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EMPIRICAL FAITHFULNESS AND TYPICALITY:

A PRAGMATIC READING OF EVERETT'S PURE WAVE MECHANICS

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"Omne possibile exigit existere."
Gottfried Wilhelm von Leibniz

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INTRODUCTION

Hugh Everett III is universally known as the father of *Many Worlds Interpretation* (hereafter, 'MWI') of Quantum Mechanics (hereafter, 'QM').

Initially the MWI was considered as science fiction or total absurdity, however over the last decades, Everett's work has been strongly re-evaluated, becoming Oxford's 'favorite' interpretation of QM.

Oxonian supporters try to defend Everett interpretation mostly by demonstrating how it can give a precise, elegant, structured account of physical reality, no matter how counterintuitive, because "ultimately, that a theory of the world is 'unintuitive' is no argument against it, provided it can be clearly described in mathematical language. Our intuitions about what is 'reasonable' or 'imaginable' were designed to aid our ancestors on the savannahs of Africa, and the universe is not obliged to conform to them" (Wallace 2010a, p. 69).

The majority of Everettian advocates feel comfortable with the proliferation of worlds, pointing to the advantages of living in a *multiverse* rather than in a single, illegible world, in which the radical dualism between classic, macroscopic reality and quantum strange microscopic entities are never solved.

Today the trend is to read PWM realistically, up to its most incredible consequences.

There are, it seems, good perspectives for future development of Everettian theory, if one accepts the idea of radical, global multiplicity [1]. Wallace expresses this optimistic view in refferring to those "Everettian cautious about the 'many-worlds' label" saying: "come on in, the water's lovely" (Wallace 2010a, p. 71).

But what was Everett's original task?

And to what extent is it possible to attribute to Everett the idea of multiverse or multiple worlds?

This is the first motivation of the present study, which is devoted mainly to the

[1] See the very latest developments in Hall, Deckert and Wiseman (2014).

reconstruction of Everett's original ideas and from which a justification of a new reading is proposed.
Everettian literature divided into supportive and detractive. This division (more or less) implicitly takes for granted that if one advocates an Everettian version of QM, then one is opting for a *realist*, even metaphysical, interpretation of Everett's original formulation, that is, his Pure Wave Mechanics (hereafter, 'PWM').
On the other hand, if one does not believe in MWI, it is due to a strong commitment of anti-realism. And this is a common view for both factions.
In other terms, even if Everett himself never proposed a metaphysical translation for his PWM, it is considered to be a matter of fact that believing in this signifies commitment to some sort of metaphysics of pluralism.
I argue that there is another possibility, and this was precisely Everett's original position.
The first chapter is devoted to reconstructing the genesis of Everettian ideas and their circulation among the most prominent physicists and philosophers.
Everett came face to face with the mathematical contradiction between the linear evolution of the Schrödinger equation and the discontinuous collapse dynamics, brought about by a measurement, during his years at Princeton. His primary task became that of formulating a solution for the so-called *measurement problem.* Neither the von Neumann-Dirac formulation or Bohr's solutions were satisfactory, both assumed a substantial and irreducible dualistic nature of quantum dynamics. The fundamental idea seemed to be the intrinsic dualistic division (the von Neumann and Heisenberg *cut*) between a classic and a quantum world, which implied an unacceptable reduction of quantum entities to the classical level of the observer, because it is the observer who gives, *via* measurement and collapse, reality to quantum states.
In 1954 Everett started working on his dissertation, aimed at finding a universal account for QM, without postulating any sort of discontinuous change. Everett tried to respond to a simple question: why not use mathematics to reveal the world, instead of assuming that the world (a well defined portion of world) should model the theory?
This is an old assumption, of course, and Everett clearly refers to a certain kind of theory (from Copernican theory to Einsteinian Special Relativity) that takes the explanation of perceived reality to be 'revealed' and 'decoded', rather than offering a mere description of it: on more than one occasion Everett ascribes his theory to this conceptual framework, stressing how science should always prove

itself with the discovery of new phenomena.
His theory was simply QM taken with no extra postulates, to its strongest consequences: but if formalism is taken without positing any kind of reduction or mysterious jumps, this means that only the deterministic and linear evolution of superposed initial states exists. Superposition is maintained even after measurements, and probabilities are obviously no longer the reflection of some ontological and objective feature of quantum world. Furthermore, all systems obey the same quantum mechanical law, because they are constantly correlated (entangled) to one another, regardless of their size.
For Everett this was the most natural way of solving the measurement problem, and it was also the most natural way of conceiving the law which governed the real world, because if QM is valid (and it has been richly demonstrated that it is) it should be applicable to all systems, with no restrictions.
All restrictions, which in the end were resumed in the 'principle of complementarity' seemed to derive from the anthropocentric assumption by which the (macroscopic) observer had a special *status*, whereas for Everett no matter the size of the system, the *status* of observer was only a relational, temporary role.
Of course, there were important consequences: if superposition characterizes the real, global quantum state, then all possible outcomes exist simultaneously, in a way or another. The strange quantum jump was introduced in order to explain singularity in observations, an even if contradictory, the multiple existence of different outcomes collides with the strength of evidence, which testifies determinate outcomes after measurements.
John Wheeler, who was Everett's supervisor, found his ideas brilliant, and useful for applications in quantum gravity.
But Wheeler revered Bohr as his mentor, and he wanted positive feedback from him. It will be shown how the strong rejection from the 'Copenhagen Monocracy' contributed to oblivion of Everett's theory, and its author's progressive abandonment of work on foundations of QM[2].
Princeton awarded Everett his PhD in Physics in April 1957. The presented version had been cut and condensed, after Copenhagen disagreement. But by that time, Everett had been doing top secret operations research for a year at the Pentagon's WSEG. He needed the doctorate to keep his job, so he had no choice but to acquiesce to his supervisor's editorial demands.

[2]See Byrne (2010b) for a brilliant biographical reconstruction.

Everett's 'amended' dissertation was published in 1957 as "Relative State formulation of Quantum Theory"; the world of physics remained largely silent.
Everett's attitude, reflecting his personality, was contradictory: on the one hand, he was committed to a robust form of conservatism, in which mathematics is the instrument for interpreting, and explaining, our perceived reality. This being the reason that standard QM is unacceptable: the 'exotic' (von Neumann and Bohr's) cut between the reality of classic and macroscopic dimensions, and indeterminacy of quantum entities was defined a "philosophical monstrosity".
But, on the other hand, even if the only tenable justification of quantum world turned out to be a deterministic, linear, explanation of quantum phenomena, the price to pay was a theory based on superposition principle, and to the extension of the most genuine quantum feature, i.e., entanglement, to all physical world.
So the resulting picture was a mixture of a deterministic conservative theory and a globally extended quantum account of reality.
Without further elements for interpreting this dichotomy, critics moved toward the direction of filling in the apparent gaps: there must be a missing piece, and it should have the form of a metaphysically featured element; for those who wanted to get engaged with "many-worlds" labeling, this was a challenge; for the more standard QM-oriented minds, the idea of multiplicity was science fiction.
It was on this point that interpreters focused on seeking appropriate metaphysics for PWM. And specifically, it was from the search of a metaphysical completion that DeWitt's version of MWI arose, becoming synonymous of Everett's original PWM.
This was something that Everett would never had accepted: even if tolerated in discussion, the metaphysical featuring of PWM was, again, out of the domain of empirical predictions. Indeed, for Everett what can be said about our physical world is always bonded to a rigid form of empiricism, from which the extent of what can be said is established by what can be perceived.
The fact that the evolution of the Everettian critique followed the search for metaphysical completion derives from the singular primary circulation of Everettian ideas, which was tied to Wheeler dictates, and from the succeeding re-evaluation of Everett's work by DeWitt-Graham[3].
This of course does not mean that it is impossible to obtain an appreciable metaphysics starting from a no-collapse, Everettian-modeled QM; this is the case, for

[3]See DeWitt and Graham (1973).

example, of the most recent developments in decoherent versions of MWI (especially the emergent version proposed by Wallace (2010, 2012)).

But it has been pointed out that this kind of operation *does not match* Everett's original purposes.

This is precisely what the first chapter aims to demonstrate through an historical analysis of Everett's published and unpublished works[4].

