IMAGINATION AS METHOD IN

THE PHILOSOPHICAL INVESTIGATIONS

IMAGINATION AS METHOD: SECTION xii, PART II

OF

WITTGENSTEIN'S PHILOSOPHICAL INVESTIGATIONS

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TABLE OF CONTENTS

INTRODUCTION

In section xii, Part II of the <u>Philosophical Investigations</u>

Wittgenstein says:

> ...if anyone believes that certain concepts are
> absolutely the correct ones, and that having
> different ones would mean not realizing something
> we realize - then let him imagine certain very
> general facts of nature to be different from what
> we are used to, and the formation of concepts
> different from the usual ones will become intell-
> igible to him. (PI p. 230)

Examples of this method can be found in Wittgenstein's later work:

> It interests us, for instance, to ascertain that
> in our surroundings certain shapes are not con-
> nected with certain colors, that for instance we
> do not see green always associated with circularity
> and red with squareness. Were we to imagine a world
> in which colors and shapes were joined in this fashion,
> we would find intelligible a conceptual system in which
> the basic division — shape and color — would not exist.[1]

Wittgenstein did not restrict himself to imagining worlds different from

this one, but also spoke of people whose motives in, say, calculating

are unlike ours (RFM IV 8), or whose modes of perception differ from the

ones we share:

> Is it possible to observe that a surface is coloured
> red and blue; and not to observe that it is red?...
> I am imagining that the observation happens by means
> of a psychological sieve, which for example only lets
> through the fact that the surface is blue-white-red
> (the French tricolour) or that it is not. (RFM V 42)

Wittgenstein also gave examples of people who, without differing from us

in perceptions or environment, simply count (RFM I 136), measure (RFM I 5),

calculate (RFM I 148) or guage the value of commodities differently.

What sort of philosophical positions did Wittgenstein mean to

challenge by such examples? Logical Atomism for one, and related

philosophies: the ideas of Russell, Frege, the Positivists, and those

expressed in Wittgenstein's own early work, the Tractatus Logico - Phil-

osophicus. Examples of concepts believed to be "absolutely the correct

ones" by such philosophers appear throughout the Investigations and

Remarks on the Foundations of Mathematics: the idea, for instance, that

names must designate absolutely simple objects and concepts (PI 59)

(since "If a proposition is to be definitely true or false in every

case...then its names must pick out their objects with pinpoint pre-

cision."[2]); the idea that "what we MEAN...must always be sharp"[3] and

that the meaning of a rule or expression determines all of its future

applications unambiguously (PI 84)[*]; the idea that all languages,

analysed properly, can be shown to have the same underlying structure

(i.e., that there exists a "general form of propositions and of language"

(PI 65)); the idea that there is a "logical reality lying behind and

sanctioning our practice of inferring"[4] (see PI 97); and the related idea

that our mathematical expressions describe a world of abstract entities.

In this thesis I will raise a number of questions about the imaginative

method Wittgenstein used to uproot such philosophical suppositions, in

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[*]Against such ideals of exactness of meaning Wittgenstein poses (as
part of a many pronged argument) the example of PI 556. He imagines a
language "...with two different words for negation "X" and "Y". Doubling
"X" yields an affirmative, doubling "Y" a strengthened negative. For
the rest the words are used alike." The arguments that follow in section
556 show how difficult it is to say whether the two words have the same
meaning in the sentences in which they occur.

particular: Do his examples work in isolation, apart from the main body
of his ideas? Is the method free from circularity?

Many of the philosophers Wittgenstein attacked had, by his account,
one thing in common: their eagerness to "penetrate phenomena" (PI 90)
and find the apriori order of the world (PI 97) led them to search behind
the ordinary expressions used by human beings for the essence of language
(PI 92). Unwilling to simply "look" and appreciate the diversity of our
expressions (PI 65,66), they tried to restrict "the order of possibilities"
and foist conditions on language and the world as the Positivists did
when they laid down the requirements a sentence had to meet to be mean-
ingful. Of such attempts to find the essential forms of language or the
world Wittgenstein said:

> Here again we get the same thing as in set theory: the form
> of expression we use seems to have been designed for a god,
> who knows what we cannot know; he sees the whole of each of
> those infinite series and he sees into human consciousness.
> For us, of course, these forms of expression are like
> pontificals which we may put on, but cannot do much with,
> since we lack the effective power that would give these
> vestments meaning and purpose. (PI 426)

At the turn of this century work was done in science which seems similar
in spirit to Wittgenstein's. Einstein, for instance, in defining "sim-
ultaneity", did not rely on the current uncritical notions of an absolute
time independent of all observers; he asked how we, as humans, could tell
whether two events were simultaneous. And the mathematician Poincaré
designed a thought experiment to show that, lacking a God's eye view of
the world, our choice of a mathematics to describe space is a matter of
convention. My thesis begins with the question: can we illustrate or
make credible the method of philosophy suggested in section xii, Part II
of the Investigations by drawing on such examples from the history of

science? A full answer to this question will only emerge later in the thesis, as part of a discussion of the independence of Wittgenstein's method.

At the end of the nineteenth century the French mathematician
Henri Poincaré invented a "parable" or thought experiment which seems,
at first glance, to be an application of the method of philosophy
Wittgenstein speaks of: it concerns the inhabitants of a world in which
certain "very general facts ofnature", different from the ones we know,
prevail. Lawrence Sklar, in Space, Time, and Spacetime, gives a con-
venient summary of the parable, which I shall follow here.
He asks us to imagine:

> ....a closed Euclidean two-dimensional disk, heated to a
> constant temperature at the center while the circumference
> is uniformly cooled to 0° absolute. The temperature
> gradient along the radius is given by $R^2 - r^2$, where R is
> the radius of the disk and r is the distance of a point
> from the center of the disk. We populate the disk with
> two-dimensional creatures who are interested in determining
> the intrinsic geometry of their world. We equip them with
> rigid rods, except that the rods all contract uniformly
> with diminishing temperatures and in proportion to the
> drop in temperature, and all the rods have length 0 when
> their temperature if 0° absolute.[5]

If the inhabitants of this world measured the distances between various
points on the disk, their results would seem incompatible with Euclidean
geometry; they would find, for example, that "...geodesics are not
straight lines but the geodesics of Lobachevski, and the ratio of cir-
cumference to radius of a circle is always greater than 2π."[6] They might
account for such results in one of two ways: by assuming that their
measuring rods remain of constant length upon transport, and that their

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world is a Lobachevskian (or saddle-like) plane of infinite extent,
or by postulating "shrinking" forces which distort measurements of their
real Euclidean environment. Since the inhabitants of the disk would have
no way of telling which of these hypotheses is correct, their choice of
a geometry (Euclidean or Lobachevskian) in describing physical space
would be a matter of convention. And since similar considerations point
to the impossibility of our ever discovering the intrinsic geometry of
three-space (do we say that light rays we use to map space travel free
paths, or do we postulate refractive fields?), the same conclusion applies
to whatever geometry we choose.

Wittgenstein suggests that the method of philosophy proposed in
the Investigations is to be used against philosophers who believe "that
certain concepts are absolutely the correct ones, and that having different
ones would mean not realizing something that we realize". It might be
argued that the effect of Poincaré's parable was to undermine just such
absolutist views as Wittgenstein mentions here. By suggesting how someone
might (without being wrong or "failing to realize something that we
realize") describe the world using a geometry different from the one we
use, the parable calls into question a number of views prevalent before
this century: the view, for instance, that there is one correct geometry
of the world, which can be read off from physical facts; the view that
sensory experience compatible with a non-Euclidean world is impossible
(for a more detailed argument against this position, see below p.p. 24-25);
and the wider view, once accepted uncritically, that empirical tests can
always be found to decide between incompatible theories of the world.[*]

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[*]see bottom of p. 7.

It is unclear, however, exactly how far pointing to the overthrow of views once thought to be "absolutely the correct ones" can contribute to Wittgenstein's attack on the various deeply ingrained philosophical positions challenged in the Investigations and Remarks. The view that our laws of logic describe the "apriori order of the world" (PI 97), and that alternative ways of inferring would simply be mistaken, seems more basic than the view, say, that there is one discoverable geometry of the world.* While Poincare's parable is useful heuristically (as it shows the power of the imagination as a philosophical tool), we will only be able to arrive at a sound estimate of its relevance for Wittgenstein's philosophy later in this thesis.

In Deviant Logic, Susan Haack challenges the sort of "absolutist" view of logic that Wittgenstein worked to dispel. She points out that any such view, according to which the laws of logic are self-evident and unalterable, is threatened, to some extent, by the very existence of alternative logics.[7] One such logic was first developed in 1936 by Birkhoff and von Neumann, and has been endorsed since then by many philosophers and scientists as a solution to problems raised by quantum mechanics. R.I.G. Hughes sketches out a typical reason given for adopting a logic which, in effect, drops the distributive law of classical logic: according to quantum theory:

> ...the electron (like a number of other elementary particles)
> had an intrinsic angular momentum, or spin. The spin is

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*Though at one time people may have considered the two views to be equally certain.

