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On the Importance of "Being There"

Introduction

Eric Livingston has a wonderful reflection on a feeling every empirical investigator has had.

> I can spend a lot of time doing things that I don't think are getting me anywhere. I am not seeing or finding anything new. Whether or not such appraisal is correct can't be determined at the time. The larger point is that without "being there" we have little opportunity to learn anything. (Livingston, 2008, p. 156)

What Eric means by "being there" is, of course, much more than simply being present on the scene. We have to be present *to* the action going on as well. And for that, we should be clear about what we are looking at and what we are looking for. In other words, we must have an analytic frame within which to sense assemble the social activities going on *as* the sociological phenomena we want to examine. Using Garfinkel's inclined plane experiment as its example, this discussion looks at some key aspects of constructing such a frame and implementing it within fieldwork investigations. We choose the inclined plane experiment because it wears its challenges and successes on its sleeve, something which makes it wonderful as an example.

Chapter Nine (Ch9) of *Ethnomethodology's Program* [Garfinkel 2002] provides the only extended account of a project which pre-occupied Garfinkel for over a decade in the 1980s and early 1990s and which, in the end, he concluded was not the success he had hoped for.[1] Even so, from the recently published cache of materials from Garfinkel's

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[1] In a recent paper on ethnography in Ethnomethodology, Anne Rawls and Mike Lynch [2022] suggest this might not be necessarily so. Following Rawls' own positioning of the report in her Introduction to it, they propose its purpose was to highlight the contingencies faced by pre-early modern scientists when mounting

nachlass, we now know that the project was to be one of the evidential centrepieces of a long-planned summary of Ethnomethodology's investigations of the natural sciences as "discovering" disciplines. This fact alone means the project is still of considerable interest. Unfortunately, the account given of it in Ch9 presents us with much the same array of problems the whole volume does. In his own introduction, Garfinkel talks of it as "proleptic", meaning it was very much an unfinished stand-in for the account he had intended to give. The Ch9 version was extracted from a compendium of notes, memoranda, letters and taped discussions and stitched together by Anne Rawls. It is neither a unified whole nor the completed account of what the inclined plane investigation was supposed to be about and what it achieved.

We are as much in awe of Garfinkel radical thinking and foundational contributions to Sociology as anyone. We understand and admire his relentless insistence on pushing his ideas to the limit. The inclined plane experiment is no different. Garfinkel takes his departure point from three sets of findings: Husserl's recovery of the foundations of modern science (what he called "Galilean Science") in the Natural Attitude of common sense; Phenomenology's display of the apodictic character of fields of consciousness grounded in sensory immersion in embodied action; and Ethnomethodology's discoveries concerning the detailed order of practical social activity. To these he adds his own aspiration to have ethnomethodological investigations yield outcomes which are of direct disciplinary/professional relevance to those engaged in the domains being studied. In the inclined plane experiment, his ambition was to demonstrate to contemporary novice and professional physical scientists how, as a practical social activity, Galilean Science is premised on commonsense interpretive methods. Had he achieved this objective, he certainly would have concretised the interdisciplinary value ethnomethodological sociology could bring a joint research endeavour.

What makes his proposal quite distinctive is that the exercise was not to be a philosophical/sociological armchair envisionment. Nor was it to be controlled historical re-enactment of what it must have been like for pre-modern scientists to come to see the material world through the lens of Galilean Science. Either would have enabled him to stipulate all

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of experiments and making them work. In this, the fact that Garfinkel "lost" the phenomenon demonstrates his success. As will become clear, while it is certainly true Garfinkel claimed to have lost his phenomenon, from what he says about the exercise it was not that which he thought was central to the project's success or failure.

the conditions he required to show how that transition could have happened and, by careful scene setting and script crafting, ensure those conditions were fulfilled. Instead, with the help of colleagues he set out to learn how to do Galilean physics by setting up and running his own version of Galileo's famous experiment. The thesis is that the collective experience of acquiring this learning would be equivalent to the epiphany–revelation– Galileo himself must have undergone when forming his new approach to scientific practice.[2] Having engineered that outcome, Garfinkel intended to reflect upon his experience, extract the general implications for understanding the foundations of Galilean Science and then convey those implications to a cohort of novice and professional physicists.

All this required a number of things to be accomplished. Garfinkel had to define what should count as the pre-Galilean and Galilean scientific outlooks. Adopting a 'misreading' of Galileo's own account, he had to design, build and run a version of the inclined plane experiment which could reveal 'the gap in the Galileo's text' and hence force attention on how a transition between the defined outlooks might be achieved. To acquire the Galilean outlook requires the production of scientific results of the general form Galileo reports. These need not necessarily be new results but certainly must be results (even if basic) which practising scientists would accept as properly scientific. Once these were in place, he would need to trace how they had been 'produced' during the deployment of the practical, embodied courses of social action which constituted the work of the experiment. Finally, from the whole set-up, he had to distil scientifically relevant (i.e., not philosophically or sociologically relevant) implications concerning the dependence of Galilean scientific practise on commonsense embodied reasoning and package them up for a scientific audience.

These are stiff objectives. The reading we offer of Ch9 looks at the character and challenges of just two tasks which would need to be completed successfully for the constraints to be satisfied; the selection of a perspicuous setting for the research and the delineation and accessing of the pre-Galilean scientific actor's point of view from which to sense assemble the experimental context. We have selected these two because they are key not just when mounting empirical studies as radical as Garfinkel's but for any

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2 The key point which everyone agrees was part of his planning, was the stripping out or bracketing of the professional and institutional overlay which has accreted on science's practice since the 17th Century.

ethnomethodological investigation. Our review traces some of the methodological decisions Garfinkel made in the implementation of the project.