This study seeks to continue the understanding of Everett's real thoughts regarding physical theories in general, which is in itself a strong commitment to the empiricist view of science, following which his theory appears to be quite different to a metaphysics of plurality.

The picture that emerges from Everett's PWM is surely misleading, at first sight: if the collapse of the wave function is something *mystical*, and its unobservability is simply unacceptable, how can we accept the idea of living in a constantly branching world, if we do not experience anything like such a prominent plurality?

The second chapter aims to show how Everett did not find any strangeness in conceiving the global state of the world as a *branching tree*, because what really counts is the description of perceived, relative state. If the theory is capable of explaining the determinateness of our single outcomes without the introduction of posited elements, then it could be considered empirically adequate.

Everett's empiricism explains how *surplus* structure coming from the global model has to be interpreted: construction of a single, all-embracing theory, applicable to the entire universe derives from the desire of discover "new phenomena, which involves the consideration of inductive inference and the factors which influence our confidence in a given theory (to be applicable outside of the field of its formulation)" (Everett1973a, p. 171).

What the model says about the global state of the world has to be considered under the light of these convictions: all that can be required to a theory is *empirical faithfulness* to our perceived reality, the model being a sort of regulative structure, by which new phenomena could always be revealed.

Everett held a holistic vision of the world: there are continuously interacting systems, whose role depends on the perspective one decides to assume. That is, our own state is always a *relative state*.

It is in this sense that Everett speaks about *subjectivity* of states: the real state is the global state, while our relative state is *locally*, *subjectively* tied to a specific

[4]This kind of reconstruction follows suggestions of Barrett (2008, 2010, 2011a, 2011b, 2012), Osnaghi and Freitas (2009), Bevers (2011).

measurement context.
I argue that in PWM there is a pragmatic criterion of selectivity in choosing which aspect of a theory model has to be isomorphic to our perceived reality, similar to that proposed by Van Fraassen (2008), in his pragmatic reading of structural empiricism, by which the space of a theory is always delimited by their users.
Observers will perceive determinateness and uniqueness of their states, but this is just a *local* perspective: it will appear that only one single outcome occurs, because the state of the observer becomes entangled with the state of the observed system, and probabilities will follow coefficients of the Born rule.
And that is the second problem with PWM: how is it possible to explain probabilities? It has often said that probabilities lose their meaning, in a situation in which all outcomes exist. In the third chapter an analysis of the problem is given.
The latest developments of probability in Everettian context aim to highlight the subjective aspect of statistical assertions: decision-theoretic strategies (Deutsch 1999; Wallace 2003, 2007, 2010) are applied to Everettian agents, whose rationality is defined as the adherence to Born rule in modeling actions.
In other words, an Everettian rational agent (who believes in PWM) will behave as statistical assertions of standard QM were true, for operationalist purposes: he will maximize expected utilities following the Born rule.
Even when an account of genuine objective chance is given in PWM (derived from genuine ignorance of Everettian agents (Vaidman 1998; Saunders 2005, 2008, 2010a)), it is reduced to the subjective level. At the fundamental, global level there are no stochastic features, because all branches exist.
Even if these approaches are very interesting and coherent with Everett's understanding of probabilities as merely subjective, it seems that a *bridging element* between the relative (subjective) and the global state is missing.
In the fourth chapter a solution is given for understanding probabilities in Everettian theory.
For Everett, the apparent uniqueness of observers and their continuity over time could be easily explained again with correlation: it is thanks to correlation that linear trajectory is observed, because systems states are entangled, and a time-wise ordered sequence of states for a system could be defined by the correlation model.
Almost all the linear trajectories over the global branching tree will be expression of *typical* observers, who will exhibit results coherent with standard QM. 'Typical' here is the expression of the *overwhelming majority*.

In this sense probability translates into a counting measure, which expresses branch counting instead of a real stochastic selection of one single state.

Typicality is the recipe for interpreting covariance of statistical assertions with perceived reality, and Everett refers to the analogous classical case: as the Lebesgue measure is the more natural measure for defining trajectories over the phase space, without any probabilistic significance, the same happens in PWM with the square amplitude, which is the only measure which satisfies the *additivity requirement* (as Lebesgue measure satisfies the Liouville's theorem, i.e., conservation of probabilities).

Furthermore, recent developments on the role of typicality in classical statistical mechanics and in measure-theoretic dynamical systems in general seem to confirm that a reading of probabilistic aspects as expressions of typical behavior rather than genuinely stochastic features is possible: the only important characteristic of probability theory is its mathematical aspect, which in Everett gives the basis for a measure of branch counting.

Interpreting, as well as merely understanding, PWM is a challenge.

The present work is devoted to reconstructing Everett's original thoughts and seeing to what extent it could be considered a self-compelling interpretation of QM, without further metaphysical or mathematical insertions.

What emerges is a quantum holistic interpretation of the world, in which the global state is a profound manifestation of the most important quantum element: entanglement. The cost is the abandonment of the anthropocentric privileged absolutisation of the relative state, which is taken as a local, relational manifestation.

Chapter 1

Back to 'original' Everett

Everett's *Relative State Formulation of Quantum Mechanics* is one of the most famous and debated interpretations of Quantum Theory.
Everett's genius lays in his recognition of the limits of standard QM that could be resolved by assuming linear dynamics to be universal, without taking the collapse postulate as an essential part of the universal description; in this way, determinate experiences of observers are treated starting from a phenomenological perspective, as subjective appearances. In other words, Everett PWM is QM taken 'seriously', up to its most extreme consequences, as a good description of the physical world. Everett's theory is the first rejection of the collapse postulate, and most no-collapse interpretations are directly inspired by or devoted to Everett's original theory, in one way or another, however it could be considered from new perspectives in its own right: starting from an integrated historical and philosophical account, the original theory appears in a new look. Here I would like to emphasize that Everett never classified or explained how his theory was to be understood concretely, and this was due in part to the historical contingencies in which the theory developed, but mostly to his indifference to metaphysical and even philosophical debates: for this reason a sort of 'translation' of his account into an ordinary language is not given and without guidelines his theory appeared to be at the least arbitrary. I aim to analyze the historical development of the theory in order to show that Everett was very aware of the sort of problems his theory could encounter, but the fact that he was not concerned about speculative questions was simply due to his strong commitment to a methodological empiricism.

1.1 The theory grows: a universal explanation for the universe

Everett's theory is the first theory to attempt to take QM 'literally', as expressing the actual state of the physical world, without adding extra postulates, or requiring ulterior interpretation. For this reason, it could be considered a *pure* quantum theory, or better still a *solely* quantum theory, and not an interpretation of formalism. As Wallace points out "'Everett interpretation' is just QM itself, read literally, straightforwardly – naively, if you will – as a direct description of the physical world, just like any other microphysical theory." (Wallace 2012, p. 2).

Everett's initial goal was to find a universal quantum mechanical explanation applicable to all physical systems, and ultimately to the entire universe. To do that, Everett kept just that part of the standard quantum formalism expressing the linear dynamic evolution, and considered it to be a complete explanation of physical reality, because up to then all attempts to formulate an acceptable interpretation of standard formalism failed to be universal due to the inclusion of the collapse postulate, which in addition introduced a stochastic element to the explanation[1]. Everett's reasoning seems to start with the main problem of QM, which under these circumstances was a partial theory, namely the *measurement problem*. The difficulties seemed to derive from the observation process, whereby collapse was introduced. Observation was a metaphor of the irrecoverable *cut* between the macroscopic state of observers and measured microscopic objects. For Everett, instead, it was the collapse postulate which seemed to be a tool posted *ad hoc*, introduced to justify the appearance of one singular outcome. For Everett this situation was untenable, because the theory did not explain in itself what it takes to be considered a measuring system and even more so because all physical systems, including measuring devices, are made up of microscopic entities at the end of the day.

Everett assessed the existing quantum interpretations and, as he emphasized in his long thesis, in none of them could he find a 'literal' version of formalism in which collapse was excluded.