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*Empiricists object to this (common) way of reading Poincare's parable (see below, p. 47).

quantized: it is always found to assume one of
only two values, either up of down, along any
direction in which it is measured. It is im-
possible, however, to specify the spin of an
electron along two spacial axes simultaneously.
For example, if the spin of an electron measured
along the x axis is up, ti is not possible to
assign any definite value to the spin alons the
y axis.
Suppose a beam of electrons is completely spin-
polarized along the x axis, which means that all
the electrons in the beam are found to have the
same spin value (say spin-up) whenever the spin
is measured along the x axis. Because the beam
has not been polarized along the y axis, one can
say of each electron in the prepared beam that
its spin along the x axis is up and that its
spin along the y axis is either up or down.[8]

If we accept the distributive law of classical logic (i.e., from "P and

(Q or R)" we infer "P and Q or P and R"), this last statement implies:

...either that the spin along the x axis is up
and the spin along the y axis is up or that the
spin along the x axis is up and the spin along
the y axis is down.
Both clauses of this assertion, however, violate
the principle of quantum mechanics stating that
spin cannot be specified simultaneously along
two axes. Since neither clause can be accepted,
the assertion itself must be rejected. One
must therefore, either reject the initial
statement about the prepared beam of electrons
or disallow a logical procedure for defining
the consequences of the statement, a procedure that
seemed quite innocuous in ordinary reasoning.
There is no motive at hand for rejecting the initial
statement, and so it seems at least one law of
classical logic cannot be applied to quantum
phenomena.[9]

Recently, a number of scientists and philosophers have claimed that such

treatments of quantum mechanics are unfounded or based on misinterpreta-

tions of the theory: J. Bub, for instance, in The Interpretation of

Quantum Mechanics, holds that under his analysis "all problems in the

way of a realist interpretation of quantum mechanics disappear."[10]* In

Deviant Logic Susan Haack mentions the view:

> ...that developments in quantum mechanics since the
> 1940's, in particular the development of the quantum
> field theory, have dealt adequately, within physics
> itself, with the problems which, in the 1930's and
> 1940's, seemed, to some, severe enough to call for
> a change of logic.[12]

But Haack also points out that continued interest in quantum logics

(Hughes' article, quoted above, was published in 1981) shows that views

opposed to such logics are "at least, controversial."[13] Further, even

if it were shown that the problems within quantum mechanics which seemed

to require a change of logic are pseudo problems, we can still say,

adopting Wittgenstein's method: imagine a world in which the most common

qualities are such that knowing the value of any one for an object

precludes knowing the values of the others. Mightn't the inhabitants of

such a world, without being wrong or failing to realize something we

realize, adopt a non-standard logic? A brief survey of the literature

on deviant logics suggests that even for such a case a number of questions

remain: Would whatever form of language such people adopt (if, say, it

were based on Reichenbach's three valued logic, in which statements take

the values true, false and indeterminate, or on Birkhoff's and von

Neumann's algebraic (and very complex) lattice theory constitute a

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*According to Michael Dummett, a "realistic" interpretation of the
statements of some given class is "essentially, the belief that we possess
a notion of truth for statements of that class under which every statement
is determinately either true or not true, independently of our knowledge
or our capacity for knowledge."[11]

genuine logic, and if so, would it be a genuine rival to classical

logic? Can there ever be grounds (even, say, living in such a world)

for adopting a logic that did rival classical logic? These are the sorts

of questions Susan Haack raises and answers in detail in Deviant Logic,

questions I will return to toward the end of this thesis. It seems clear,

however, even without broaching these questions, that the existence of

quantum logics, and the possibility (suggested by quantum mechanics) of

a world of objects very different from the objects we are accustomed to,

lends some plausibility to Wittgenstein's view of logic and credibility

to the method of philosophy suggested in the Investigations. It may seem

puzzling, then, that Wittgenstein warned the philosopher against pre-

occupation with science (PI 109) and claimed, in Culture and Value, that

Western scientists were unlikely to understand the spirit in which he

worked.[14] I will try to account for this apparent oddity presently.

So far in this paper I have tried to indicate how the method of inventing fictitious beings and facts of nature can be used to show that concepts "different from the usual ones" are possible. But many of the remarks in the Investigations suggest that this method may have substantial limitations. For even if we assume that whatever we can imagine is also possible, we may, according to Wittgenstein, sometimes be mistaken in thinking we can imagine something. Having a picture of a certain possibility (say, of an inanimate object being conscious (PI 390)) should not mislead us into thinking that the picture, or the sentence associated with it, is necessarily meaningful. We may think, for instance, that we can imagine a race of people who never speak an audible language, and who "still say things to themselves in the imagination" (PI 344), or who never perform oral or written sums, and calculate exclusively in their heads (PI 385), but such pictures are confused and violate "the logic of our expressions". (PI 345) We assume that because people sometimes calculate silently or speak without speaking publicly that a whole people might do nothing else, since, "...then they would merely be doing always what as it is they do sometimes". (PI 344) This, however, is like saying: "If it is possible for someone to make a false move in some game, then it might be possible for everybody to make nothing but false moves in every game." (PI 345) Just as it make no sense to speak of a game where nothing but false moves are made (the concept "game" loses its

purpose), so it is senseless to speak of calculation or language where

no criteria for saying a sum has been arrived at correctly, or a word

used properly, exist.[*]

Throughout the Investigations and Remarks Wittgenstein attacks

various pictures he considered misleading or contrary to the logic of our

expressions: that following a rule is "something that it would be possible

for only one man to do, and do only once in his life" (PI 199); that

someone might be said to invent a game, though it was never played and

though no one had ever played any games (PI 204); that a fixed realm of ma-

thematical objects, existing apart from us, determines the truth and

falsity of mathematical propositions. We think we know what it would be

like for the sequence 7777 to occur (or not) in the decimal expansion of

π, but as Wittgenstein says of such a picture:

> When someone sets up the law of the excluded middle, he
> is as it were putting two pictures before us to choose
> from, and saying that one must correspond to the fact.
> But what if it is questionable whether the picture can
> be applied here?
> And if you say that the infinite expansion must contain
> the pattern ϕ or not contain it, you are so to speak
> showing us the picture of an unsurveyable series reaching
> into the far distance.
> But what if the picture began to flicker in the far
> distance? (RFM IV 10)

To show that the picture may really begin "to flicker in the far distance",

Wittgenstein draws on another example from mathematics. Asking whether

we can be mistaken in thinking we understand the question "Does the

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*One might say, of course, that the people of the imagined society
have private criteria of correctness, but Wittgenstein's famous
private language argument (starting around PI 257) is meant to
exclude this possibility.

sequence 7777 occur in the development of π?", Wittgenstein adds:

> For many mathematical proofs do lead us to say that we
> cannot imagine something which we believed we could
> imagine. (E.g., the construction of the heptagon.)
> They lead us to revise what counts as the domain of
> the imaginable. (PI 517)

Wittgenstein's aim in many of the arguments in the Investigations

and Remarks is to exclude certain "sentence-like expressions" from our

language: unqualified use of the law of the excluded middle; talk of

private languages and of things having consciousness or sensations apart

from the contexts in which such words are normally used. Careful analysis

of such forms of expression can show that they violate the basic "grammar"

of our language, just as careful analysis of the problem of constructing

a heptagon with rule and compass shows that the task is impossible given

our rules of construction:

> "...So does it depend wholly on our grammar what will
> be called (logically) possible and what not — i.e.
> what grammar permits?" — But surely that is arbitrary!
> — It is not every sentence-like formation that we know
> how to do something with, not every technique has an
> application in our life; and when we are tempted in
> philosophy to count some quite useless thing as a
> proposition, that is often because we have not con-
> sidered its application sufficiently. (PI 520)

The question immediately arises—can this form of argument be turned against

Wittgenstein? For when we invent situations in which concepts different

from ours seem intelligible, aren't we always in danger of violating the

subtle logic or grammar of our expressions, as we would if we said:

"Imagine a people who speak no audible language, but still say things to

themselves in the imagination"? The problem is particularly acute, since,

according to Wittgenstein, the pictures and tendencies that lead us to

violate the rules of grammar are all deeply ingrained in our language and

culture. Can we justify trying to undermine or eradicate a certain

picture (say, that a particular concept is necessary) by using a method

which, because it relies on the imagination, allows just such pictures

to intrude? We can say, at the very least, that a philosopher using such

a method would have to be extremely sensitive to the pitfalls of language.

Toward the end of this paper I will return to this point, discussing its

implications for those histories in which the physiology or physical

surroundings of the people described seem possible, or in which their

concepts seem intelligible (i.e., concepts we might have if, say, our

motives or modes of perception were quite different). But the majority

of Wittgenstein's examples seem to involve people with concepts and

practices that no amount of scene-setting could allow us to make sense of.