Section 1. The Problem and its Set-up

Problem Statement

"Galilean" is the epithet Husserl attached to the "turn" which Natural Philosophy made during the late Renaissance and early modern period. From Ancient Greek and Medieval philosophy, Renaissance philosophy had inherited the problem of securing the relationship between the world-as-experienced and the world-as-it-is-in-itself. This was encapsulated in the perennial question: How can we know how the world is independent of our experience of it? In the 16**th** and 17**th** century, increasingly the answer to this question was felt to be in the application of a logical 'method' and the primary example of such method was Euclid's Geometry. Systematic reasoning about phenomena using methods akin to those of Geometry, it was thought, would reveal the structure of the natural world. Turning to the world in this way involved three preparatory steps: abstraction of phenomena from experience; idealisation of the features of abstracted phenomena; and formalisation of the relationships among idealised phenomena. Galileo's primary notation for this formalisation was Geometric and Algebraic. To his contemporaries as well as to later scholars, Galileo represented the epitome of this nascent approach, hence Husserl's attribution.

Husserl's interest in Galilean science is its philosophical significance as a breaking apart of the common-sense identification of experience and reality and the resulting inversion in their epistemic status. For Husserl, the Galilean method became what we would now call the "imaginary" [Taylor 2004] we still rely on to acquire truth about the natural world. In his *Crisis*, Husserl [1970] analysed at length the philosophical grounding of Galilean science in our common-sense 'natural attitude' to the 'Lifeworld' of daily life and what he took to be the unfortunate consequences it has had for our understanding of the world and our place in it. It was Aron Gurwitsch [1966 ; 2010] who identified the social scientific significance of Husserl's problematisation of Galilean science. This was the requirement for the scientific actor to hold the two distinct Gestalts of Galilean Science and the Natural Attitude simultaneously. In the theoretical praxis of Galilean science, these two alternative configurations have to be bound together. However, as distinct configurations of the world-for-experience, logically they could not be fused.

It is this binding which defines Garfinkel's problem. He articulates it by 'misreading' Gurwitsch as proposing when scientists undertake commonsense practical science, they face the task of continually integrating, synthesising or entangling the configuration of experience under the natural attitude with the configuration of experience under the Galilean attitude. Identifying and describing the 'shopfloor work' of learning how to perform this task is the problem Garfinkel set himself.

To show how this binding is carried out, Garfinkel sought a setting in which the uncovering of the possibility of a mathematisation of common-sense experience of the natural world could be brought clearly into view and with it the possibility of a binding of the two Gestalts demonstrated. This demonstration would be the trajectory of the epiphany to which we referred earlier. The problem in doing this is that modern science takes such mathematisation for granted. It has become institutionalised at the heart of its investigative processes. It is built into the very fabric of its operative experimental method. Rawls and Lynch are right about this. To reveal 'the work' of accomplishing the Galilean binding in contemporary science, Garfinkel would have had to find some way to frame and set aside (or "bracket") that assumption about mathematisation whilst performing that science itself. And to do that, he would have to learn the contemporary science being done. Alternatively, he could have found a setting in which the science was mathematically naïve and hence closer to pre-Galilean forms. If he could find such a setting, he ought to have been able to acquire such early-modern science relatively easily and subject his experience of doing to ethnomethodological investigation. As we sat, he chose the first option and took Galileo and his inclined plane experiment as suitable candidate model for a demonstration of the transition from pre- to Galilean science.

Registration

Garfinkel makes his problem tractable by rendering it under EM's master trope: treat social life as the sense assembly of instructed action. The device which this rendering relies on is carried by the familiar distinction between logic-in-use and reconstructed logic. Garfinkel casts this distinction as juxtaposing of two possible "accounts" of the work of science. The first is the work as a flow of sense assembled experience undertaken under the rubrics of Galilean science. The second is the work as a flow of sense assembled experience undertaken under the rubrics of the Natural Attitude. These are the two Gestalts. Garfinkel asks what is missing from the first ("the gap in the texts") which is required under the second for the work to be done? What, to use our preferred expression, has been "effaced" from the Galileo's text which has to be included in a pre-Galilean description for the latter to be a description of how to undertake a successful experiment in terms accepted by post-Galilean science? The gap in the texts and what it stands for *is* Garfinkel's phenomenon. It consists of the concrete details of the practical doings which are filtered out from the abstracted, idealised and formalised account of the science undertaken which was provided by Galileo. In setting up his investigation, Garfinkel proposes to counterpose the description of the experiment which Galileo provides with his own suitably shaped description of the embodied flow of practical decision making and contingency management through which he and his colleagues will have managed to carry out the inclined experiment for themselves.

There are three things going on here which we need to make sure we keep separate.

1. The distinction Garfinkel proposes between the text of the inclined plane experiment as the social object which Galileo and his peers oriented to and that text as the particular sociological object Garfinkel needs it to be for his investigation. We have some comments on this.
2. The distinction he needs to make between the social experience of running the inclined plane experiment under the Natural Attitude and the nature of the experiment as construed under Galilean science from which he can extract results he can communicate to his designated scientific audience. We will have very little to say about this.
3. The alignment and elision of features of experience under both Galilean science and the Natural Attitude as enabling a possible trajectory for the epiphany required via the achievement of a binding of these Gestalts. If he can demonstrate the possibility of the transition, Garfinkel will have shown how a view which registers the world in mathematical terms could be acquired. This is what will take up most of our attention.

Notation

The gap in the texts provides Garfinkel with an analytic space but what precisely does he want to focus his analysis on? Here he turns to two characteristics of Galilean science which he believes have general consent. First is its aim to be probative. While this feature might exhibit entail Mao-like progress, overall progression can be and is marked by the cumulative sum of propositions whose evidential status is taken to be closed. Second, it can lose its phenomenon.[3] This latter is particularly important for Garfinkel because it offers a clear contrast with Sociology. Sociologists *never* lose their phenomenon. Or, perhaps more accurately, Sociologists can't lose their phenomenon. What he means by "losing the phenomenon" is described at length in the materials released from his *nachlass.* He takes it to be an endemic feature of practical science that a set-up, experiment or simulation can capture a desired phenomenon in one run and, on the next, completely fail under what appear to be identical circumstances.[4] Ensuring the continued presence of the phenomenon across initial findings and their replication is what scientific method is designed to do. It is how the probative character of Galilean science is secured. What is done to secure and preserve the phenomenon in this way is the element of the gap in the texts which Garfinkel homes in on.