The main motivation that led Everett to propose his interpretation, as Everett himself indicates in a letter to Jammer, was "to resolve what appears to me to be

[1] See Saunders (2010a, p. 3): "[...] this is the only place where probabilities enter into quantum mechanics".

inherent inconsistencies in the conventional interpretation. I was of course struck, as many before and also many since, by the apparent paradoxes raised by the unique role assumed by the measurement process in QM as it was conventionally espoused. It seemed to me unnatural that there should be a "magic" process in which something quite drastic occurred (collapse of the wave function), while in all other times systems were assumed to obey perfectly natural continuous laws". (Everett 1973b). It was unacceptable for Everett that something magic, or mystical[2] could occur during a measurement, and this strong rejection is repeated in his works.

There are two important events that probably inspired Everett in his work on QM: the course in 'Methods of Mathematical Physics', taught by Eugene Wigner, and Bohr's public lecture, both took place in 1954.

Wigner used to question the standard interpretation in his classes. As Byrne remarks "there is little doubt that he had been discussing the problem with his students for many years"[3]. As evidence of Wigner's influence there is a handwritten draft of his dissertation in which Everett outlines Wigner's friend's dilemma; moreover, a version of Wigner's friend's story is the starting case from which Everett develops his ideas in his long thesis[4].

During his lecture Bohr explained his principle of complementarity. Talking with his young colleague Charles Misner and Aage Petersen, who was Bohr's assistant, Everett recalled that his theory had crystallized during a drunken conversation in his room. Everett himself reported this fact in a conversation with Misner: "…It was because of you and Aage Petersen, one night at the Graduate College after a slosh or two of sherry, as you might recall. You and Aage were starting to say ridiculous things about the implications of QM and I was having a little fun joshing you and telling you some of the outrageous implications of what you said…" (transcript, Everett and Misner 1977b).

Apart from nice anecdotic issues, the quotation underlines Everett's feeling of disagreement with standard interpretations of QM. This is also confirmed in a letter to Jammer, in which Everett reports, in a more formal language, the same ideas (Everett 1973b).

Starting from the measurement problem, Everett felt the need to find a universal

[2] See Everett's letter to DeWitt (1957d) as another example of emphasis on 'mystical' aspect of observation in standard interpretations.

[3] See Barrett and Byrne (2012, p. 12).

[4] See the draft and the other original manuscripts at UCISPACE archive (http://ucispace.lib.uci.edu/handle/10575/1060).

explanation which would embrace even observers treating them as physical systems.

Everett was inspired by conceptual and theoretical worries: his first aim was to *model* observation as a normal, ordinary event, and to treat all physical systems in the same way and with the same dynamical law: QM, with its predictive power, has to be considered a universal law, because all physical systems are ultimately constituted by particles governed by quantum interactions. A good and satisfactory physical theory should be applicable to the entire universe, and QM makes no exception.

1.1.1 The measurement problem

As mentioned, Everett firstly focused on the measurement problem.

Measurement was explained by the dissipation of superposition, and collapse postulate made its entrance into the theory in order to explain why we actually see just one determinate outcome.

Firstly exposed as in its controversial implications by von Neumann (1932, transl. 1955), the measurement problem entailed the irreconcilability of a quantum mechanical description of reality with the classic account of our ordinary physical world. Everett did not accept the limits of quantum mechanical explanations for the measurement problem: he felt that classical-textbooks versions of QM, which in the end derived from both Copenhagen interpretation and von Neumann-Dirac description of the measurement problem, were worrying.

On the one hand Bohr's explanation gave no reason for this special treatment of macroscopic objects[5].

The principle of complementarity was, using Everett's words, a "philosophical monstrosity". Indeed, for Everett the fact that a physical theory aims to describe a part of the world which is not *actually* real was at the very least nonsense. In a letter to DeWitt he wrote:

> The Copenhagen interpretation is hopelessly incomplete because it's a priori reliance on classical physics (excluding in principle any deduction of classical physics from quantum theory, or any adequate investigation of the measuring process), as well as a philosophical monstrosity with a "reality"

[5] See Faye (2014) for a more analytical description of Copenhagen Interpretation.

> concept for the macroscopic world and denial of the same for the microcosm. (Everett 1957d).

On the other hand von Neumann's description of the measurement problem firstly recognized[6] the "peculiar dual nature of quantum mechanical procedure which could not be satisfactorily explained" (von Neumann 1955, p. 417), by which after a measurement takes place we no longer have a linear superposition of entangled states of measured object-measuring apparatus.
That is, the reduction of the wave packet expressed in a collapse.
Both the standard von Neumann-Dirac and the Copenhagen formulations encountered different but related difficulties. While with the first account the collapse of the wave function is simply posed, and we cannot explain how measurement devices are to be treated within quantum explanation, with the latter we are obliged to stipulate that measurement interactions can be understood by supposing that a classical measuring device has a special *status*, our reality being dramatically divided into two irreconcilable parts. For this reason the standard formalism fails to provide a quantum mechanical understanding of the measurement process: our classical world seems to be the 'true' reality, and deserves a classical explanation, while the quantum world simply does not gain existence until a measurement occurs. For Everett, both formulations lead to the same contradictions.
The main problem arises from the understanding of the *status* of the observer during the measurement process: if we think of a measurement as a *cut* between a classical object and a quantum mechanical element, then we are faced with the difficulty of understanding what an observer really is, because we cannot use the same quantum description for both elements of an observation.
Both in his long thesis and the short version Everett uses von Neumann language, i.e. *external observation language*[7].

[6] See Krips (2010): "By embedding the quantum mechanics within the formal theory of Hilbert spaces, von Neumann provided the first rigorous axiomatic treatment of QM by it within the formalism of Hilbert spaces. He also allowed a new conceptual architectonic to emerge from within the theory itself rather than following Heisenberg, Bohr, and Einstein who imposed a system of concepts a priori. Von Neumann also intervened decisively into the measurement problem. Summarizing earlier work, he argued that a measurement on a quantum system involves two distinct processes that may be thought as temporally contiguous stages".

[7] Note that the external observation formulation is also used in a quantum cosmological context (see for example Smolin 2008), in which a single quantum state (or a single Hilbert space) for the universe is not a sufficient description for a close system. In this context, complete information is given by the sum of partial and incomplete, but mutually compatible, information of quantum state descriptions of all the possible observers (the boundary between the part of the universe that is considered the observed system and the part that is considered the observer may be chosen arbitrarily, so the split is relativized). This is a class of interpretations based

For von Neumann "we therefore have two fundamentally different types of intervention which can occur in a system S" (von Neumann 1955, p. 351); Everett refers to Process 1 and Process 2, using von Neumann's words.
Process 1 indicates "the arbitrary changes by measurement" (von Neumann 1955, p. 351). Everett describes it as "the discontinuous change brought about by the observation of a quantity with eigenstates φ_1, φ_1, in which state φ will be changed to state φ_j with probability $|(\varphi, \varphi_j)|^2$" (Everett 1973a, p. 3). Process 2 is described as "the automatic changes which occur with the passage of time" (von Neumann 1955, p. 351).
Everett reports: "The continuous, deterministic change of state of the (isolated) system with time according to a wave equation $\frac{\partial\varphi}{\partial t} = \bigcup \varphi$, where $\cup$ is a linear operator" (Everett 1973a, p. 3).
The first process describes a measurement and implies a random collapse, while Process 2 describes the deterministic evolution of a system unless a measurement occurs.
The 'measurement problem' arises from the conflict between these two dynamical laws, and this incompatibility reflects the aforementioned 'cut' between the quantum mechanical description of observed systems on one hand, and observers, that are treated externally, on the other hand[8].
Under these respects, a measurement should be considered as a *special event*, which represents a final division between our macroscopic classical world and the quantum microstate, and furthermore it introduces a stochastic account in physical explanation.
As Saunders has well resumed, the collapse cannot be seen as something *primitive*, or inevitable, because it is under-specified: the time is undefined, and the criteria for the kind of collapse are "strange" (Saunders 2010a, p. 3).
For Everett the main problem however is concerned with the connection between observers and the theory. Observers are treated as external to the measurement process, and problems related to this kind of arbitrary cut are even more evident

on Everett which give a relational account of quantum mechanics (Rovelli 1996) mostly applied to quantum cosmology. It must also be noted that Everett himself refers to a real global state, intending a close state (an isolated state) for the universe, but he never declined this into a detailed description of how conceiving it in practice (in other words, conceptually speaking, the only isolated state is thought for the only close system, the universe, for which, conceptually there cannot be an external observer).