He suggests, for instance, in <u>Remarks</u>, that someone might conclude 2 + 2

= 6 by the method $\underset{3\ \ 3\ \ 3\ \ 3\ \ 2}{\fbox{x x}}$ (RFM I 38), or that (3.4) + 2 = 10 by counting

$\underbrace{1\ 1\ 1}\ \underbrace{1\ 1\ 1}\ \underbrace{1\ 1\ 1\ 1}$ (RFM I136). Such arithmetics would seem to either
1 2 3 4 5 6 7 8 9 10

be trivially different from ours (i.e., using the numeral "2" in place

of "3") or to be inconsistent in some way. (And there are other examples

in <u>Remarks</u> of people with concepts which seem to involve them in

contradictions: the tribesmen, for instance, who guage the size of a

pile of wood by its area (RFM I 149)). Barry Stroud claims that such

examples do not involve real contradictions but that when we trace out

the wider consequences of counting, calculating, etc. in deviant ways the

problems in understanding what it would be like to follow such procedures

can be "multiplied indefinitely":[15]

> ...consider the people who sell wood at a price
> proportionate to the area covered by the pile of
> wood... Surely they would have to believe that a

> one-by-six-inch board all of a sudden increased in
> size or quantity when it was turned from resting
> on its one-inch edge to resting on its six-inch
> side. And what would the relation between quantity
> and weight be for such people? A man could buy as
> much wood as he could possibly lift, only to find,
> upon dropping it, that he had just lifted more
> than he could possibly lift. Or is there more
> wood, but the same weight?... And so on.[16]

The question arises: If, as we have seen, Wittgenstein admits

that there are concepts which the grammar of our language shows to be

"unimaginable", and holds that it is reasonable to exclude these from

our discourse, why does he invent histories in which just such concepts

(as unimaginable, surely, as the occurrence of 7777 in the infinite

expansion of π, or the race of private mathematicians) are common place?

Stroud tries to answer this sort of question in "Wittgenstein and Logical

Necessity". He claims that if we interpret Wittgenstein's examples

properly we can use them to undermine a Platonist conception of logical

necessity, and, at the same time, defend him against Michael Dummett's

charge of radical conventionalism (explained below). In what follows I

will try to show how Stroud's analysis is unsatisfactory.

In a number of passages of Remarks Wittgenstein suggests that

our agreement over such things as what it means to follow a rule or

mathematical operation through a number of steps in the same way (our

agreement, for instance, that writing "1002" after the series "2,4,6,...

998,1000", is to continue the series in the same way) constitutes the

"given" in human experience, the bedrock for which no justification can

be provided. He says of inference:

> The steps which are not brought into question are
> logical inferences. But the reason why they are
> not brought into question is not that they "certainly

> correspond to the truth" — or something of the
> sort — no, it is just this that is called
> "thinking", "speaking", "inferring", "arguing".
> (RFM I 155)

and

> The danger here, I believe, is one of giving a
> justification of our procedure when there is no
> such thing as a justification and we ought simply
> to have said: that's how we do it. (RFM II 74)

The most we can say of a particular step in a chain of inference is that

humans generally (this is one of the facts of our "natural history") take

this step — but not that we do so, or are correct to do so, because the

step corresponds with "the truth", or a realm of abstract objects, or the

meanings we have given our terms, or any mental event or sensation such

as a feeling of intuitive certainty. Wittgenstein's arguments throughout

Remarks and the Investigations are devoted to showing that these various

philosophical explanations of inference are vacuous, that we can find

nothing that compels* or justifies (in the philosophical sense) our

procedures. Does this commit Wittgenstein to a form of "radical con-

ventionalism", the view that "...we are free not only in the choice of

our axioms and rules, but also in what to count as following them, free

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*see RFM I 113, for instance: "But am I not compelled, then, to go the
way I do in a chain of inferences?" — Compelled? After all I can pre-
sumably go as I choose! "But if you want to remain in accord with the
rules you mubt go this way." — Not all, I call this accord. — "Then you
have changed the meaning of the word 'accord' or the meaning of the rule."
— No; — who says what "change" and "remaining" the same mean here? How-
ever many rules you give me — I give a rule which justifies my procedure.

to count whatever we wish as consequences of our rules"?[17]* Stroud

summarizes Wittgenstein's position as follows:

> ...Wittgenstein points out that it is essential
> to inferring, calculating, counting, and so forth,
> that not just any result is allowed as correct.
> If everybody continues the series as he likes,
> or infers just any way at all then "we shan't call
> it 'continuing the series', and also presumably
> not 'inference'."[19]

Hence, while nothing we need give a metaphysical explanation of compels

us to a certain result in a proof, we are not free to calculate or infer

as we choose, simply because gratuitous operations with signs are not

calculations or inferences. What we have come to call calculating,

inferring, etc. are regular practices and could not exist at all if

people constructed proofs haphazardly and could not agree over their

results. We wouldn't call such activity calculating or inferring, not

because haphazard "proofs" couldn't possibly "correspond to the truth",

but because we only apply such words to a certain type of practice.

Hence, while Wittgenstein observes in <u>Remarks</u> that a person who didn't

accept the standard mathematical proofs would come into conflict with the

rest of humanity, and would relinquish techniques that had been found

"to pay" (see RFM I 116, for instance), he gives no further "explanation"

of why we agree in logic and mathematics, or why we should agree:

> I have not said <u>why</u> mathematicians do not quarrel,
> but only <u>that</u> they do not. (PI p. 226)

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*i.e., After a proof has been carried out can we still "decide" whether
to count it as a proof? And is it up to us "to decide to regard any
statement we happen to pick on as holding necessarily?"[18]

The trouble with Stroud's treatment of Wittgenstein's "histories"
is that it seems, in places, to turn Wittgenstein's remarks on our not
having "a clear concept" of something into an explanation of "why
mathematicians do not quarrel". In defending Wittgenstein against
Dummett's charge of radical conventionalism, Stroud uses the following
argument: A conventionalist account is one that presents us with clear
or intelligible alternatives to our practices. But the concepts
Wittgenstein invents in his examples are not meant to be intelligible:

> Can the people in Wittgenstein's examples properly
> be said to differ from us only in having adopted
> different conventions? I think the answer is "No".
> One thing implied by saying that we have adopted,
> or are following, a convention is that there are
> alternatives which we could adopt in its place.
> But in the case of writing "1002" right after
> "1000" there appear to be no alternatives open
> to us. It seems impossible to understand how we could
> "adopt the convention" that writing "998,1000,1004,..."
> is going on in the same way, or taking steps of the
> same size.... And since the intelligibility of al-
> ternative concepts and practices is required by the
> thesis of radical conventionalism which Dummett
> ascribes to Wittgenstein, I think the thesis in not
> borne out by Wittgenstein's examples.[20]

Stroud's aim in his article is to show that Wittgenstein's examples
preserve the "objectivity"[21] of our procedures. Having outlined
Wittgenstein's remarks on our "shared judgements" and "forms of life",
he says that while logical necessity is not, in Wittgenstein's view, "like
rails that stretch to infinity and compel us always to go in one and only
one way...neither is it the case that we are not compelled at all."[22]
But how are we compelled? And what does "compelled" mean if not "forced
to go in one and only one way"? Stroud might say that Wittgenstein's
examples show that there is never just one way of following a rule open

to sentient beings in general, but that it is a (contingent) fact that,
as humans sharing certain "judgements" and "natural reactions" (Stroud's
phrase), we will find only one way of preceding in a proof intelligible.
The argument Stroud outlines against considering Wittgenstein a radical
conventionalist (above) seems to commit Wittgenstein to this position.
But while Wittgenstein does say in Remarks:

> So much is clear: when someone says: "If you follow the
> rule, it must be like this", he has not any clear concept
> of what experience would correspond to the opposite.
> (RFM III 29)

this observation on how humans react to rules of logic and mathematics
should not be turned into a principle guaranteeing the objectivity of
those procedures. The argument (supposedly consistent with Wittgenstein's
ideas): "Radical conventionalism is not tenable — we are not free to
continue a series or proof any way we like because at each step of the
proof we find only one way of proceeding intelligible" is non-empty only
if we have some criterion for deciding whether a step is intelligible
apart from the fact that humans take it. For unless we can say what
makes a step intelligible apart from this, then the claim that we are not
free to procede in a certain way because to do so would be unintelligible
to us reduces to the claim that we cannot procede that way because we do
not. Hence if we want to speak of being "compelled" in mathematics, or
find some guarantee that humans will continue to agree in a determinate
way, then we need an account of what makes a step intelligible or unin-
telligible: we are led to search for a principle grounding our agreement.
But this is exactly what Wittgensetin wants to avoid, and why his remarks
on intelligibility and our shared "judgements" and "forms of life" should

not be seen as referring to mechanisms that assure our agreement, but rather as analogous ways of saying "This is what we do". If something assured our agreement or determined our procedures then it (and not our agreement) would be the "given".