Since the activity effaced by the gap in the text is essential to the doing of the science, there must be a way of making it describable in scientific terms. And yet, though it is known by scientists under the scientific Gestalt, the gap can only be made visible by sociological investigation of its social character. What is known is that which is routinely necessary for the science's practice, but which is also taken for granted in that practice and in its accounts of that practice's performance. This taking for granted is what makes it invisible.[5] Remember, the reason Garfinkel undertook this study was not just to make the binding of the Gestalts *sociologically analysable*. He also wanted to make these details available to physicists and hence *scientifically analysable* as relevant findings for them. If the details are necessary for preserving the phenomenon and hence securing probativeness, they *must* be scientifically relevant. But, of course, sociological description of practising science is not scientific description of practising science. To make the findings available to

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[3] Garfinkel talks a lot about this and regales us with stories which he heard from his wife, his scientific friends and others. However, it is not clear just how common the loss of phenomenon is nor whether it should really be taken as one of the characterising features of scientific practise. After all, most cultures have their popularly recounted "myths" which function in all sorts of beneficial ways to reinforce normative order. The same could well be true for science and the talk of 'loss of phenomenon'.

[4] One version of this contingency which gives the term surface plausibility is known to every R&D manager. It is the regular way a 'canned demo' of some technology only seems to work reliably when the scientist who put it together is within 20 feet of it. They don't have to do anything. They don't have to be leading the presentation. But if they are 'away', you can bet the demo won't work.

[5] Garfinkel's *Studies* [Garfinkel 1967] is replete with examples of how and why this is so.

scientists in ways that they can see as relevant means finding a form of description which straddles both worlds. It is, to use the phrase he adopts, a description made "inside-with" the science.[6] It will be a hybrid; a science-EM hybrid.

To bring out the distinctiveness of what he is aiming for, in the version of the summary introduction to his planned series of studies of EM and the sciences contained in the recently released cache of materials, Garfinkel runs an extended contrast between what he terms 'analytic ethnography' and his proposed science-EM hybrid. The first is exemplified by Lynch's work on Micro-biology; the second by Livingston's work on Mathematics. What marks them apart is the seriousness* with which their findings can be treated by the scientists they study. This difference arises because Lynch is not competent in the Micro-biology and Livingston is competent in Mathematics. Lynch gives an "outside-in" description and Livingston gives an "inside-with" one. What Garfinkel asserts Lynch cannot provide is the interior configuration of the phenomenal field of the practical science of Micro-biology as a construal of the social-actor-as-scientist's point of view. How that point of view is constituted and what it imports for the early-modern researcher is what Garfinkel himself hopes to discover for himself. This is the field of consciousness achieved through embodied immersion in the phenomenal field of the shopfloor pre-Gailean scientific organisation and management of the concrete contingencies of scientific method as evidenced in the preservation of the phenomenon.

As we have already noted, the implication of this analytic/hybrid distinction for Garfinkel is that if he is to provide a depiction of scientific practice on the basis of his approach, he will have to learn the science relevant to the work he wishes to examine. This gives him the last component of his research design. He must learn the science he needs by carrying out his scientific experiment under the conditions described above whilst *at the same time* undertaking an ethnomethodological investigation of that experiment. Rendered through his transformations under the ethnomethodological attitude, he will find what has fallen through the gap in the texts. It is that which will form the basis of his findings for science.

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[6] This phrase is explained as follows:

> "Inside-with" is a phrase that Lois Meyer coined in her Ph.D. dissertation. The use of "inside-with" by EM authors should be used to criticise Merleau-Ponty's "intertwining" and "chiasm" as well as recent variants on these metaphors. [Garfinkel, 2002, p.271, fn.12.]

Section 2. Problem Specification

As already noted, the setting Garfinkel chooses to investigate is the inclined plane demonstration of the Law of Falling Bodies as described by Galileo. Here are the reasons he gives for this choice.

> We figured, we'll go to the ancient accounts, including Galileo's account of his science, and pick it up at a time when he wasn't answerable to the professional association of physicists. He had something like a crowd at court. When he would go to court what they wanted to know from him was, "What's new?" And if he could tell them what's new, then what they wanted to know in what's new was "What could we do with these balls?" What are you telling us? To roll them down this plank? Okay, that sounds like it's just right.
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> So, we figured, if that's what physics could have been at that time it's good enough for us. We went for that.
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> The idea was to specify what discovering work in the work site looked like when there was a serious science you were doing, but no professional association or accepted literature to which the work was accountable. [Garfinkel 2002, p. 267]

Now, we don't for a moment think Garfinkel approached the specification of his project in as off-hand a way as he makes out. But the above does point to two important features we should explore. The first is the context in which "Galileo's account of his science" was originally given, which was not, of course, anything like the scenario Garfinkel offers us in his text as well as the endogenous character that account has in the context it is given in. The second is the accountability as science for Galileo (and his peers) of the work reported; what were the expectations Galileo oriented to and hence had to satisfy in order to show his work could be taken seriously? Just as important, of course, is what he wanted his peers to see his contribution as being. What sense assembly of his work was he seeking? While Galileo may not have been a member of a modern professional body, he wasn't Robinson Crusoe the physicist either.

THE DISCORSI ACCOUNT

Ch9 quotes the whole of Galileo's description of his demonstration. However, no context is provided. The only piece of information about the text's character (what it was for and how it might have come about) is the highly unlikely suggestion made in the quotation above about the occasion on which the description might been given. Galileo's account is to be 'misread' as instructed action but without any attention being given as to what kind of a document it was and how it 'worked' in the settings in which it was read. What is the relationship of the description to the experiment it describes? Without considering its context-of-use, we have no way to tell if the account-as-social-text could reasonably be expected to bear the weight of being deployed sociologically under the rubric of instructed action.[7] As we work through the case Ch9 presents, this problem will return again and again.