[8] See Barrett (2010, p. 226): "if we suppose that measuring devices are physical systems like any other, then the standard collapse theory is inconsistent because the incompatible laws might be applied to the same evolution; on the other hand, if measuring devices are somehow special, the standard theory is incomplete since it does not tell us what interactions should count as measurements".

if the existence of more than one observer is allowed.
But if we want QM to provide a universally valid explanation we should consider a situation including one or more, rather than one, observers, because that is the constitution of the universe. Exteriority of observers should be applied to observers with respect to their role, which is *relative*, and not with respect to the theory: while Everett uses external observation formulation, he simply refuses to treat observers themselves as external to the theory: he allows that a system has *occasionally* the role of observer, regardless to his size[9].

1.1.2 How to model observations: Everett's solution

In order to avoid this incompatibility, Everett starts from a new account of measurement that does not postulate any sort of discontinuous change, because every object is treated as internal to the theory. After a short revision of the alternatives to the measurement paradox, none of which could be considered tenable, Everett introduced his own alternative[10]:

> *Alternative 5:* To assume the universal validity of the quantum description, by the complete abandonment of Process 1. The general validity of PWM, *without any statistical assertions*, is assumed for all physical systems, including observers and measuring apparatus. Observation processes are to be described completely by the state function of the composite system which includes the observer and his object system, and which at all times obeys the wave equation (Process 2). (Everett 1973a, p. 4).

His theory has some important virtues:

[9] As Barrett points out: "the problem with the standard collapse theory, according to Everett, was that it required observers always to be treated as external to the system described by the theory, one consequence of which was that it could not be used to provide a consistent physical description of the universe as a whole since the universe contains observers" (Barrett 2012, p. 5).

[10] Everett provides an overview of some alternatives to the measurement paradox at the beginning and comments them in the 'Discussion', at the end of the long thesis.

- *Universal validity*: the existence of more than one observer is allowed; this also implies *completeness*, because Everettian QM is applicable to the entire universe, since there are no isolated systems (apart from universe itself) [11];

- *Psycho-physical parallelism*: every object could be considered a good observer depending on its relative position, regardless of whether he is human or a servomechanism[12];

- *Deduction of classical phenomena*: our world is not to be divided into classic and quantum, only the first receiving the *status* of reality[13], because reality itself is composed by microscopic entities;

- *Simplicity*: no extra postulate or *ad hoc* assumptions have to be posted[14];

- *Determinism*: Everettian QM ensures a deterministic explanation because the only stochastic element, that is the collapse postulate, is unnecessary[15].

[11] At the end of the long thesis Everett refers to the "popular interpretation" as the position by which the wave function expresses the objective features of the system, but, because its state discontinuously changes under observation, it could be assumed just starting by a 'solipsist position', since only if there is just one observer the system's dynamics could be explained without introducing the collapse postulate. On the other hand Everett's theory has the great advantage of explaining the linear dynamics without reducing to a solipsism, because the only isolated system is taken to be the whole universe (Everett 1973a, p. 110).

[12] Everett refers to the psycho-physical parallelism in commenting Bohr's interpretation: "And to draw the line at human or animal observers, i.e., to assume that all mechanical apparata obey the usual laws, but that they are somehow not valid for living observers, does violence to the so-called principle of psycho-physical parallelism, and constitutes a view to be avoided, if possible To do justice to this principle we must insist that we be able to conceive of mechanical devices (such as servomechanisms), obeying natural laws, which we would be willing to call observers" (Everett 1973a, p. 7).

[13] Everett clearly refers to Copenhagen interpretation. See for example the end of the long thesis, in which Everett criticizes the "strong reliance upon the classical level from the outset, which precludes any possibility of explaining this level on the basis of an underlying quantum theory. (The deduction of classical phenomena from quantum theory is impossible simply because no meaningful statements can be made without pre-existing classical apparatus to serve as a reference frame.) This interpretation suffers from the dualism of adhering to a "reality" concept (i.e., the possibility of objective description) on the classical level but renouncing the same in the quantum domain" (Everett 1973a, p. 111).

[14] This is referred to 'hidden variables' theories. It could sound strange prima facie that Everett is adopting something like Occam's razor, since it is one of the first and more frequent objections that his theory had received (for example see DeWitt 1957), but in his opinion, proliferation of non-necessary entities has to be read respect to the theory, and not in what the theory could 'create' in a metaphysical sense. See the next chapter for a clarification of everettian methodological empiricism.

[15] Everett (1973a, p. 114) refers to 'stochastic interpretations' (Bopp). He also intends that determinism is not a necessary requirement, but because his interpretation ensures a deterministic account, it is also preferable (a discussion of everettian requirements for physical theories in general will be addressed in the next chapter).

Everett was thinking of a *unitary* explanation, with which one can explain both macroscopic and microscopic reality, without adding extra-postulates through which the theory would become partial. Such a description entails that observers are treated as physical systems, independently of their own nature, and observation, *via* measurements, does not represent a special event. Measuring systems have to be treated quantum mechanically, because if the theory does not provide a different account for observers while positing it, it will lose its explicative power. In this sense, there are just *interconnected* systems, and the state of an isolated system is simply nonsense. So the entire universe could be explored with the same wave mechanics. Determinism follows as a consequence of having removed this special *status* of observation (namely the stochastic element).

This means that if one wants to formulate a good physical theory, one should be motivated by the purpose of finding a universal explanation, which is characterized by all-encompassing: physics is nothing other than a description of reality, meaning reality in its totality; so if one wants to formulate a good physical theory, then all physical systems have to be described by the same theory. QM in this respect has to be taken as universal, otherwise we would accept a drastic division between a classic and a quantum world, a division that implies ontological and epistemic separation.

Classic external observation has to be rejected, because there is no such thing as an isolated system: the universe is composed of interacting and connected systems. So, if a system interacts with a quantum object, it has to be treated within the same framework: this implies that during an interaction an observer system becomes *entangled* with the observed system, and, following linear dynamics, what one obtains is a superposition of states, each expressing a possible outcome.

Entanglement is one of the basis of Everett's theory: composite systems are always entangled, and "by its very nature, entanglement can arise only in composite systems – those that consist of two or more parts" (Barbour 2000, p. 222).

If one applies entanglement to the entire universe, and if one takes, as Everett does, both observers and observed elements as described only by the deterministic linear law, without postulating any sort of cut during the measurement, then one obtains a universal explanation in which collapse disappears[16].

[16] See Barbour (2000, p. 222): "an essential element of the many-worlds interpretation as it is now almost universally understood is that the universe can and must be divided into at least two parts – an observing part and an observed part. However, Everett himself looked forward to the application of his ideas in the context of unified field theories, 'where there is no question of ever isolating observers and object systems. They are all represented in a single structure, the field"'.

So *entanglement* and *universality* are the simple ideas at the very basis of everettian QM.
Everett drops the collapse postulate, Process 1, from the Standard Formulation of QM, then he deduces the empirical predictions of the standard collapse theory as the *subjective experiences* of observers who are themselves treated as physical systems described by the theory. He calls his theory *Pure Wave Mechanics* (PWM), because the dynamic evolution of the involved systems follows the linear evolution as described in Process 2.
This implies that the *superposition principle* is maintained even after measurement, because "in general a composite system cannot be represented by a single pair of subsystem states, but can be represented only by a superposition of such pairs of subsystem states" (Everett 1973a, p. 9).
We can describe S_2 as a measuring device M, while S_1 is a measured object. Then the evolution of a composite system $S + M$, where system S is measured by a system M, will be described by:

$$\sum_i a_i \phi_i^S \varphi_0^M \rightarrow \sum_i a_i \phi_i^S \varphi_i^M = \Psi^{SM}.$$

We look at two systems, S_1 and S_2, with associated Hilbert spaces H_1 and H_2, as a unique composite system S, whose Hilbert space can be written as a tensor product of $H_1 \otimes H_2$. So the state of S can be written as a superposition of orthonormal sets of states $\left\{\xi_i^{S_1}\right\}$ and $\left\{\eta_j^{S_2}\right\}$ for S_1 and S_2, respectively [17]:

$$\Psi^S = \sum_{ij} a_{ij} \xi_i^{S_1} \otimes \eta_j^{S_2}$$

Although S is in a definite state, S_1 and S_2 do not possess a definite single state[18].
We can describe S_2 as a measuring device M, while S_1 is a measured object. Then the evolution of a composite system $S + M$, where system S is measured by a system M, will be described by:

$$\sum_i a_i \phi_i^S \varphi_0^M \rightarrow \sum_i a_i \phi_i^S \varphi_i^M = \Psi^{SM}.$$