In place of the claim "These precedures (in logic and mathematics) are intelligible, therefore we follow them", Wittgenstein would substitute the claim "These procedures are followed and are therefore (what is) intelligible". The first claim seems plausible (as does Stroud's account) because we think we have a means of deciding whether a certain step in a proof is intelligible: we can introspect and find out by that means. But just as, in the Investigations, Wittgenstein argues against identifying understanding exclusively with any mental event, feeling or process, and insists that the final criterion of understanding must be public (the use to which we put a rule), so in Remarks he distinguishes thinking from inner acts:

> ....what goes on in the talker, his images, sensations
> and so on...does not constitute the thinking. (RFM I 116)

When someone asserts a contradiction or mathematical absurdity we may feel: "...while he may indeed say it, still he can't think it". But Wittgenstein replies: "...not: try as he may he can't think it, but: it is for us an essential part of "thinking" that — in talking, writing, etc. — he makes this sort of transition". (RFM I 116) Though Wittgenstein admits that there is some sense in saying "he can't think it", since confronted with an absurdity we can't "fill it with personal content", or "respond", or feel we can "go along with it" (RFM I 116), still, such responses (or their absence) are inessential to thinking.[*] What is

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[*]see bottom of p. 21.

thinkable can only be read in "the technique of thinking" (RFM I 133),

those rules and procedures in mathematics, logic, etc. that "show what

human beings call thinking". (RFM I 131) What can be thought is given,

at any one time, by those procedures, not by introspection, and that is

why, for Wittgenstein, an account of what can be thought cannot ground

such procedures or show them to be determined.*

In Remarks, just after suggesting that the essential criterion

for deciding whether something is unthinkable isn't introspection,

Wittgenstein adds:

> And I say further that the line between what we include
> in "thinking" and what we no longer include in "thinking"
> is no more a hard and fast one than the line between
> what is still and what is no longer called "regularity".
> (RFM I 116)

This view seems to follow from Wittgenstein's identification of thinking

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*Though I owe my insight into Wittgenstein's view of thinking primarily
to my reading of Remarks on the Foundations of Mathematics, Baker and
Hacker give a similar account in Wittgenstein: Understanding and Meaning.
see, for instance, p. 476: "But whether we can or cannot "think a
particular thought" is not "discovered" by strenuous mental endeavour.
It is determined by whether a sentence has sense, whether the rules of
grammar permit a certain combination of words, whether we assign sense
to such a sentence."

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*Wittgenstein does not deny that the phenomena philosophers cite to
explain the regularity of our practices exist (say, the feeling that we
are compelled by a proof, or that we cannot really think an absurdity):
he simply gives us a different picture of their importance. Confronted
with an alien society, we would apply the term "proof" to whatever
strings of sentences had the role of proofs in their lives, regardless of
our ignorance of what goes on inside such beings.

with "the technique of thinking". For if we were to compare the public

rules and procedures of inference, calculation, etc. followed in a

modern society with those of a pre-modern, or ancient, or primitive

society, and noted the sorts of expressions allowed therein, we would

see differences in what those societies "included in thinking".

To support the view of thinking outlined above we would have to

draw on Wittgenstein's various arguments against identifying understanding,

meaning etc., with inner acts or experiences. I will pass over the

majority of these arguments at this point since my aim in this thesis is

as much to clarify Wittgenstein's ideas and show their interconnection

as to argue for them in detail. I wish to show, in particular, how a

correct interpretation of Wittgenstein's "histories", and of the method

of inventing such, requires a clear overview of his ideas on thought

and imagination. But in showing this, I also hope to indicate where,

in Wittgenstein's wider work, replies can be found to a number of the

objections one is inclined to raise to his method.

There is one strand among Wittgenstein's arguments that cannot
be overlooked if we wish a clear picture of his account of thought. For
even if we were convinced that what can be thought is not given by
private introspection, we might still believe that science will someday
show us what humans ought to have "included in thinking" all along, and
free us from having to read off what is thinkable from the "technique
of thinking". But in section 109 of the Investigations Wittgenstein
says:

> It was true to say that our considerations could not
> be scientific ones. It was not of any possible
> interest to us to find out empirically "that, contrary
> to our preconceived ideas, it is possible to think such-
> and-such" — whatever that may mean. (PI 109)

This statement seems to conflict with those of section xii, Part II of
the Investigations where Wittgenstein first proposes the method of
inventing fictitious facts of nature, and remarks that the philosopher's
interest "certainly includes the correspondence between concepts and
certain very general facts of nature". For is such a correspondence
exists, why shouldn't the philosopher make extensive use of scientific
facts in investigating it, or wish to know, on empirical grounds, what
possibilities of thought really are open to humans?

Though it is unclear from section 109 what Wittgensetin means
when he speaks of "finding out empirically that we can think such-and-such",
two possibilities come to mind:

a) we may discover a psychological or physiological fact about humans which would show new possibilities of conceptualization or visualization.

b) we might discover a fact about our surroundings which would inspire a new way of thinking about various features of the world (say, time and space).

Using examples from modern science I will try to say what such discoveries might look like, and explain why Wittgenstein would minimize the philosophical importance of either type of discovery.

In Space, Time and Motion, W.C. Salmon reiterates the arguments by which the nineteenth-century scientist Hermann von Helmholtz claimed to have shown that our inability to visualize or imagine a non-Euclidean space of three dimensions is "a matter of psychological fact — not a priori necessity":[23]

> There are no special difficulties in visualizing two-dimensional spaces, whether they be Euclidean or non-Euclidean. We can literally see the surfaces or we can call them up in our imagination. In either case they are seen from without as two-dimensional manifolds embedded in three-dimensional space.... Let us call this external visualization.
> It is easy to suppose that there is no particular problem in visualizing a Euclidean space of three dimensions, but clearly this is very different from the external visualization of a two-dimensional space. We cannot step outside of our three-dimensional space into a four-dimensional space in order to visualize externally either a three-dimensional Euclidean space or a three-dimensional non-Euclidean space. Instead, we must formulate an appropriate conception of internal visualization in order to understand what is involved in the visualization of a three-dimensional space of any variety.... To visualize a space internally...is simply to imagine the types of experiences one would have if he were living in such a space.[24]

Such experiences, says Salmon, for a person living in a non-Euclidean space, might include finding the sum of the angles of all triangles measured to be greater than $180°$, or discovering similar departures from

Euclidean geometry in the relation between the radius and volume of

spheres.

Salmon adds:

> It may seem simply impossible to imagine or picture
> a three-dimensional non-Euclidean space, no matter
> how successfully we can conceive intellectually the
> possibility of measurements that fit a non-Euclidean
> geometry. We must not forget, however, the psychological
> power of our lifelong conditioning to the Euclidean
> framework. If we had grown up in a world in which
> non-Euclidean relationships were a matter of daily
> experience, then it seems likely that the visualization
> of a three-dimensional non-Euclidean space would pose
> no more psychological difficulty than does the visual-
> ization of three dimensional Euclidean space for us.[25]

Insofar as this argument shows that a way of visualizing or imagining

space which we thought was necessary is not so, Wittgenstein might have

considered it philosophically interesting. But as an example of the

method suggested in section xii, Part II of the Investigations, he would

probobly have found it unnecessarily elaborate and reliant on scientific

information (see below). Further, though the argument does not claim to

have shown definitely that we can visualize non-Euclidean space, it

suggests that the question might be resolved by empirical investigation.

It is the empirical resolution of the question that Wittgenstein would,

section 109 suggests, find irrelevant philosophically.

But why should the answers science gives to such questions be of

no interest philosophically? Why shouldn't a philosopher concern himself

with what we can show, on empirical grounds, to be thinkable or unthink-

able? In section 392 of the Investigations Wittgenstein suggests that

for many questions our analysis inevitably "oscillates between natural

science and grammar". The philosopher's concern is always with the

latter, those most general (and obvious) rules and procedures given in our language, which one can survey without performing any experiments or carrying out elaborate observations. To illustrate the difference between grammatical observations and scientific ones, Wittgenstein asks us, on page 212 of the Investigations, to imagine a physiological explanation of the experience of "seeing an aspect" of a figure:

> When we look at the figure, our eyes scan it
> repeatedly, always following a particular path. The
> path corresponds to a particular pattern of oscillation
> of the eyeballs in the act of looking. It is
> possible to jump from one such pattern to another
> and for the two to alternate.... Certain patterns
> of movement are physiologically impossible; hence,
> for example, I cannot see the schematic cube as two
> interpenetrating$_*$prism." And so on. Let this be
> the explanation. (PI p. 212)

One might want to say that the discovery of such an explanation would show that seeing an aspect of a figure is "a kind of seeing", a physiological event which can be accounted for scientifically. But Wittgenstein answers:

> You have now introduced a new, a physiological,
> criterion for seeing. And this can screen the
> old problem from view but not solve it.... The
> psychological concept hangs out of reach of
> this explanation. (PI p. 212)

A discovery of the sort Wittgenstein describes needn't have any bearing on the ways and conditions under which people apply the (psychological)

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*Interestingly, this passage may have been written with Helmholtz in mind, whose works on physiological optics sought (in his words) "...to explain our knowledge of the visual field through observation of likenesses while we move our eyes".[26]

expressions "seeing", and "seeing an aspect", unless, of course, it became standard practice for people to take certain types of movement of the eyes as the sole criterion for having seen an aspect. And since the philosopher's interest is in "grammar", he need concern himself with physiological or psychological accounts of "seeing", "reading", "thinking", etc. only if such accounts become part of the grammar or application of those terms.