What do we mean by 'context' here? Galileo describes his experiment in an aside which occurs in the middle of a mathematical treatise. It is presented as an interruption in the flow of a conversation. But, of course, the conversation is Galileo's rhetorical artifice—a standard narrative construction used in the Natural Philosophy of the time. These narratives consisted of extended monologues of declarations and responses containing formal arguments and their proofs. In the *Discorsi*, there are three interlocutors: Salviati (Galileo the scientist), Salgredo, and Simplicio (interested and well-informed citizens). The text quoted from the *Discorsi* is not extracted from nor a rendition of Galileo's own laboratory journal (and not taken from any of the relevant *folios* which contain his journal entries either). Neither does it appear in any similar memorandum. It exists only in this one place, a published formal presentation of *mathematical* results. In the treatise, Galileo does describe concrete, substantive (and hence empirically investigable) objects in mathematical terms. He also provides descriptions using terms referencing physical objects, their motions and relationships in term of idealised mathematical objects. It is not immediately clear the description of the demonstration is not yet another idealisation.

Throughout the treatise, Galileo introduces his theorems as the preliminary results of his investigations. Whatever his inductive/abductive method is, from the way the dialogues generally go, we can presume the point of describing the experiments is to confirm the formal proofs given. They are demonstrations of the empirical adequacy of those proofs. In other words, they are not the 'data' from which the findings (proofs) are derived. Galileo

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[7] Some say that was the point. By treating it in the way he does, Garfinkel ensures the "gap in the text" will have to be confronted. The gap is an axiom about the text. But that justification entirely misses the key issue of the whole design. This is not any old exercise in demonstrating the Law of Free Fall. There are lots of ways to demonstrate the Law which do not require reproducing a version of how Galileo says he demonstrated it. The question is how can Garfinkel assure himself (and us, the readers) that the ways in which he fills out the missing detail does support and sustain his exercise as a reasonable version of what Galileo would have had to have done? That, presumably, is what he wanted to do rather than just replicate either a demonstration of the gap or a demonstration of the Law. If it wasn't, why bother with Galileo in the first place? There are plenty of other places you can find mathematically naïve science going on.

proceeds by stating axioms, deriving theorems and proving them. When he describes any of the experiments he has undertaken, it is to show results which match (or seem close enough) to those 'predicted' by the proofs.

The theorem in this case states the distance travelled by a body falling under constant acceleration is directly proportional to elapsed time ($d \propto t$). Having proved this theorem, Galileo develops a corollary. Under constant acceleration, $d = t^2$ He then proves this using a combination of geometric and algebraic methods. His proof implies if he can consistently collect measures for d and t and they show d is equal to $t^{\mathbf{2}}$, he can claim to have demonstrated the acceleration of a falling body is constant. At the end of the proof (p.178), Simplicio asks for evidence that 'nature' (the motion under gravity is called 'natural') really does conform to the 'law' Galileo has proved.

In response, Galileo tells a story. A board 33 feet x 9 inches x 3 inches (modern conversions of cubits and finger widths) was sourced. A channel 1 inch wide was grooved on its edge and lined with parchment. At one end, the board was lifted by 3 feet. Repeated runs of rolling a bronze ball down the slope were undertaken and the elapsed time measured in pulse beats. Runs which varied by more than 1/10 of a pulse beat were rejected. Runs with the board fixed at different inclinations were included. The full, ¾, ⅔ and ½ lengths were used. Time of descent was measured by synchronising the run of the ball with the collection of water in a glass. Ratios of the weights of the water were used to measure the ratios of elapsed times. This set up allowed the proofs to be confirmed. The phenomenon in Galileo's account is an isomorphism of the Law deduced from the mathematics and the empirical results obtained by rolling balls down a slope. What he has demonstrated is the proportionality of distance to time for a body in free fall (given the absence of any relevant dynamic forces other than gravity, but he doesn't say that. His story has already told you how the most important of such forces, friction, was effaced).

Garfinkel calls Galileo's story a 'careful*' description of the demonstration's 'details*'. Careful* description of the details* of social action is one of the desiderata for ethnomethodological accounts. Elsewhere in *Program*, he defines what these terms mean. Details* means a description of an action exhibits the "immediacies and certainty of its identifying orderliness" (p. 273) and careful* means "so written as to lend itself to reading alternately as instructed actions" (p. 264 note 1). To invoke a phrase Garfinkel himself used elsewhere, these are curious terms to apply to Galileo's description if only because it lacks almost all of the detail of how the setup was built and used.

The most obvious thing missing is a set of results. In other descriptions, Galileo does give his measurements and their construction. Not here. Then there are the detailed dimensions of the board and groove which are given and but not the kind of wood from which it is made.[8] Since the dimensions are crucial parameters for the demonstration, what are Galileo's reasons for choosing them? In fact, did he choose them? What difference would it make, for example, if the board was not 22 cubits by ½ cubit by 3 finger widths? Or do these numerals, like Ali Baba's 40 thieves and Goldilocks' 3 wishes, really function as culturally given 'magic' numbers? What about the 2 cubits lift? Is that special? Why not 1 or 3 cubits? Are these features ways of managing unknown "demonically wild contingencies" which Galileo was forced to use or just any old set of arrangements which he threw into the story? If, as Rawls and Lynch suggest, Garfinkel wants to know how any of this might matter, would he not call these issues out? And if he doesn't comment when this detail is missing then what is all the fuss over the criticality of the configuration of details* about? Certainly, compared to Garfinkel's own list of things he says he was planning to include in his own description, Galileo's presentation of his own cupboard of contingencies seems pretty bare.