[17] I am using formalism of the short thesis (Everett 1957b, p. 143).
[18] Everett remarks "except in the special case where all but one of the a_{ij} are zero" (Everett 1957b, p. 143), maintaining the eigenvalue-eigenstate link.

which is not a state that describes an observer having one particular result[19].
The situation is always the same, for all kinds of interactions, regardless of the size of the object.
There is nothing special in quantum measurement, because there is nothing special in the quantum world. Instead of thinking of a measurement as a special event, Everett takes linear wave mechanics as a complete description of the real state of a physical system, while collapse serves for phenomenological explanations, with probabilities expressing subjective appearances. So collapse is confined to a subjective level, in a relational context. This means that it could be considered as a valid explanation of a contextualized situation, and just for practical and operationalist purposes, but it does not translate into the only reality, to the extent that reality is to be taken as completely expressed by universal wave function. So collapse, which is the source of all evils, is just a mathematical tool, an operationalist instrument. Our experiences then have to be deduced in some way from the universal state:

> Since the universal validity of the state function description is asserted, one can regard the state functions themselves as fundamental entities, and one can even consider the state function of the whole universe. In this sense this theory can be called the theory of the "universal wave function", since all of physics is presumed to follow from this function alone. There remains, however the question as to whether or not such a theory can be put into correspondence with our experience. (Everett 1973a, p. 6).

This means that collapse should be interpreted simply as a subjective experience, and the state of a measured object, together with the state of the observer is a *relative state*, while the global state is expressed by the universal wave function.
Everett introduces the 'Relative State' formulation to explain how it is possible to see just one of the superposed outcomes, all possibilities being equally real. The relative state expressed by the singular outcome is a phenomenological appearance, or better still, an epiphenomenon. Reality as it is perceived is just the expression of correlations, and we can look at it from different, interconnected experiences; but, at a universal level what matters, what counts as fundamental, is the universal state. So the relative state is only an aspect of the fundamental level, but globally

[19] It needs an important remark here: it has been noted that, even if there is no mention of it, in this form of notation there is an implicit use of Ehrenfest's theorem (even if Everett made no mention of it), which is the basis of decoherence (Saunders 2010a, p. 39). Decoherence will be discussed at the end of the present chapter.

the *symmetry* is maintained. In order to give an account of our predictions, the theory should provide a mechanism through which we can justify our experiencing determinate records: if we want to accept the theory as a universal account of the world, starting from where we look at our state as a partial, relative state, we should ensure that our results are confirmed by the predictions of the theory.
So the theory should also provide an account from which we can construct our relative states and in which we can find ourselves as experiencing our determinate outcomes.
Using the above formalism, we can always deduce the relative state of a system S_2 once we choose a state for S_1: this means that starting from the state of the composite system S we simply choose a state for S_1, for example ξ_k, and then we assign a relative state to S_2:

$$\Psi\left(S_2;\ rel\ \xi_k,\ S_1\right) = N_k \sum_j a_{kj} n_j^{S_2},$$

where N_k is a normalization constant[20].
It follows that we can always represent the composite system S as a *single superposition* derived from the choice of one relative state for the components S_1 and S_2:

$$\psi^S = \sum_i \frac{1}{\mathrm{N}_i} \xi_i^{S^1} \psi(\mathrm{S}_2;\ \mathrm{rel}\xi_\mathrm{i},\ \mathrm{S}_1).$$

This implies that the state after a measurement is always superposed: using Everett's words, the state "splits" in different states, each of which expresses a correlated observer-observed object state with an apparently determinate state. But there is nothing like a determinate state at this level, because the only determinate state is the final superposition. Rather than a linear trajectory, the final state is represented by a "branching tree".
Relative state formulation provides a good explanation for our outcomes, but as Everett himself noted "this indefinite behavior seems to be quite at variance with our observations, since physical objects always appear to us to have definite positions" (Everett 1957b, p. 457).
It is important that the theory could explain why a *typical* observer, with any sort of memory configuration, perceives a determinate record, and remembers it, in situations in which other measurements are performed, or the same measurement is repeated. The superposition principle implies that each element is in an

[20] Everett specifies that "this relative state for ξ_k is independent of the choice of basis $(\xi_k)\,(ik)$ for the orthogonal complement of ξ_k alone" (Everett 1957b, p. 456).

eigenstate of the observation, and the observer-system state describes the observer as having the perception of that particular observed system-state. Furthermore, a *typical* element of the final superposition appears to perceive an apparently random sequence of definite results for the observations, and also that his memory states are correlated, i.e. he possesses a *memory configuration* in which the earlier memory coincides with the later.

Probabilistic assertions of Process 1 appear to be valid for a *typical* element of the final superposition.

To a certain extent it can be said that the principle of complementarity is inverted. Human knowledge is surely limited to collapse postulate meaning that superposition cannot be experienced in its totality, so collapse is a phenomenological explanation in this sense. Our knowledge is still classical, simply because all the objects that we see have classical features; they are, nonetheless, in superposition, but we cannot see that. In this light, looking at the world through classical 'glasses' means just looking from a partial perspective, namely focusing on one specific outcome, the fundamental level being the final wave function[21].

With PWM the measurement problem is resolved, because all the systems involved in the measurement process follow the same law for the dynamical evolutions. But in dropping the collapse, the standard explanations for determinate measurement records and usual quantum statistics are untenable. The determinate-record and probability problems, respectively, are the problems of providing replacement explanations for determinate measurement records and quantum statistics in PWM without appealing to Process 1.

1.1.3 Traditional problems with Pure Wave Mechanics

It has often been said that at least *prima facie* Everett encountered challenging difficulties, a classic Everettian problem became: the *determinate record problem* and the *probability problem*, which resist up to the last accounts of the Everett theory, but solutions do not find *consensus* among physicists or philosophers.

The determinate record problem addresses the difficulty in finding an explanation for one specific outcome, when the theory entails all outcomes being equally real

[21] See ''Macroscopic objects and classical mechanics" and "Amplification process" (Everett 1973a, p. 86 and 90) for a more detailed explanation of how our knowledge is to be intended as 'classical'; see also Saunders (2010a, p. 8) for a brief introduction to decoherence in PWM, which is the last trend in interpreting how classic macroscopic world emerges from quantum microphysics (decoherence will be discussed later in detail).

(apart from the arbitrary choice of one specific quantity to be measured, and a relative specific decomposition of the general superposition, i.e. the *preferred basis* choice, which has been solved with decoherence[22]), while the probability problem addresses the difficulty of interpreting the meaning of probability in a context in which all possibilities occur[23].

Everettians have always moved in the direction of filling in the missing details, giving rise to a large variety of 'interpretations of Everett's interpretation'[24]: the 'bare theory'[25], 'splitting worlds'[26], 'decohering histories'[27], 'relative facts'[28], 'single minds' and 'many minds'[29], 'many threads'[30] and the latest developments of decoherence and emergent worlds[31]. Such interpretations of PWM are interesting in their own right, but they do not necessarily blend well with Everett's own views. These approaches became the history of Everett's own view, and lately they appear to be very different from the original recipe.

As Barrett points out[32], there are significant reasons to distinguish Everett's own interpretation, or theory[33], of QM from other interpretations that generate from it, because Everett's interpretation is distant from the overwhelming majority of the theories that inspired to him.

I feel that the main problems derived at the beginning in a certain vagueness that affects speech in the *splitting process.* When a measurement occurs, superposition implies that there will be different entangled states for the observed system and

[22] See Barrett (1999) and Butterfield (1996) for a description of the preferred basis problem. From the 1980s onwards decoherence theory is considered the solution to the problem of definiteness (i.e., the definition of the decomposition of the wavefunction). The basic idea is that dynamical processes imply emergence, rather than specification a priori, of a particular basis (interference between processes described by separate terms of the decomposition is negligible). See Wallace (2003, 2010, 2012), Bacciagaluppi (2012) and Saunders (2010a) for more details on decoherence.