But suppose we answer: no matter what ordinary criteria continue to govern peoples' use of terms such as "seeing an aspect", "visualizing", "thinking", "imagining", etc., we could be forced, if we discovered explanations of the sort Wittgenstein proposes on page 212 of the Investigations, to treat the physiological events as final criteria, since they (and not what is given in grammar) are essential to the psychological phenomena. We might distinguish, as Wittgenstein does in sections 157 and 158 of the Investigations, between criteria for saying a machine has read script (the movement of certain parts, etc.), and criteria for saying a human has done so (certain types of behavior). If we can only determine when, say, a child first begins to read, by observing his behavior, then it makes no sense to speak of "the first word the child ever read", since behavioral criteria are too vague to allow such expressions. But we tend to think that whatever expressions we can apply to the machine ("The first word it read"— the word it registered after we had connected certain parts) can also be applied to humans. We assume that some type of mechanism (which we would surely understand if not for our "too slight aquaintance with what goes on in the brain and nervous system" (PI 158)) must underlie our activities. But Wittgenstein's point in section 157

and 158 of the Investigations is this: since nothing guarantees that we

will ever find physiological explanations of "reading", "thinking",

etc.*, and since the knowledge of neurological processes can't possibly

guide our present application of psychological words, we shouldn't let

our enthusiasm for mechanistic pictures influence our analysis of the

grammar of such words. Suppose we reply, however, that in using express-

ions such as "The first word the child read" we are simply exploring a

type of grammar that will undoubtedly force itself upon us when we do

discover neurological explanations?** Does Wittgenstein's view that a

philosopher need not be concerned with what science might tell us about

thought, reading, etc., and his injunction against hypotheses of the sort

"If this were the case (or were discovered) then we would have these

concepts, this grammar",*** simply reduced to a faith that no mechanistic

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*In Zettel Wittgenstein says: "No supposition seems to me more natural
than that there is no process in the brain correlated with associating or
with thinking; so that it would be impossible to read off thought-processes
from brain-processes.... If this upsets our concept of causality then it
is high time it was upset."[27]

**It is not very helpful to explain Wittgenstein's remarks in section 109
of the Investigations by citing the formula: "Grammar gives what can be
thought — that is why empirical discoveries are of no interest to the
philosopher". (This is what the analysis of Baker and Hacker seems to
amount to: see pages 476 and 520-21 of their book.) For the question
remains: What effect do advances in science have on our criteria for
applying various ordinary terms: can't empirical discoveries force a
change in grammar?

***See section xii, Part II of the Investigations, where Wittgenstein
says: "I am not saying: if such-and-such facts of nature were different
people would have different concepts (in the sense of a hypothesis)...."

explanations of psychological terms will ever be found? For even admitting that we can never, strictly speaking, be _compelled_ by scientific results to change our criteria for applying a psychological term, still, can't we see in advance what sort of criteria and ways of speaking a result will lead us (as rational human beings)to adopt, and shouldn't a philosopher concern himself with such possibilities?

A defender of Wittgenstein, believing in the "unconditioned" nature of our practices, might argue that human history follows an unpredictable course,* and hence that philosophers should simply describe and not anticipate (or propose) changes in grammar. But even without drawing on a particular view of history, or the point stressed by many philosophers of science, that the way scientific paradigms replace one another historically is never a simple matter of logic (as if rigid logical rails determined the future course of science), we can, I think, defend Wittgenstein's position on psychological terms. For it is not clear that any new scientific paradigm, even foreseen, need force fundamental changes in the grammar of psychological terms. Suppose we find that continuing a particular mathematical series correctly can be

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*The following quote from Wittgenstein seems to suggest he held this view (I found the quote in R. Rorty's _Philosophy and the Mirror of Nature_, where it serves as an epigraph, hence I can provide no context): "when we think about the future of the world, we always have in mind its being at the place where it would be if it continued to move as we see it moving now. We do not realize that it moves not in a straight line, but in a curve, and that its direction constantly changes."

attributed to, or correlated with the occurence of a certain brain-process. Could we ever rule out the possibility that at some point in the series, which we might not reach until long after the discovery, the brain-process should only correspond to what everyone judged to be an incorrect step? That this might happen suggests that physiological criteria need never become final criteria for continuing a series correctly (or even for knowing how to). Similar arguments apply to what physiology might allow as "thinkable", "imaginable", "being in pain", etc.

Despite what we have said so far about the grammar of psychological terms, it would be senseless, I think, for a supporter of Wittgenstein to deny that empirical discoveries, and the scientific advances occasioned by such discoveries, can, in certain areas, bring important changes in grammar and thereby significantly alter the "land-scape" of what can be thought. To see, for instance, the change of grammar brought to physics by the relativistic account of spacetime, we need merely adopt a technique Wittgenstein frequently used to differ-entiate the grammar of two words — that of comparing the varieties of question in which the words can meaningfully occur. If we compare the types of question which the Newtonian and relativistic "pictures" of spacetime allow for the same set of words — "time", "place", etc. — we see that the current picture of spacetime carries a grammar very different from that of the previous one. For, given a pair of events, e_1 and e_2, in Newtonian spacetime:

> We can ask if they occured at the same time. We can
> ask, given that they did not occur at the same time,
> just how large the temporal interval between them was.

> (If they did not occur at the same time we can
> also ask which of them was <u>later</u> than the other...)
> ...We can ask whether or not the events occured at
> the same place. If they did not, we can inquire
> about the spatial separation between them... "How
> far apart in space were e_1 and e_2?" is a meaningful
> question whether or not e_1 and e_2 were <u>simultaneous</u>
> events.[28]

But none of these questions can be asked about events in Minkowski

spacetime!

One might reply, of course, that the special grammar of Minkowski

spacetime, in which the "intervals" between events are given by integral

formulae,[29] has remained the concern of scientists and left our ordinary

grammar unaffected. But even if this is true, we can still imagine

conditions, of the sort humans may someday encounter in space, in which

relativistic ways of speaking might be common. For it is clear that in

ordinary discourse "Simultaneity is assumed to be an invariant notion,

two events simultaneous for one observer are simultaneous for any other

observer".[30] Further, if an event <u>a</u> is simultaneous with <u>b</u> relative to

one observer and <u>b</u> is simultaneous with <u>c</u> for another observer, we can

safely assert (without qualification) that <u>a</u> is simultaneous with <u>c</u> for

both observers. But in relativity theory:

> The notion of simultaneity we end up with is not
> invariant, it is only defined relative to an
> observer in a given inertial state of motion...
> there is no reason to believe in general that <u>a</u>
> is simultaneous with <u>c</u> relative to either observer.[31]

If we were accustomed to communicating with observers moving at velocities

approaching the speed of light we might begin to speak of simultaneity

only relative to particular frames of reference (realizing that the law

of transitivity does not apply across frames of reference as it does in

ordinary discourse) and to conceptualize time in the manner suggested by

O. Costa de Beauregard:

> In Newtonian kinematics the separation between past
> and future was objective, in the sense that it was
> determined by a single instant of universal time,
> the present. This is no longer true in relativistic
> kinematics...[32]

> To use a two dimensional analogy, one might say that
> classical kinematics can be graphed on a sheet of
> ruled paper in which the coordinate axes are labeled
> <u>absolute space</u> and <u>absolute time</u>, while relativistic
> kinematics is inscribed on a blank sheet of paper,
> each point provided with a compass card divided
> into three sectors labeled <u>past, future</u> and <u>elsewhere</u>.[33]*

FUTURE
ELSEWHERE ELSEWHERE
PAST

FUTURE
——————
PAST

> ....there can no longer be any objective and essential
> (that is, not arbitrary) division of space-time between
> "events which have already occured" and "events which
> have not yet occured".[34]

The argument rebutted in the preceding paragraph is inadequate for a

second reason: To say that the grammar of physics should be ignored

because it hasn't affected our everyday discourse is to restrict the scope

of philosophy arbitrarily.** Wittgenstein's ideas do not commit him to

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*Events labeled "elsewhere" for an observer at a particular point of
spacetime are those that cannot be reached by any causal signal leaving
him at that point.

**It wasn't simply because scientific explanations of psychological terms
might leave the grammar of those terms unaffected that I argued that
philosophers might ignore such explanations, but because our ordinary
grammar may embody as much of what is important to "thought", etc. as
any scientific grammar. I don't think this is true, however, of the
physical terms "space", "time", etc.

taking only what ordinary humans utter in the most mundane circumstances as sound or interesting: he is not, as many of his critics seem to think, an "ordinary language" philosopher in that respect. In his voluminous later work he explores the conditions of language in general, remarking on topics in the foundations of mathematics, aesthetics, ethics, anthropology, biology and physics. Further, his view that what can be thought is given in the "technique of thinking" seems to commit him to saying that advances in science and mathematices can bring substantial changes in grammar. Why then does he warn the philosopher against preoccupation with science and empirical facts? We have seen why he might do so for a mechanistic account of thought, but why science in general? I will suggest two reasons for this.