If we step back for a moment, though, and think about the text and the experiment's place in it, one thing becomes obvious. While a lot of the detail about how the experiment was designed and run is missing, one central thing from the point of view of his new science is very visible, the place of indirect measurement. This is important. Pre-Galilean scientists were familiar with cross-tabulations of tallies and direct measurement of natural properties (counts of pulse beats and length of board runs, say, in this case). These we can call "naturalistic noticings". What was crucial in the shift to the Galilean point of view was the realisation that the relationships among these noticings could be expressed arithmetically.[9] That is, some observed and measured data could be expressed as arithmetic functions (sums, products, ratios or powers) of others. Measurement, then, is not only direct observation. Galileo's Law involves direct measures of distance and time being used to generate an

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8 Anyone who works with wood knows different woods have different surface properties. Some (e.g., Ash and Elm) can be planed very smooth. Others (e.g., Cedar) remain fluffy no matter what you do. So, surface properties matter which might be the reason parchment was required. Now, you might say his interlocuters know all this. May be so. But then what is being 'omitted' from Galileo's presentation is just the same 'taken for granted background knowledge' Garfinkel suggests required to make texts in Sociology and Physics careful descriptions. The description Galileo gives is both careful* and not careful*

9 We should remember that although Galileo is credited with this 'revolution', he did not achieve it all on his own. Innovations in double entry book keeping, for example, were going on in Italy at much the same time. These led to the creation of all sorts of new 'derived accountants' objects' such as measures of cash-flow.

indirect measure of the rate of change in velocity (i.e., acceleration). This is done by the application of a mathematical function (the squaring of time) to those direct measures. The length of the board and the time of descent have a *functional* relationship to the ball's rate of change in velocity. Under that function, the two numbers deliver a value for acceleration. If the length of the board is varied, does the rate of change in that value vary? If it doesn't, then the ball travels under constant acceleration. That falling bodies travel at constant acceleration is Galileo's Law. The whole experiment is envisaged as a device to produce indirectly measured values for acceleration by generating the direct measures as input for the function. It is a demonstration machine not a discovery machine.

What has to be untangled in the specification of Garfinkel's problem, then, are three not two steps. The first is commonsense experience of informal measuring (this ball rolls faster than that ball). The second is the experience of scaled measuring (this ball takes 5 pulse beats. That ball takes 6 pulse beats). The third is the combination of direct measures to provide indirect measures of the kind which Salviati reports. The first and second tasks are Pre-Galilean science. The third is Galilean. The point of the Galilean experiment is to provide a setup which, under controlled manipulation, demonstrates the functional relationships of the proof actually do hold in nature. As Galileo put it, his experiments are designed to show the laws of nature are written in the language of mathematics. In his version of the experiment, Garfinkel has to discover for himself how to turn commonsense experience of perceptual differences into directly scaled measures and then into indirect measures by demonstrating their arithmetical relationship to some properties which can't be directly measured. Learning first that this last step can be successfully done and then how to do it systematically is the epiphany.[10]

Given all this, it is pretty clear Salviati can't be used as the resource for constituting the social-actor-as-pre-Galilean-scientist. The outlook he presents in the text is not pre-Galilean and the conduct of the experiment as described in the text is not pre-Galilean either. As we

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[10] In the paper we quoted at the beginning of this discussion, Livingston brings out this very point but with regard to another of Galileo's experiments, that of the pendulum. When undertaking his own studies of the pendulum, Livingston had to learn how to manipulate physical features of the experiment in a controlled fashion in order to produce direct measures, that is observable, scalable measures of actions of one part of the set-up which in some mathematised combination could stand for actions of another part. In so doing, he moved across the pre-Galilean/Galilean divide.

have just explained, the key to Galilean science is the combination of methods applied to direct *and* indirect measurement. It is Salviati's proofs which uncover the functional relationship. The experiment demonstrates it. This does not mean the inclined plane set up could not be re-designed and used to discover that measured relationships might be possible. It is simply that Salviati does not describe setting it up and using it that way. For Salviati, it is a given that relationships among objects in the world can be described in terms of mathematical functions. The pre-Galilean scientific outlook consists in looking at the world in terms of scaled measures of directly observable variables. In contrast, the Galilean scientific outlook consists in looking at the world for functional (usually arithmetical) relationships between scaled measures which can stand for indirect measures of other variables. Galileo didn't discover this possibility by running experiments. He discovered it by reflecting relationships he observed in the world around him, undertaking formal proofs of theorems he derived for those relationships and then proving them. His experiments were to demonstrate the "reality" of those relationships in nature. The demonstrations he describes in the *Discorsi* are exercises in Galilean science not a discovery process for the possibility of Galilean science.

This might sound like nit-picking, but it isn't. It is about the construal of the social actor as an object for investigation. What do we attribute to them as their point of view? The Salviati-in-the-text can't be used as the resource for Garfinkel's pre-Galilean social-actor-as-scientist because he arrives at the experiment and its properties (the measures) via the proofs. He is already Galilean. What Garfinkel needs for his version of the pre-Galilean attitude is a social actor who can discover indirect measuring through the experiment. And that is not Salviati. Or rather, it might be Salviati if he did not already have the mathematical view on the world he clearly has. To constitute his scientist-as-sociological-object, Garfinkel would have to set aside the mathematical knowledge and expertise Salviati has acquired and displays regarding functional relationships. It is because it is designed to enable those relationships to be exhibited which makes the experiment Galean science. What Garfinkel and his colleagues have to do to achieve the objective they have set themselves is to turn a set of noticings of their own into direct measures and then derive an arithmetical function for those measures which transforms them into indirect measures of some relevant phenomenon. Simply repeating what Galileo/Saviati claims he did won't do it, because that whole experiment (the setup and the results) is predicated on the certainty such relationships can be demonstrated.

Saving the Theory

There may well be a reason for the lack of detail in Galileo's description. As we have already noted, the story Galileo gives is nowhere to be found in his notebooks. There are other setups with measurements which approximate to the Law, but they are not used. The consensus among the numerous attempts to reproduce his experiment is that Galileo could not possibly demonstrate the Law because of the practical circumstances of his own experimental conditions. Naylor (1974), for example, points to the following:

a. Although the groove is lined with a smoothing surface (parchment), replications of the experiment show this probably would have added rather than reduced significant friction. Galileo was well aware of the effects of friction as well as an acute observer of his experiments, so he is unlikely not to have noticed the difference the parchment would have made.
b. Inevitably, the bronze balls would have caused increasing wear and tear on the parchment surface, especially if the experiment was run 100 times (the number Galileo claims). This wear would create lateral (rocking) momentum and small ridges, both of which would slow the ball's descent by more than the 1/10 pulse beat rule. The need to discount so much data would surely have forced Galileo to re-design the set-up.
c. It is impossible to achieve the required accuracy of timing (pulse counts to a tolerance of 1/10 of a second approximately) without modern stop watches.
d. Consistent synchronisation of ball release/arrival and water collection opening/shut off to the tolerances required has not been reproducible. This implies (random) measurement error in the results for distance and time were inevitable.