[23] See Barrett (1999), and Bacciagaluppi (2013) for a detailed discussion.

[24] It was common idea that Everett interpretation 'stands for an interpretation' (see for example Barrett, 1999).

[25] See Geroch (1984), Albert and Loewer (1988), Albert (1992), Barrett (1999).

[26] See DeWitt (1970).

[27] See Gell-Man and Hartle (1990).

[28] See Saunders (1995, 1996, 1998).

[29] See Albert and Loewer (1988), Lockwood (1989).

[30] See Barrett (1999).

[31] See Zeh (1970), Zureck (1991), the aforementioned Gell-Man and Hartle (1990) and the Saunders-Wallace approach (Saunders 2010a, Wallace 2010a, 2012).

[32] See Barrett (2011a, p. 1).

[33] Wallace makes a distinction between the term 'interpretation' and 'theory', advocating that the former has had unfortunate consequences in the philosophy of physics, contributing to the idea that there is one theory and many possible interpretations, limiting the discussion to the research of the correct one. This is not the case for everettian QM, which is QM "taken literally" (Wallace 2012, p. 3).

observer: so the unique initial state splits into different states, following a ramified (like a 'branching tree') trajectory.
Everett did not characterize in a philosophical sense the splitting process: what is missing I believe is a clear and explicit translation of branching process into a more detailed and accessible language. Everett says nothing about how to feature the splitting process: by which I mean that we do not really know what kind of entities are involved, and what kind of ontological nature we should ascribe to them; this is the underlying problem from which determinate record and probability problems arose (and this will be precisely the problem I will analyze in the next chapters). Of course, the fact that Everett never claimed to have deduced the existence of splitting worlds from a version of PWM does not mean that it is impossible to do so. But whether and to what extent such a deduction might be possible depends on what metaphysical assumptions one takes to be properly included in a full statement of PWM. This is how the story went from the beginning and throughout the following thirty years.
More generally it is not clear how the connection between the theory and what the theory says about how the world is to be interpreted. Sceptical visions underlined how the connection of formalism with reality was not at all obvious, because a mere mathematical tool (i.e., the mere formalism) has nothing to do with reality itself. In the formalism there is no trace of this connection and this is the reason why a lot of work has been done in the direction of finding the missing piece of Everett's interpretation.

1.2 Historical analysis of the reception of PWM: from oblivion to rediscovery

In this sense Everett's interpretation has been misunderstood for years. As Harvey reported to Everett (Harvey 1977), Jammer called it "one of the best kept secrets of this century".
I believe that a new reading of Everett is necessary, and in some respects it is 'in the air': a 're-reading' of the genuine original account, that I will from now on call *the original core*, that consists of bare formalism (PWM, with the rules for relative states construction), the deductions of determinate-records and probabilities, namely our classic perceived reality, and Everett's own strong convictions about the requirements and standards that make a physical theory a good theory,

recently fortified by new documentation, that sheds light on how the scheme is to be understood.

The fact that this kind of analysis is still *in nuce* derives from the vicissitudes related to the historical development and circulation of Everettian ideas, and to Everett's apparent indifference to most theoretical (and sometimes metaphysical) problems.

This kind of approach is in an embryonic stage, and not yet crystallized, but I feel that it should became a methodological prerequisite. Indeed a deeper reading of Everettian assumptions explains at least why he considered them self-compelling and complete.

Reconstruction of Everett's original thinking is important for two reasons: finding the *original core* should be considered an essential starting point. Furthermore, the *original core* now reconstructed might appear to be astonishingly interesting in its own right, as well as self-contained in some important elements.

Everett's theory has been rejected, disdained and forgotten for thirty years.

The most common opinion was that his theory was just science fiction. It is only since the late eighties that the debate on Everett's theory has been reignited receiving wide *consensus*. The recent discovery of the "basement treasure"[34], consisting of unpublished notes, correspondence and manuscripts, has allowed a deeper and more complete reading of Everett's theory, and a better understanding of the theoretical framework. Thanks to this recent and illuminating discovery we can now see Everett's reaction to the rejection of his theory, but also clarify how even though Everett abandoned QM, he continued to considered his theory a complete theory up to the time of his death.

This new perspective, together with Everett's empiricist account of scientific theories in general may be useful in understanding why Everett advocated completeness for his theory. This would be an integrated historic-philosophical approach which I assume is very important in understanding PWM, because it will be clear that his empiricism was a very strong assumption.

I will develop the consequences of the new starting point in the next chapters, while in the present chapter I want to underline how metaphysical speculation, which was the very first reading and which to some extent influenced later developments, in trying to fill in the missing splitting process description, even though it was not Everett's purpose, attached to PWM.

[34] This unpublished works are available online at UCISPACE (University of California Irvine) under the address http://hdl.handle.net/10575/1060, and in the brilliant volume edited by Barrett and Byrne (2012).

Once clarified that Everett's work has suffered from a certain degree of misunderstanding, it becomes important to understand why, and whence this feeling arises. As said above, the most robust misunderstanding comes from the lack of a detailed account of the splitting process, or better splitting in itself[35]: this is the reason why interpreters focused on understanding how branching should be intended, assuming that it was a structural absence to be filled with some kind of metaphysical structure.

What is the subject (or the object) of a splitting process? When does a splitting precisely occur? What happens afterwards and, above all, why don't we perceive anything as a splitting?

These questions moved both Everettians and detractors of PWM to formulate the classic problems discussed above.

There are both practical and theoretical reasons as to why Everett did not speak about splitting in a more detailed form.

If we take into consideration the historical and biographical reconstruction of Everett's work, recently developed (Barrett and Byrne 2012), we can see how Wheeler, who was Everett's advisor, pushed his pupil into mitigating his ideas and his exposition in order not to offend Copenhagen 'Monocracy'. From a practical point of view, when giving his explanation and speaking about splitting Everett mitigated his opinion, even when proposing it in a metaphorical context, to avoid offending Bohr's authority.

Wheeler himself wavered all his life regarding Everett's theory, fluctuating more towards the opinions of authority than his own convictions. His position oscillated between a strong acceptance of Everett's theory on the one hand, and devoted reverence to Bohr, who was his mentor, on the other[36].

Everett decided to work on foundations in his doctoral dissertation. He started working on it in 1956, and in his correspondence to Wheeler he presented his theory to him.

Wheeler recognized that Everett's theory was a challenge to authority, but he was also convinced that the theory could serve to quantum gravity[37], on which he was working. He also knew that paying lip service to Bohr's authority was a necessary

[35] As a matter of fact, it has to be said that Everett described the process, and branching 'machinery' very well. What is missing is a more concrete account, an image of what the world(s?) looks like after the branching process takes place.

[36] Misner reported (transcript, Everett and Misner 1977a) that Wheeler ironically said to believe in Everett's theory "except on Tuesday, once a month" because "he has to reserve one day a week to disbelieve in it".

[37] See Wheeler's assessments to the long thesis and the correspondence with Everett (Wheeler 1956a, 1956b, 1956c).

requirement for its diffusion and acceptance.
Looking at the theory from an historical perspective, Wheeler's decisions regarding Everett's theory (for example the decision to mitigate metaphorical expressions, cut the most original parts, and to eliminate the most evident polemic nuances against Copenhagen interpretation) derived from the necessity to render his theory acceptable, and above all 'accepted'.
Wheeler's devotion to Bohr is even clearer if one considers the development of the thesis: the first version had been cut in its more brilliant parts after Bohr's *ipse dixit*, and the refined and cut version of the long thesis, published as "The Relative State Formulation of Quantum Mechanics", is the result of this operation[38].
Nonetheless, Everett's original task was obvious even after being mitigated with Wheeler's advice of using it in order to justify quantum gravity, both in the introduction of the short version and in Wheeler's assessments. And its reception, however, was a disaster.
So the only circulating version of Everett's theory was that of the short thesis, and it was from that version that all his problems arose.
If the publication of the short version did not help Everett with its immediate reception, Copenhagen's strong rejection and the theory's subsequent oblivion for a long period did not help in the long run.
Copenhagen's approach to Everett's theories was both conservative and 'aggressive'. In 1959 Everett met Bohr in Copenhagen to discuss the thesis; it was "a hell of a...doomed from the beginning"[39], and determined Everett's final abandonment of working on the foundations of QM, as well as Wheeler distancing himself from PWM.
Moreover, its subsequent recovery with DeWitt did not improve things much, because the theory was presented as a Many Worlds Interpretation, with a definitive label that went far beyond the Author's intents.
Apart from the necessity to formulate the theory in a way that would obtain the consensus of Copenhagen, Everett did not feel that the use of a more evocative language was necessary to the theory in itself. The relative state formulation was

[38] The long thesis was organized around the measurement problem and its solution, whereas the short thesis presented a theory suitable for the development of quantum gravity, cosmology and field theory. The short thesis no longer contains the chapter on information theory and correlation, Everett's survey of possible solutions to the measurement problem, and his extended discussion of the nature of physical theories. What remains is a distilled presentation of Everett PWM, his principle of the fundamental relativity of states, and his derivation of the standard quantum statistics.

