and exploiting "new" social imaginaries that can impart perspective and confer distinction on human relations, and it is this intellectual engagement, this practice, that renders them crypto-ethnographers. In this sense, the politician and the anthropologist face at the close of the century a common dilemma: how to reclaim society. I show how Le Pen and Blair distill new forms of perspectivism to align new structures of feeling and to create new, highly contingent bases of relationality. Their interpretative accounts seek to link the individual to a selective or exclusionary rendering of collectivity. What they create are