Because of a – d, the mostly likely scenario is that Galileo opted to accept his 'ideal' mathematical proofs in the face of an inability to get better than overly loose confirmations of the proof's predictions. In other words, the story he tells 'saves' the theory (the proofs of the relationship $d \propto t$ and $d = t^2$) in which he was interested. Of course, even though this has been widely commented on in research on method in Physics, it hasn't stopped descriptions of the inclined plane experiment featuring prominently in introductory texts in Classical Mechanics. But there are very good pedagogical reasons for that. The Law is a crucial stepping stone in the development of Classical Mechanics and its eventual formulation by

Newton. If you want to understand what Newton achieves, it is essential to know what Galileo proved. This is notwithstanding the difficulty of demonstrating it.

So, a not unreasonable way of viewing Galileo's experiment is as a convenient fiction inserted into the *Discorsi* as a way of saving his theory. This is important but not for the obvious reason. It does not imply there is no point is trying to make Galileo's experiment or some version of it work. Generations of seasoned and novice physicists have tried and continue to try to do just that. It is rather that the *Discorsi* does not provide a resource for descrying and accessing an occasion when the move from a pre-Galilean, naïve mathematical scientific Gestalt, to a Galilean one took place. Everything in it, and this is especially true of the inclined plane experiment, pre-supposes the mathematisation of nature is not just possible but can be done and so is required. It is the mathematisation which makes the rolling of the balls, the counting of the pulses and the collecting of the water Galilean scientific practices.[11] The whole text is not just about finding the possibility of the mathematisation of nature but the certainty of the results if that is done. In other words, the experiment described in the *Discorsi* is not a perspicuous setting for revealing the transition to Galilean science and the idealisation of Salviati is not a good resource for characterising a pre-Galilean-scientist-as-social-actor who undergoes the transition from pre-Galilean to Galilean scientific points of view.

Clearly, this has implications for Garfinkel's planned exposition of the gap in the texts and for his findings as representations of how to bind the two Gestalts. He cannot just take the text "as is". Instead, if he wants to use the text as he proposes, he has to interrogate that text for its methods of mathematisation and for the practical skills Galileo could call upon as well as the resources he must have had access to. Having circumscribed their relevance, scope and application, Garfinkel could then try to find ways of stripping these aspects out from his own exercise. Doing that would amount to an *epoché* of the Galilean scientific attitude. His choice of text has taken him back to where he started; the need to discount the mathematisation in the science. What the *Discorsi* is about is demonstrating the efficacy of what were for the times sophisticated algorithms providing measurements of natural phenomena and not just the application of numerical procedures to generate numbers

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[11] In that sense, the identification of the practicalities of the 'measuring' provides the answer to the application of Shills' question to the inclined plane experiment. See [Garfinkel et al. 1981]. It is what makes it science and not, for example, play, a pastime or some other small group activity going on using the set up.

representing physical events. In sum, his reliance on the *Discorsi* is likely to complicate the counterposing strategy Garfinkel was pursuing.

Section 3. Analytic Protocols

The Experiment

The premises for the inclined plane exercise were:

1. The lived work of doing the proving of the Law and demonstrating its truth status in the experiment was structured to prevent the contingent loss of the phenomenon which the Law describes.

2. As with all scientific reports, Galileo's description of what was done and how it was done effaces almost all of this work.

Because he had never been a scientist nor conducted an experiment as a practising scientist would, Garfinkel feels he is unable to 'read between the lines' and fill in this omission. He doesn't know what it means for a science/scientist to find and lose a phenomenon. As he says, he can ask the scientists but since he doesn't know the science, he cannot understand what their answers mean in the ways the scientists do. No matter what they told him "We could not make their affairs teachable *to them* by what they were teaching us".

In undertaking the experiment, Garfinkel wanted to reveal the "measured and measurable" lived work of doing the proof and its demonstration in *all* the detail required to ensure the preservation of the Law of bodies in free fall.[12] This lived work is his phenomenon. The preliminary list of what will need to be recorded on p. 268 is, of course, just a list and necessarily contains only some of the detail. It might seem 'obvious' much of what is involved in mounting and carrying out his experiment cannot be materially relevant for the success of the exercise as he has defined success. The only trouble is Garfinkel is fond of quoting the story about James Olds' reaction to his lab assistant's cleaning and tidying the Lab shelves. Who knows in advance of any scientific experiment what will turn out to be critically material to it? In fact, that very question is the lodestone for what he calls h-sociology*. It is the *raison d'être* for studies like the one we are discussing. We are given lots and lots of details whose properties are to be examined but not the basis for selecting them nor the enumeration of

[12] Except, of course, only the physical demonstration is covered in Garfinkel's description.

which of the features/properties of those details will be picked out. Both Garfinkel and we know he could not list all the relevant detail in advance nor summarise in the eventual report all that did eventually turn out to be important. The question is what were his selection criteria and how did he operate his stopping rule? Answers to these questions indicate how he set the boundary conditions on his version of the experiment.[13]

PRE-GALILEAN SCIENCE AND ITS PHENOMENAL FIELDS

The heading for the section we now have in view is "What Did We Do?" and it opens with the following:

> An Ethnomethodological study of Galileo's inclined experiment describes the demonstration's lived work in phenomenal details. These details exhibit the demonstration's engineered design as a coherent domain of empirical phenomena of social order in physics: The law of free falling bodies. [Garfinkel, 2002, p. 273]

This is pretty cogent. What Garfinkel has his eyes on are all the phenomenal details of the experiment's lived work *modulo* the unavoidable lived differences which arise from the different circumstances in which the original and Garfinkel's version were carried out. Naturally, neither we nor he can know how much difference these unavoidable differences might turn out to make for the comparability of the two exercises. Will Garfinkel's version work? Will the results confirm the proportionality of time to distance enough times to count as a demonstration? Then again, the Law and its corollary ($d \propto t$ and $d = t^2$) are mathematical objects. The giving of proofs using the arithmetic of Geometry can certainly be treated as a social practice and it is a perfectly proper sociological axiom to insist their practical demonstration also constitutes the construction of a social order. Although one can imagine descriptions of the lived detail of proving the laws featuring in a description of a version of Galileo's demonstration, the performativity of that version does not affect their fundamental epistemological status *for Galileo* as empirical certainties.