[39] Everett reported his meeting with Bohr in these terms to Misner (transcript, Everett and Misner 1977a).

a self compelling explanation of the world, without a metaphysical assumption: there is evidence that Everett believed that his PWM gave a complete, descriptive interpretation, for him his theory was logically and ontologically self-contained, but the singular way in which he addressed problems has led interpreters to think that his theory is incomplete and requires filling in with some important details.
But, back to the main question, which kind of entities are the subject of splitting process?
There are substantially two possibilities[40], which have been advocated down the years, and which depend upon the metaphysics one wants to defend:

- Splitting process is referred to *observers*: that is, this solution is proposed by those who want to take the boundary between our (classic) knowledge and microscopic (quantum) world as a matter of consciousness, mind, information. Anti-realist welcome this solution. Of course, this approach leaves the question of psychophysical parallelism open, which was one of Everett's first tasks;
- Splitting process is referred to *metaphysical entities* (more or less, *worlds-like* entities): there is a constantly branching universe, composed of (slightly) different copies of our perceived world at each time. The problem here is that to justify the passage between our classic world and the (unperceived) proliferation of quantum copies of it.

As it will be shown, there are significant reasons to refuse that one of these declinations was Everett's original intention.

1.2.1 Splitting observers

There are few places in which Everett himself speaks about the branching, or splitting process.
References to splitting *observers* can be found in his notes and early drafts, and at least some of the language about splitting observers can be found in even the final version of his PhD thesis:

[40]See Saunders (2010a, p. 4).

> Consequently we have a theory which is objectively causal and continuous, yet at the same time subjectively probabilistic and discontinuous. The theory can lay claim to a certain completeness, since it applies to all systems, of whatever size, and it can also give an explanation of the appearance of the macroscopic world. The price, however, is the abandonment of the concept of the uniqueness of the observer, with its somewhat disconcerting philosophical implications. (Everett 1957b).

As mentioned above Everett focused on the role of observers from the very beginning of his work on the foundations of QM. As we have seen above, the first problem was formulating a quantum theory capable of explaining a world that contained more than one observer. In a handwritten list containing several hypotheses for the title of the thesis title, we find for example "QM in a world of independent observers," "inclusion of all observers into the machinery of QM," and "the multi-observer form of QM," (1955a).
In a short paper addressed to Wheeler, in which Everett preliminarily faced the problem of the splitting process we find a very evocative metaphor:

> As an analogy one can imagine an intelligent amoeba with a good memory. As time progresses the amoeba is constantly splitting, each time the resulting amoebas having the same memories as the parent. Our amoeba hence does not have a life line, but a life tree. The question of the identity or non identity of two amoebas at a later time is somewhat vague. At any time we can consider two of them, and they will possess common memories up to a point (common parent) after which they will diverge according to their separate lives thereafter. We can get a closer analogy if we were to take one of these intelligent amoebas, erase his past memories, and render him unconscious while he underwent fission, placing the two resulting amoebas in separate tanks, and repeating this process for all succeeding generations, so that none of them would be aware of their splitting. After a while we would have a large number of individuals, sharing some memories with one another, differing in others, each of which is completely unaware of his "other selves" and under the impression that he is a unique individual. It would be difficult indeed to convince such an amoeba of the true situation short of actually confronting him with his "other selves". The same is true if one accepts the hypothesis of the universal wave function. Each time an individual splits he is unaware of it, and any single individual is at all times

> unaware of his "other selves" with which he has no interaction from the time of splitting. (Everett 1955c).

And here Wheeler wrote in marginal notes: "this analogy seems to me quite capable of misleading readers in what is a very subtle point. Suggest omission". The analogy does not occur again. Even if Everett was clearly willing to speak about splitting observers, it is more probable that here he just wanted to provide a consistent theory capable of modeling more than one observer.
It is in another early draft document that we find the first explicit talk about splitting observers. Everett says:

> The essence of this theory is the abandonment of the concept of the uniqueness of observers, i.e. that there are individual entities, machines, people, etc., which remain single unique individuals throughout periods of time. In this theory when measurements (or in general any observations) are made on systems by "observers" (by which we mean merely other systems) the observer itself splits into a number of observers, each of which sees a definite result for the state of the system.
> Now, all of this, which is seemingly quite farfetched and contrary to our experience, is actually implied if one takes seriously the formalism of wave mechanics [without the collapse dynamics] and we shall even see that we can recover [the collapse dynamics] from this picture as a tool of practical expediency, not as a basic hypothesis. (Everett 1955c).

And even these two paragraphs are barred to be omitted.
When discussing reversibility and irreversibility in the long thesis Everett says:

> . . . in observation processes the state of the observer is transformed into a superposition of observer states, each element of which describes an observer who is irrevocably cut off from the remaining elements. . . . As soon as the observation is performed, the composite state is split into a superposition for which each element describes a different object-system state and an observer with (different) knowledge of it. In a particularly careful description of how to understand measurement in the long thesis, however, Everett characterizes the split in terms of states splitting rather than physical observers splitting. We note that there is no longer any independent system state or observer state, although the two have become correlated in a one-one manner. However, in each *element* of the superposition (2.3),

> $\varphi^i\psi^O\ i[...,\alpha^i]$, the object system state is a particular eigenstate of the observer, and *furthermore the observer-system state describes the observer as definitely perceiving that particular system state.* It is this correlation which allows one to maintain the interpretation that a measurement has been performed. (Everett 1973a, p.68).

The most important account of Everett's role of the observer is in a footnote:

> At this point we encounter a language difficulty. Whereas before the observation we had a single observer state afterwards there were a number of different states for the observer, all occurring in a superposition. Each of these separate states is a state for an observer, so that we can speak of the different observers described by the different states. On the other hand, the same physical system is involved, and from this viewpoint it is the *same* observer, which is in different states for different elements of the superposition (i.e., has had different experiences in the separate elements of the superposition). In this situation we shall use the singular when we wish to emphasize that a single physical system is involved, and the plural when we wish to emphasize the different experiences for the separate elements of the superposition. (e.g., "The observer performs an observation of the quantity A, after which each of the observers of the resulting superposition has perceived an eigenvalue."). (Everett 1973a, p. 68).

The most important thing which appears from this note is that for Everett it was solely *a matter of language*: it was important to affirm the reality of splitting process, even when the observer apparently does not perceive anything like splitting. But it does not seem that Everett's goal was that to formulate a many-minds-like theory, i.e. something like a division between consciousness, mental domain and physical, 'real' world. Plurality is strictly connected to reality, in a physical sense[41].