In A Companion to Wittgenstein's "Philosophical Investigations" G. Hallett quotes Wittgenstein's comment on Sir James Jean's The Mysterious Universe: "...I loathe it and call it misleading".[35] Wittgenstein abhorred speculative popularizations of modern science, just as he abhorred similar treatments of mathematics. His aim in the philosophy of mathematics was, as V.H. Klenk points out, not to tamper with the subject by, say, prohibiting the use of non-finitary techniques of proof, but to warn against using mathematical results to prove unwarranted philosophical theses (the existence of abstract entities, or infinite sets, for instance).[36] But though he thought that philosophy should leave mathematics "as it is",[37] this did not prevent him from filling a book with remarks on the foundations of mathematics. This suggests that similar foundational work might safely be done in science, so long as the philosopher was content with a descriptive overview of the subject,

and did not try to create a superstructure of his own, or use physical terms outside of the theories in which they have an application.*

(Whether proper analysis could make philosophical problems disappear in science, as Wittgenstein hoped they would in traditional philosophy, is beyond the scope of this paper to say; Lawrence Sklar's book Space, Time and Spacetime is devoted to showing that philosophical problems continue to surround modern theories of space and time.)

Wittgenstein's second reason for renouncing philosophical pre-occupation with the results of science we have already touched on. Though examples from science can be used to make the possibility of "concepts different from the usual ones" intelligible (as I have tried to show in my discussion of quantum logic and Helmholtz's arguments), Wittgenstein would warn against the temptation to turn such thought experiments into "hypotheses". In section xii, Part II of the Investigations he suggests that the method of philosophy proposed there should not be used to support claims of the form: "If such-and-such facts of nature were thus, people would have these concepts", but only to show that concepts we believe to be necessary are not really so. And we shouldn't think that a cure for our tendency to turn thought experiments into

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*"Why shouldn't I apply words in ways that conflict with their original usage?... In a scientific perspective a new use is justified by a theory. And if this theory is false, the new extended use has to be given up. But in philosophy the extended use does not rest on true or false beliefs about natural processes. No fact justifies it. None can give it any support."[38]

philosophical hypotheses might be found in more scientific knowledge,

or even a conventionalist (what Sklar calls a "super Duhemian"[39])

perspective on the history of science. To see this we need merely note

Poincaré's treatment of his own parable. In Space, Time and Spacetime

Sklar says:

> ...the allegation is frequently made by philosophers
> that for any full blown physical theory an alternative
> theory can be constructed which saves all the same
> observational consequences. In the case of geometry,
> however, we can not only speculate about such alter-
> natives but, as the Poincare parable shows, actually
> construct them.[40]

But while Poincaré's arguments lead him to deny that we can discover an

a priori (or even a posteriori) relationship between any one geometry

and the world, this did not prevent him from advancing a hypothesis of

the sort Wittgenstein warned against. For despite his claim that our

choice of a geometry to describe the world is essentially a matter of

convention, Poincaré argued that Euclidean geometry will always be our

proper choice, since it is simpler than any of our rivals. Though

Poincaré's argument contains a logical flaw (for even if Euclidean

geometry is simpler than any other, it doesn't follow that we should

choose it to describe the physical world, since, as a system of physics,

the conjunction of Euclidean geometry and Newtonian physics with Lorentz

modifications may be more complex than the conjunction of non-Euclidean

geometry and Einsteinian physics[41]) Wittgenstein would have seen addition-

al reasons for rejecting it. For the argument assumes that no matter how

our circumstances change we should (i.e. would be compelled as rational

beings to): a) value simplicity over all (or even most) of the qualities

we might weigh in choosing a geometry, and b) continue to judge Euclidean

geometry the simplest of the geometries. But the method of philosophy
proposed in section xii, Part II of the Investigations is meant to
undermine just such assumptions. Euclidean geometry, according to
Poincaré, is simplest not only "because of our mental habits...it is the
simplest in itself, just as a polynomial of the first degree is simpler
than a polynomial of the second degree".[42] But can't we imagine beings
for whom mathematical operations involving second degree polynomials
might be easier than operations involving those of the first degree, or
beings accustomed to visualizing and working with non-Euclidean geometry?
Or, more to the point, can't we imagine beings who might say, sincerely,
that they judged some non-Euclidean geometry (and not necessarily
one of the many geometries developed by mathematicians and
physicists since Poincaré made his claims)   to be simpler than Euclidean
geometry? Such questions show Wittgenstein's method in its most basic
form: whenever we feel tempted, in keeping with a certain concept, to
say "Things must be thus", or feel compelled, in calculating or inferring,
to take a certain step, we should simply imagine doing or saying other-
wise. The many examples of unintelligible practices and statements that
dot Wittgenstein's work are the fruit of this method, and show that the
invention of intelligible alternative concepts based on the results of
science cannot be the whole of Wittgenstein's method. For the temptation
to say that, faced with certain facts of nature, we would (by whatever
principle: "simplicity", "comprehensiveness", "degree of confirmation",
etc.) have these concepts, requires a more radical form of therapy.

But the question remains: does Wittgenstein's method, when it
it used to present us with unintelligible concepts, have any real force?

For a philosopher who holds that Euclidean geometry is simple "in itself" might say that beings who judged Lobechevskian geometry "simpler" than Euclidean would not be speaking about "simplicity," but some other quality, just as a Platonist might say that beings who adopted an alternative arithmetic would (even if by some miracle they could use arithmetic for practical ends) not really be speaking about numbers. I will deal with this objection in what follows.

IV

The objection to Wittgenstein's method raised at the end of the
last section is similar to one posed by Crispin Wright in <u>Wittgenstein</u>
<u>on the Foundations of Mathematics</u>. Referring to Wittgenstein's discussion
of people who measure with elastic rulers (see RMF I 5), Wright says:

> Measurement with soft rulers will be useless if the
> results are applied for the kinds of purposes for
> which we measure; but if they are not, it is seriously
> unclear what good grounds there could be for saying
> that people who, talking apparent English, solemnly
> lay floppy rulers alongside things and seem to record
> readings are doing anything that may informatively be
> described as "measuring".[43]

Cora Diamond, in a review of Wright's book, generalizes the objection
as follows:

> Wright argues that Wittgenstein's examples, if
> looked into and developed, will "destabilize":
> it will appear <u>either</u> that the activity described
> has an application so unlike that of measuring
> (inferring, calculating) as to make it unreasonable
> to describe it as such; <u>or</u> the application, the
> purposes, of the activity will be like enough to
> ours to make it clear that these people are using
> procedures inferior to ours: if it is really
> measurement that they are using these soft rulers
> for, they would be well advised to change over to
> more rigid ones. One unit of measurement may be
> superior to another in certain circumstances and
> inferior to it in others; but if Wright is correct
> that is not how it is with the choice of rigid
> rather than soft rulers. If your purposes allow the
> description of what you are doing as <u>measurement</u>,
> you must be better off with rigid ones.[44]

To show that examples of alternative ways of measuring, etc. need not
"destabilize" in either of the two directions outlined by Wright, Diamond

suggests that we imagine people whose method and purpose in applying,

say, an alternative system of measurement are close enough to ours that

we can describe what they do as "measuring", but far enough away that it

will be "unclear whether they should achieve their purpose any better by

going over to our methods".[45] Claiming that what people do "is identifi-

able as reckoning time if the reckoning is used, e.g., to co-ordinate

activities and to keep track of their history and relationships",[46]

Diamond quotes from a book on Feudal society to show that "soft rulers"

were used to reckon time in the Middle Ages:

> Reckoning ordinarily...twelve hours of day and
> twelve of night, whatever the season, people of
> the highest education became used to seeing each
> of these fractions, taken one by one, grow and
> diminish incessantly according to the annual
> revolution of the sun.[47]

She adds:

> A more uniform measure (the burning of candles
> of uniform length) was known but was not generally
> used because it was not wanted; people were gen-
> erally indifferent about what would be marked by
> less "elastic" measures. Given their purposes,
> "elastic" measurement is not inferior.[48]

Professor S. Ajzenstat has pointed out that Diamond's example does not

meet Wright's objection fully, since, insofar as the Medieval system of

time reckoning is useful for co-ordinating activities, it involves

rigid units: the length of the Medieval hour may vary throughout the

day, but at least the individual time keepers use hours of the same size

relative to each other. Hence the possibility of completely elastic

rulers (of the sort Wright mentions) hasn't been shown. Diamond's

example does, however, help to rebut the disjunction she presents, namely,

the claim that any activity we might want to call measuring will either

have an application "so unlike that of measuring (inferring, calculating)

as to make it unreasonable to describe it as such; or the application,

the purposes of the activity will be like enough to ours to make it clear

that these people are using procedures inferior to ours". Recently I

happened upon an example which is, I think, more telling than Diamond's:

> According to a proposal now being considered by
> a committee of the International Bureau of
> Weights and Measures the meter would be defined
> as "the distance traveled in a time interval of
> 1/299,792,458 of a second by plane electromagnetic
> waves in a vacuum"... The best available method
> of establishing the speed of light is to compare
> the wave length of electromagnetic radiation with
> its frequency... Under the proposed redefinition
> such experiments would still be possible, but their
> results would have a different interpretation. The
> speed of light would no longer be subject to revision;
> any refinement in the accuracy of measurement would
> alter not the velocity of light but the length of
> the meter.[49]

Beings with a system of physics in which the speed of light is not

invariant, and in which units of length are fixed by, say, rigid objects,

might accuse us, if we fixed the speed of light in the manner described

above, of using "soft" or completely elastic rulers. Suppose we could

only decide between the variant physics on purely pragmatic grounds, or

that no grounds could be found whatsoever: could we say that only one

of the systems of measurement is "correct" or that one suffers in using

soft rulers? That a concpet as basic and apparently simple as measurement

can now clearly be seen to be tied to systems of physics, that there have

been so many varieties and techniques of measurement, suggests that such

concepts need not be fixed or characterized by Wright's disjunction.