What Garfinkel does go on to provide are informal summaries of baskets of difficulties, challenges, troubles, requirements, necessities and redirections he and his colleagues encountered when setting the demonstration up and then running it. As an

[13] Notice we do not say "how well Garfinkel's set up maps onto that of Galileo". It remains to be seen if Garfinkel's experiment has any kind of mapping relationship to that of a pre-Galilean scientist we might be able to envisage and the work such a scientist would have to have carried out.

ethnographic account, though, much of the key detail is missing. Take the grooving. This was first to be done using a DIY router, but that idea was quickly given up. Why was that and did those reasons matter? Did opting for wall moulding glued onto the board change the phenomenal character of the experiment? Did the very different physical properties of the solidified, sanded epoxy resin forming the channelling make an important/minor difference? Since the experiment had not already been run, they could not know the answer to such questions, so on what basis was this decision made? What alternatives together with their advantages/disadvantages did they consider? How did they check and calibrate their suppositions?

In assembling descriptions of relevant phenomenal field properties, Ethnomethodology borrows a simple sorting device from other forms of fieldwork ethnography. It asks: "Why that there?" The answers provide grounds on which to array the identified properties. Here are a few examples where Garfinkel might have asked this question but, from the description given, appears not to have done.

1. Setting up the audio and video. How were video angles selected? How were cameras positioned to allow consistent marking of transitions down the board? Did the requirements for these choices affect the design of the core set up? If so, how was that design adjusted? What precisely was the purpose of the audio? Was it for ancillary commentary while the experiment was in flight? Or were there expectations concerning intrinsic critical 'experimental auditory details' which meant they should be collected? If so, what were they, how were they identified and how were they to be used?
2. We are told lots of different metaphors, images, formulations and modes of description were tried out by the team as useful ways of describing for each other what was happening during the building and running of the experiment. Which of these survived and why? Even more important, how did they feature within the courses of action in which they were used? What was their 'performativity'?
3. At one point, the basic distinction between a set of experimental protocols and its actual implementation is pointed to. Given how key measurement and its transformations is for the binding of the Gestalts, it is odd there is no description of how that distinction emerged in the experiment, nor how it was understood, worked through and then assimilated and effaced.

4. Reference is made to lots of learning and lots of practice requiring lots of repetition. Actors, sports people and practitioners of all kinds know the importance of training 'muscle memory' to allow focused attention on the physical particulars of performing a course of embodied action to be dispensed with. But just how was the training of muscle memory done in the experiment?[14] As part of this description, reference to made to the Bergsonian distinction between experienced inner time consciousness with its intrinsic fluidity and outer 'marked', metronomic clock time. Again, we are given no detail of how this distinction was deployed and hence no 'inside-with' description of its contextual character.

The most important of these under-described bundles of detail is the "loss of the phenomenon". We learn "the board absorbed moisture during a night of heavy rain, So, *we had lost our phenomenon*." [p. 276]. But what exactly constituted 'losing the phenomenon' here? Which phenomenon had been lost and how did they know? From the discussion, we can assume it was the ability to demonstrate $d = t^2$, but this was Galileo's phenomenon. The phenomenon for Garfinkel's version is the lived-work details of carrying out the experiment as part of an epiphany. So how did the warping affect the ability to demonstrate that? [15] And what was it exactly about the board caused it or might have caused it.[16] On this key feature of the lived work and its scientific implications, the detail is missing.

In the end, while the account contains an extensive list of things, many of what on the surface appear to be critical elements are not described in the detail seemingly required under the rubrics for hybrid EM-science* Garfinkel sets out in *Ethnomethodological Policies and Methods* (Ch5 of *Program*).

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[14] There is reference in the Lynch release to Todes' (2001) reflections on this issue. They are not picked up in this report where they would seem to be very germane.

[15] What was it about the condition of the (wet) board which prevented them being able *now* to see d would not continue to be equal to t^2?

[16] Remember the central feature of the Law is its universality. On the theory, the Law remains true no matter what circumstances it is tested under. It is just you can't demonstrate it. What trials did they make? What remedies did they apply? What determined the stopping point on remediation? That it might seem obvious a warped board is no use *cannot* count as an h-sociological* conclusion. It is not an "inside-with" description.

Section 4. Analytical Results

THE SHOP FLOOR WORK OF PRE-GALILEAN SCIENCE

As with the previous section, this section of CH9 begins by summarizing what it will reveal. This the carefully* described domain of the Law of falling bodies. The term domain refers to workplace-specific-this-worldly-work, in this case of uncovering the Law of falling bodies. This introduction seems clearly to couch the phenomenon as the co-relation of the two orders of description, Ethnomethodology's description of the lived work and Physics' description of the demonstration. It is followed by a list of topics. Eventually, we get this:

> We came upon the *phenomenal field of Galileo's experiment with the inclined plane in detail given everything that detail could possibly be*. That's a big mouthful. We found we were getting beautiful, beautiful things with an inclined twenty-two foot board, with billiard balls, and putting those balls on a track of finely sanded wall moulding, and hearing-watching them roll, to the point of their rolling in the watched-hearable ways they did. So we were beginning to specify the phenomenal field of the experiment—and now you can sense the trouble. [p. 278]

The one thing not mentioned is the forging of a collection of measured results concerning the proportionality of distance to time or some other feature of the experiment, which is what, from the acquisition of the Galilean point of view, the experiment was about. Before they "lost" their phenomenon, did they confirm Galileo's Law or some other similar relationship? If they did, that would certainly close any potential gap between some envisaged piece of pre-Galilean scientific work and the that which Garfinkel undertook. It would also lend some credence to the claim the "big, beautiful things" they were getting bear some relationship to the shifts in phenomenal field a pre-Galilean scientist might experience. It might even lend some validity to the claim that going through the experience of undertaking the experiment had resulted in the required epiphany and that the ethnomethodological account of the experiment was a description of that transition.