In Remarks Wittgenstein seems to anticipate an objection of the

sort Wright raises: referring to people who measure with elastic rulers

he observes:

> It can be said: What is here called "measuring"
> and "length" and "equal length", is something
> different from what we call those things. (RFM I 5)

But he answers the objection in more general terms than Diamond:

> The use of these words is different from ours; but
> it is <u>akin</u> to it; and we too use these words in a
> <u>variety of</u> ways. (RFM I 5)

Wittgenstein thought that many of our concepts, the most rigorous included ("number", for instance (PI 68)), are used like the concept "game"; we apply them without hesitation to a wide variety of objects and activities, though we can find no single characteristic or set of characteristics shared by all of these things. Board games, ball-games, games played haphazardly by children are all bound together under the concept game by relations of "family resemblance", webs of "similarities and analogies which could not be constructed or predicted in advance".[50] The claim "If your purposes allow the description of what you are doing as <u>measurement</u>, you must be better off with rigid rulers". presupposes that the set of purposes for which we might be said to take measurements is fixed once and for all by the concept "measurement" (and fixed in such a way that each of these purposes can only be served by rigid rulers). Wittgenstein would reverse this claim: the concept of measurement is given at any one time by the totality of purposes and techniques which we associate with measuring, a totality which no simple definition can encompass, but which is marked off and extended by relations of family resemblance. Though Diamond doesn't refer to "family resemblance" in her answer to Wright, and only offers the example quoted above as a tenuous reply, her response is consistent, I think, with Wittgenstein's

wider philosophy, since it suggests how we might, by indicating the

multiplicity of loosely related purposes for which we take measurements,

begin to show that "measurement" is a concept as unbounded as the

concept "game".

In an article published in The Encyclopedia of Philosophy

Norwood Hanson describes the nature of the phenomena studied in quantum

mechanics: in that theory

> ...there exists no concept of "the exact state of
> the microparticle". Indeed the algebraic analogue
> of "electron e is exactly at position x,y,z at time
> t with precisely the energy v", is virtually un-
> grammatical in quantum mechanics, whose rules of
> construction and transfromation could attach no
> meaning to such a symbol cluster. Not only does
> this make the character of our knowledge of micro-
> particles different in kind from our (apparent)
> knowledge of classical microparticles, it immedi-
> ately renders all laws within quantum mechanics
> irreducibly probabilistic. Answers to many well-
> made questions within quantum mechanics do not
> come as discrete quantitative values for general
> algebraic variables. They come rather as ribbons
> or belts or brackets or packets of possible values
> for the variables which figure in one's questions
> about nature.[51]

Even if this sort of interpretation of quantum mechanics proves unfounded

(as some contemporary philosophers and scientists believe will happen),

and it is shown that under the new mechanics we can still speak of

subatomic events as fully determined, and frame realist interpretations

of such, the fact would remain that for over half a century the majority

of scientists were willing to recognize as "physics" the study of

phenomena thought to be so unlike the phenomena studied by classical

physics that a revision in our logic was called for. This suggests

that disciplines radically different in content and application (as

different as soft rulers, used for measuring, are from hard?) may still

fall under the same concept. One could reply, of course, that the old

and the new physics both receive that name because they share a common

set of rules or techniques for investigating nature. But Thomas Kuhn has

argued at length in The Structure of Scientific Revolutions that what the

various "research problems and techniques that arise within a single

normal-scientific tradition" have in common:

> ...is not that they satisfy some explicit or even
> fully discoverable set of rules and assumptions
> that gives the tradition its character and its
> hold upon the scientific mind. Instead, they may
> relate by resemblance and by modeling to one or
> another part of the scientific corpus which the
> community in question already recognizes as among
> its established achievements. Scientists work
> from models aquired through education and through
> subsequent exposure to the literature without quite
> knowing or needing to know what characteristics
> have given these models the status of community
> paradigms. And because they do so they need no
> full set of rules.[52]

According to Kuhn the search for rules guiding scientific research only

becomes important when reigning scientific paradigms are challenged by

new ones, at times, that is, when the possibility of finding a coherent

body of rules common to all science is diminished considerably:

> The transtion from Newtonian to quantum mechanics
> evoked many debates about both the nature and the
> standards of physics, some of which still continue.
> There are people alive today who can remember the
> similar arguments engendered by Maxwell's electro-
> magnetic theory and by statistical mechanics. And
> earlier still, the assimilation of Galileo's and
> Newton's mechanics gave rise to a particularly
> famous series of debates with Aristotelians,
> Cartesians, and Leibnizians about the standards
> legitimate to science.[53]

The difficulty of finding any defining (necessary and sufficient)

rules or content common to all the branches and varieties of physics

(and science in general) suggests that even rigorous disciplines such

as these may be marked off by relations of family resemblance. And this,

in turn, suggests that measurement, a concept tied in its various appli-

cations to scientific paradigms, may be such a concept too. (One could

argue, of course, that our ordinary concept of measurement is insulated

from changes in science, that it is fixed and simple, but we can see

even from Diamond's example (the supplanting of the Feudal system of

time reckoning by the modern), that this claim is dubious.)

Confronted with a philosopher who holds that things can be

simple "in themselves", and that people who judge Lobachevskian geometry

"simpler" than Euclidean could not really be talking about simplicity,

a follower of Wittgenstein would have to cite the variety of ways in

which we use the word "simplicity", showing it to be a concept marked

by relations of family resemblance. Against a Platonist he could only

stress the "MOTLEY" of mathematics (RFM II 46), the profusion of tech-

niques and applications found there (or draw on one of the other argu-

ments against Platonism that can be culled from Remarks). Faced with

the view that the meanings of logical and mathematical constants "compel"

certain uses ("The way the fomula is meant determines which steps are

to be taken." (RFM I 2)), the Wittgensteinian would achieve little by

imagining someone following a certain proof differently from us, for

we might say the person had simply construed or "meant" the terms of

the proof differently. Here the follower of Wittgenstein would have

to resort to an argument about meaning of the sort Klenk gives:

> ...consider the possibility of applying our rules

differently - inferring 'q̄' from 'p' and 'p v q',
for instance. In this case it may seem that the
reason we use the rule differently is that the
meanings of the terms are different. (Perhaps we
have here exclusive disjunction.) But again, this
is to put the cart before the horse. "Isn't this
like saying: 'If this man were to act differently
his character would have to be different'.... We
say 'behavior flows from character' and that is
how use flows from meaning." (RFM I 13) Just as
character can be seen as the cumulative result
of behavior, so the meaning of the term is simply
a function of the way it is always used.[54]

Wittgenstein's method, when used to present us with examples of people

calculating inferring, measuring, or making judgements of simplicity,

etc. in ways that seem unintelligible or inferior to ours, has little

force, it seems, without the support of other of his ideas.

V

Even if we were unwilling to accept those ideas of Wittgenstein

that bolster his more radical examples, we might still hope that the

method of philosophy proposed in section xii, Part II of the Investigations,

when used to present us with intelligible alternative concepts (based, for

instance, on examples from science), is philosophically neutral, and free

from contentious presuppositions. To see that this hope is unfounded,

however, we need only return to the examples mentioned at the beginning

of this thesis. There I suggested that Poincare's parable shows the

possibility of constructing two incompatible theories of the world which

no amount of empirical data could decide between. Lawrence Sklar sketches

out an empiricist response to this reading of the parable: What the

physicist traditionally means by the terms "geodesic" or "straightest

line between two points" is "path of light ray in vacuo between these

points." When we try to preserve Euclidean geometry against unexpected

physical results by changing such "coordinative definitions"[55] (definitions

that link geometrical and physical terms) we are not, according to the

empiricist:

> ...really saving Euclidean geometry at all. We are
> changing the usage of such terms as "geodesic",
> "distance between P and Q along C", etc., in such a
> way that given these new meanings the old sentences
> of Euclidean geometry remain true. But they no
> longer assert the same propositions, since the
> meanings of the terms in the sentences have been
> changed by the revision in the nongeometric portion
> of the theory.[56]

46

Such sentences express what the sentences of, say, Riemannian geometry,

linked with different coordinative definitions, might express. Sklar

generalizes the empiricist or "reductionist" position as follows:

> Presented with the set of alternative, apparently
> incompatible theories, all with the same observa-
> tional consequences, it seems as though we are
> in the position of having to make a choice, but
> having to make it on no grounds of rationality
> whatsoever. But this is merely an appearance.
> A proper understanding of what theories are, and
> what the theoretical assertions contained in them
> mean, will show us that all of the "alternative"
> theories are not proper alternatives at all — they
> are simply the same theory presented in different ways.
> The appearence of multiplicity is simply the confused
> consequence of failure to note ambiguity of linguistic
> usage. Insofar as theories predict the same observa-
> tional results, they are one and the same theory.[57]

Sklar raises a number of objections to this view (none of them offered as

decisive) which I will not discuss here. Even without broaching this

issue too deeply, we can see that the exact import of Poincaré's parable

will reamin unclear until the debate over the way words (in this case

theoretical terms) get their meanings is resolved.

Similar considerations apply in the case of quantum mechanics.

Faced with the question "Is Birkhoff's and von Neumann's quantum logic

a genuine logic, or merely algebra under another name?" a Wittgensteinian

might point to family resemblances between such "logics" and classical

logic, as R.L.G. Hughes does in "Quantum Logic". Having said that "Logic,

although notoriously hard to define, deals with certain kinds of relations

between sentences: What follows from what, what is consistent with what,

and so on,"[58] Hughes points out that the logic of Birkhoff and von Neumann,

even "though it deals entirely with sentences stating that some vector

lies in some subspace,"[59] does these things too. (Other quantum logics,

that of Reichenbach, for instance, are truth functional and much closer

to classical logic.)

The question "Can there be logics that genuinely rival or conflict

with classical logic?" is more difficult to answer. In Deviant Logic

Susan Haack outlines the following argument against genuine rivalry

(not one she supports):

> (a) if there is change of meaning of the logical
> constants, there is no real conflict between
> Deviant and classical logic.
> (b) if there is Deviance, there is change of
> meaning of logical constants,
> so
> (c) there is no real conflict between Deviant
> and classical logic.[60]

Haack presents Quine as a proponent of such a view, on the ground of

such quotes as:

> ...departure from the law of excluded middle would
> count as evidence of revised usage of "or" or "not"...
> For the deviating logician the words "or" and "not"
> are unfamiliar or defamiliarised.[61]

A Wittgensteinian might try to minimize claims of the sort Quine makes

(and hence premise (b)) by pointing out family resemblances in, say,

the usages of the words "or" and "not" in quantum, Intuitionist and

classical logic. But Haack points out that the argument against genuine

rivalry is also undermined if we reject premise (a). In presenting

Quine as a proponent of this argument, however, she seems to have missed

seeing that this is exactly what he does in Philosophy of Logic (a book

discussed at length in Deviant Logic). For though Quine says (in keeping

with premise (b)) that someone who denies the classical law of non-

contradiction "only changes the subject,"[62]* and that the failure of the

law of the excluded middle within a three valued logic is "nominal"

(since "the terminology "true," "false" and "negation" carries over into

it from our logic only by partial analogy"[64]), he still insists that such

a logic is "genuine"[65] and "can coherently challenge our classical true-

false dichotomy."[66]   And the Intuitionist, while not "controverting us

as to the true laws of certain fixed logical operations",[67] should be

viewed:

> ...as opposing our negation and alteration as
> unscientific ideas, and propounding certain other
> ideas, somewhat analogous, of his own.[68]

Hence it is clear that a Wittgensteinian and a Quinean might disagree

over the extent to which there is a change of meaning of logical terms

in deviant logics, but still agree that there are uses of such terms

that constitute genuine logics and that conflict with classical logic.

The foregoing discussion does not, of course, begin to exhaust

the controversy over deviant logics, but it should be clear that for

quantum logic, as for Poincaré's parable, the philosophical implications

of such examples cannot be "read off" directly from the situations

described — invariably questions of meaning must be settled before any

conclusions can be drawn.   This is true, I think, even of Wittgenstein's

simpler examples.   In the Investigations he says:

---

*"They think they are talking about negation, '$\sim$', "not'; but surely
the notation ceased to be recognizable as negation when they took to
regarding some conjunctions of the form 'p·$\sim$p' as true..."[63]

> ...if things were quite different from what they
> actually are — if there were for instance no
> characteristic expression of pain, of fear, of
> joy...this would make our normal language-games
> lose their point. (PI 142)

and

> "What would it be like if human beings showed
> no outward signs of pain (did not groan, grimace,
> etc.)? Then it would be impossible to teach a
> child the use of the word 'tooth-ache'." (PI 257)

A philosopher inclined to say that meanings are established by inner

acts will see very little in such examples when they are given in

isolation. He might answer, as Wittgenstein anticipates:

> Well let's assume the child is a genius and itself
> invents a name for the sensation! (PI 257)

To block this natural response Wittgenstein embarks, in section 257,

on his subtle and many-stranded argument against the possibility of

private languages.

It should be clear, from the points made so far, that to know

what conclusions can be drawn from applications of the method of philos-

ophy suggested in section xii, Part II of the Investigations, and to know

where the method can legitimately be applied (see section II of this

thesis), we must first have resolved a number of questions about words

and the contexts in which they are meaningful. And if we are to draw

conclusions favorable to Wittgenstein's philosophy from applications of

the method, we must first have accepted a number of his ideas on this

issue.

FOOTNOTES

1. Garth Hallet, _A Companion to Wittgenstein's "Philosophical Investigations"_ (Ithaca: Cornell University Press, 1977), p. 747.

2. _ibid._, p. 40.

3. _ibid._, p. 40.

4. V.H. Klenk, _Wittgenstein's Philosophy of Mathematics_ (The Hague: Martinus Nijhoff, 1976), p. 59.

5. Lawrence Sklar, _Space, Time, and Spacetime_ (Berkley: University of California Press, 1974), p.p. 91-92.

6. _ibid._, p. 92.

7. Susan Haack, _Deviant Logic_ (Cambridge: Cambridge University Press, 1974), p. 27.

8. R.I.G. Hughes, "Quantum Logoc", _Scientific American_, 245, 4, p. 202.

9. _ibid._, p. 202.

10. Jeffery Bub, _The Interpretation of Quantum Mechanics_ (Boston: D. Reidel Publishing Company, 1974), p. ix.

11. Michael Dummett, _Truth and Other Enigmas_ (Cambridge: Harvard University Press, 1978), p. 274.

12. _op. cit._, Haack, p. 148.

13. _ibid._, p. 148.

14. Ludwig Wittgenstein, _Culture and Value_ (Oxford: Basil Blackwell, 1980), p. 7.

15. Barry Stroud, "Wittgenstein and Logical Necessity" in _Wittgenstein_, ed. G. Pitcher (Notre Dame: University of Notre Dame, 1968), p. 488.

16. _ibid._, pp. 487-88.

17. _op. cit._, Klenk, p. 43.

18. _ibid._, p 43.

19. _op. cit._, Stroud, p. 478.

20. _ibid._, p.p. 484 and 493.

21. _ibid._, p. 496.

22. _ibid._, p. 496.

23. Wesley Salmon, _Space, Time and Motion_ (Belmont: Dickenson Publishing Co., Inc., 1975), p. 20.

24. _ibid._, p.p. 17-18.

25. _ibid._, p. 20.

26. _op. cit._, Hallett, p. 698.

27. Gordon Baker and Peter Hacker, _Wittgenstein: Understanding and Meaning_ (Oxford, Basil Blackwell, 1980), p. 617.

28. _op. cit._, Sklar, p. 58.

29. _ibid._, p.p. 58-59.

30. _ibid._, p. 255.

31. _ibid._, p.p. 255-56.

32. Olivier Costa de Beauregard, "Time in Relativity Theory: Arguments for a Philosophy of Being," In _The Voices of Time_, ed. J.T. Fraser (Amherst: The University of Massechusetts Press, 1981), p. 429.

33. _ibid._, p. 428.

34. _ibid._, p. 429.

35. _op. cit._, Hallett, p. 767.

36. _op. cit._, Klenk, p. 2.

37. _ibid._, p. 2.

38. _op. cit._, Wittgenstein, p. 44.

39. op. cit., Sklar, p. 119.

40. _ibid._, p. 117.

41. _op. cit._, Haack, p. 39.

42. T.J. Gross, _The Russell-Poincare Debate_, Unpublished M.A. thesis,

McMaster University, p. 53.

43. Crispin Wright, Wittgenstein on the Foundations of Mathematics (Cambridge: Harvard University Press, 1980), p. 71.

44. Cora Diamond, "Wright's Wittgensetin," The Philosophical Quarterly, 31, 125, p. 365.

45. ibid., 365.

46. ibid., p. 365.

47. ibid., p. 365.

48. ibid., p. 365.

49. "Science and the Citizen," Anon. column, Scientific American, 246, 6, p. 81.

50. op. cit., Baker and Hacker, p. 568.

51. N.R. Hanson, "Quantum Mechanics, Philosophical Implications of," Encyclopedia of Philosophy, First Edition, v. 7, p. 46.

52. Thomas Kuhn, The Structure of Scientific Revolutions (Chicago: The University of Chicago Press, 1970), p.p. 54-46.

53. ibid., p. 48.

54. op. cit., Klenk, p. 42.

55. op. cit., Sklar, p. 98.

56. ibid., p. 98.

57. ibid., p. 122.

58. op. cit., Hughes, p. 212.

59. ibid., p. 212.

60. op. cit., Haack, p. 8.

61. ibid., p. 8.

62. W.V. Quine, Philosophy of Logic (New Jersey: Prentice-Hall Inc., 1970), p. 81.

63. ibid., p. 81.

64. ibid., p. 84.

65. _ibid._, p. 84.

66. _ibid._, p. 85.

67. _ibid._, p. 87.

68. _ibid._, p. 87.

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