The trouble Garfinkel says they ran into can be put in a single sentence. The description they could give of what they had done was of no interest to the professional and novice physicists to whom they tried to give it. The team had run up against the Gestalt divide which had defined their problem and they had no way get through or over it. Although Garfinkel does not say so, it seems safe to surmise that they did not do what Livingston had learned to do. They had not developed a method for generating properties of the experiment's phenomenology (their embodied experience of immersion in it as a field of consciousness) which could then be mathematised as measures of the required physical properties of the runs of billiard balls. If they had, they might well be talking hybrid EM/Physics. Instead, what they could describe was the ethnomethodologically construed embodied practices of building and running an experiment. But the physicists had no interest in that. There was no learning for physicists in it. All the interesting work, all the innovation that has been secured was on the ethnomethodological side of the EM/Physics constructed experiment.

Garfinkel did "come upon" a phenomenal field and he certainly "found" some phenomena. But they didn't constitute the experience of coming to see the world from the Galilean scientific point of view. He worked hard to create/produce the configuration of an experimental experience and keep producing it. The problem was the things he and the team had found were either invisible for Physics, ineffable in Physics or of no interest to Physics. What they had learned the physicists didn't want to learn or could see no point in learning. No transition had been made and no gap closing achieved. Since Garfinkel says teaching the physicists what the team had learned about in doing the physics they did and its scientific significance was the express purpose of the experiment, the experiment hadn't achieved its main objective. In addition, from the report we don't know if it achieved its secondary purpose, the revealing of the shopfloor lived work of learning to demonstrate the Law of falling bodies or some other putative law. We do know, the team was convinced they lost their phenomenon. But the report doesn't tell us what *ethnomethodologically* this amounted to. Failures in science and elsewhere are not uncommon. They are certainly not necessarily disasters. If responded to appropriately, can be of great value.

OUTCOMES OF THE EXPERIMENT

The objective of the inclined plane experiment was to develop a hybrid EM/Physics account of that experiment's lived work. Validation that the objective had been achieved would be found in the seriousness* with which the audience of physicists to whom the results were to be disseminated took those results. In taking them seriously, they would confirm the results were of significance for the conduct of the Physics being done and not simply a sociologically interesting rendering of what doing the experiment entailed. Garfinkel is frank about the outcome. They did not achieve that validation.

To what was the failure attributed? Here are the options offered.

- It was part and parcel of the character of this kind of investigation and of doing this kind of work. It forms one of the normal natural troubles of hybrid ethnomethodological investigations.
- It was Garfinkel and his colleagues' poor communication and/or pedagogic skills.
- They started in the wrong place with the wrong topic. The one they chose was always likely to be recalcitrant. But if this was so, which would be better ones to start with?

As we have suggested on several occasions, we think the third option is the most likely. The problem set-up and its registration made it unlikely the experiment could be successful. There were three elements to this. The text chosen, i.e., Galileo's account of the Law and its demonstration, resists being treated as instructed action of the kind Garfinkel wanted. The description chosen did not readily fit the role of representing an idealised trajectory from a pre-Galilean to a Galilean scientific outlook. Neither does it display the counterposing of viewpoints necessary for that transition. Developing the proofs and then running the demonstrations would have required sophisticated (for the time) mathematical and physics knowledge as well as significant understanding and skills regarding the performance of the physics which was being developed. In short: seeking to discover the proofs and arranging their demonstration would only make sense to and be possible for someone who was already a reasonably proficient practicing Galilean scientist. Third, Garfinkel and his colleagues found no way to render their ethnomethodological descriptions of the shop floor work of the experiment into scientifically relevant and scientifically shaped measured findings. In other words, they had found no way to render their findings as in ways which would align with how Physics might describe the experiment.

One way to summarise the problem which Garfinkel started with is to suggest the translation of common-sense experience into mathematical formulations ruptures the structures of the initial experience. At the end of their experiment, he and his colleagues were unable to find an "inside-with" form of description which repaired that rupture sufficiently for the scientists who were their audience and partners to be able to use it to translate their Galilean viewpoint back into the phenomenology Garfinkel had described to them. Garfinkel and his colleagues had not been able to traverse the hoped for epiphanic transition. As a consequence, but going in the other direction, neither could the physicists.

Section 5. Conclusion

The inclined plane experiment was not the only investigation of Galilean science which Garfinkel undertook. He made some initial studies of introductory Chemistry with David Sudow and there is the famous Pulsar paper. In their own ways, neither was entirely successful. Nor is it the only ethnomethodological study of Galileo’s work. Dusan Bjelic and Eric Livingston too have looked at Galileo’s experiments [Livingston 2008; Bjelic 2023]. What is interestingly different about their studies is that the deployment of the physics being done is front and centre in the exercises they carried out. Both show they understand the nature of the physics being undertaken in the experiments they are concerned with. Garfinkel underscored the importance of acquiring relevant domain knowledge when setting up his problem. In the end, the centrality of the practicalities of that knowledge is missing from his description and it is that which lies behind the failure of the experiment. Perhaps the lesson we should draw is that the shop floor work of experimental science relies as much on what constitutes the ‘haecceities’ of the relevant science as it does on those of the common sense Natural Attitude.

All of which takes us back to the importance of “Being There” and the problems of selecting a perspicuous setting and defining the actor’s point of view. Neither can be decided in advance of close observation of the domain to be investigated and the activities which go on there. Whether the research site is a factory, an office, a school or even one’s own study, it is a mistake to presume you know in advance what it must be like as an environment of objects for sociological investigation. The first step in any investigation should be reconnaissance and the acquisition of enough local knowledge for an initial projection of its sociological construal as co-ordinated courses of action. From that construal, decisions can be made about its suitability as a perspicuous setting for the phenomenon one is interested in and the constitution of the social actors who carry out the courses of action under the sociological description to be given. As Garfinkel’s study shows us, without “being there” and present to the action taking place as the endogenously produced courses of action they are, there is very little chance of providing any kind of ethnomethodological description, let alone the inside-with descriptions required by hybrid/EM.

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