1.2.2 Splitting worlds

Another step towards understanding Everett's original idea of splitting is in his reaction to DeWitt's proposal, thanks to which the theory was brought to light again after fifteen years of neglect. Everett in fact never spoke about worlds, even

[41] This contradicts many-minds-like interpretations; see also Bevers (2011).

if his theory is best known as Many Worlds Interpretation (MWI).
Dewitt's attempt to explain Everettian QM is the first historical attempt to fill it in with extra structure. He believed that Everett did not characterize the subject of a relative state properly, and took the theory to extremes: relative states are to be represented as single states of singles copies of the world, each of which having a definite outcome. DeWitt called this, now complete, theory as EWG, because for him it was an Everett-Wheeler-Graham theory. He introduced this new metaphysical commitment to splitting worlds as its central feature: real and causally closed worlds are to be thought of as relative states of a cross section of the universal wave function.
He represented this with a Schrödinger's cat experiment:

> The animal [is] trapped in a room together with a Geiger counter and a hammer, which, upon discharge of the counter, smashes a flask of prussic acid. The counter contains a trace of radioactive material—just enough that in 1 h there is a 50% chance one of the nuclei will decay and therefore an equal chance the cat will be poisoned. At the end of the hour the total wave function for the system will have a form in which the living cat and the dead cat are mixed in equal portions. Schrödinger felt that the wave mechanics that led to this paradox presented an unacceptable description of reality. However, Everett, Wheeler and Graham's interpretation of QM pictures the cats as inhabiting two simultaneous, non interacting, but equally real worlds. (DeWitt 1970, p. 31)

So, without adding extra postulates, PWM for how DeWitt intended it, is a *meta-theory* from which the interpretation is logically deduced[42]:

> Nevertheless, this is precisely what EWG would have us believe. According to them the real universe is faithfully represented by a state vector similar to that [above] but of vastly greater complexity. This universe is constantly splitting into a stupendous number of branches, all resulting from the measurement like interactions between its myriads of components. Moreover, every quantum transition taking place on every star, in every galaxy, in every remote corner of the universe is splitting our local world on earth into myriads of copies of itself. (DeWitt 1970, p. 33).

[42] See also Bevers (2010).

DeWitt assumed that with PWM the formalism of QM is capable of yielding its own interpretation:

> Without drawing on any external metaphysics or mathematics other than the standard rules of logic, EWG are able, from these postulates, to prove the following meta-theorem: The mathematical formalism of the quantum theory is capable of yielding its own interpretation. (DeWitt 1970, p. 33).

The long and the short versions of Everett doctoral dissertation were published in the anthology 'The Many Worlds Interpretation of Quantum Mechanics' in 1973, and from that moment the language of *worlds* stuck to Everett's interpretation, even if the world's speaking is due to DeWitt's reception. There is good evidence that DeWitt's version of Everett's theory is filtered by Graham's own view, while Everett and Wheeler's versions are different.

Wheeler himself recognized that MWI derived from DeWitt, and not from Everett. "Bryce DeWitt, my friend at Chapel Hill, chose to call the Everett interpretation the "Many Worlds" interpretation" says Wheeler, and even if "DeWitt's terminology is now common among physicists" he added in brackets "(although I don't like it)". Wheeler then said that "The idea has entered into the general public consciousness through the idea of "parallel universes." Although I have coined catchy phrases myself to try to make an idea memorable, in this case, I opted for a cautious, conservative term. "Many worlds" and "parallel universes" were more than I could swallow." (Wheeler and Ford 1998, pp. 269-270).

For Wheeler, many-worlds speaking was an "oversimplified way" of understanding Everett's theory, and indeed Everett's own view is more subtle than suggested by DeWitt's presentation.

The point now is that DeWitt implicitly recognized that it was not Everett's original idea to speak about worlds, but in all cases it was a logical consequence already contained in the theory that the different parts of superposition, deriving from a cross-section of the universal wave function, have to be intended as "worlds". Indeed, he refers to the acronym EWG giving credit to Everett for the meta-theorem, to Wheeler for supporting him and to Graham for clarifying the meta-theorem. For DeWitt, PWM alone entailed *worlds*, and these entities should be regarded as explaining our experiences.

Even if DeWitt intended that the existence of many splitting worlds was entailed in PWM alone, this is not exactly a logical consequence. As Barrett points out, "as a point of logic purely mathematical postulates entail only purely mathematical theorems, one cannot deduce any metaphysical commitments whatsoever regarding the physical world from the mathematical formalism of PWM alone" (Barrett 2010, p. 281); a metaphysical commitment can be envisioned as logically entailed only if it is a proper statement of the theory, which involves interpretational principles that go beyond bare mathematical formalism.

1.3 A matter of language?

Both Wheeler and Everett preferred to use "relative state" language, and this choice is not simply a matter of convention.

Everett was more confident using the language of branches or relative states in his written works, whereas the language of worlds was employed, or better tolerated, in conversation.

His preference in using relative state language and referring to relative states, branches and elements of the superposition is also related to his preference of describing the correlation structure in terms of splitting observers, as mentioned before. There is also evidence of Everett's awareness of the difficulties involved in using both languages, because both are misleading, although he does not appear to have been particularly focused on the problem, even if he was fully aware that these kind of conceptual difficulties were absolutely resulting from his theory; he assumed that the problems are of a metaphysical or linguistic nature.

The document of the 1962 conference on the foundations of QM at Xavier University is a precious element in understanding Everett's attitude towards worlds language. Everett was invited only after a discussion about 'parallel worlds', in order to clarify his position. Podolsky resumed in this way: "Oh yes, I remember now what it is about—it's a picture about parallel times, parallel universes, and each time one gets a given result he chooses which one of the universes he belongs to, but the other universes continue to exist" (Transcript: Conference at Xavier University, Werner 1962, Monday AM 13).

Once invited, Everett had the opportunity to explain in person his theory as follows:

> The picture I have is something like this: Imagine an observer making a sequence of observations on a number of, let's say, originally identical object systems. At the end of this sequence there is a large superposition of states,

> each element of which contains the observer as having recorded a particular definite sequence of results of observation. I identify a single element as what we think of as an experience, but still hold that it is tenable to assert that all of the elements simultaneously coexist. In any single element of the final superposition after all these measurements, you have a state which describes the observer as having observed a quite definite and apparently random sequence of events. Of course, it's a different sequence of events in each element of the superposition. In fact, if one takes a very large series of experiments, in a certain sense one can assert that for almost all of the elements of the final superposition the frequencies of the results of measurements will be in accord with what one predicts from the ordinary picture of QM. That is very briefly it. (Transcript: Conference at Xavier University, Werner 1962, Tuesday AM, 18)

Podolsky insisted on a change in using worlds language. "Perhaps it might be a little clearer to most people if you put it in a different way. Somehow or other we have here the parallel times or parallel worlds that science fiction likes to talk about so much."

And Everett replied "yes, it's a consequence of the superposition principle that each separate element of the superposition will obey the same laws independent of the presence or absence of one another. Hence, why insist on having a certain selection of one of the elements as being real and all of the others somehow mysteriously vanishing."

Later in the exchange Podolsky said: "It looks like we would have a non-denumerable infinity of worlds" and Everett replied "Yes".

And also, in replying to Shimony, he added: "each individual branch looks like a perfectly respectable world where definite things have happened" (Transcript: Conference at Xavier University, Werner 1962, Tuesday AM, 18). It is also important to note that Everett, having the opportunity of revising his own comments, decided to keep the term world.

As further proof, Deutsch[43] recalls a private conversation with Everett who was "very enthusiastic about many universes, and very robust as well as subtle in its defense, he did not speak in terms of 'relative states' or any other euphemism"

[43] Deutsch has since become one of the strongest proponents of the multiverse theory, a version of DeWitt's splitting worlds interpretation of Everett. Indicating his degree of support for the theory, Deutsch claims that "[t]he fruitfulness of the multiverse theory in contributing to the solution of long-standing philosophical problems is so great that it would be worth adopting even if there were no physical evidence for it at all" (Deutsch 1997, p. 339).

(quoted in Byrne 2010b, p. 321–2). It seems now that Everett used the worlds language in conversations, or at least accepted it, and there is also no trace of Everett's clear objection to DeWitt speaking of his theory as a Many Worlds Interpretation, because he recognized that it was this that gave great academic interest to his theory, after a long period of neglect.

Everett's opinion of DeWitt was asked on two occasions.

In June 1977 William Harvey, associated with the Science Studies Unit at the University of Edinburgh, wrote to Everett, asking if he approved of the way DeWitt presented his theory (Harvey to Everett 1977).

Everett replied by describing the process that led to his paper being published in the DeWitt-Graham anthology; he then concluded ". . . I certainly approve of the way Bryce DeWitt presented my theory, since without his efforts it would never have been presented at all" (Everett 1977a).

We can get another important insight into Everett's own way of thinking from his correspondence with Lévy-Leblond. He submitted an enthusiastic paper to Everett (Lévy-Leblond to Everett 1976), in which he argued that there had been a "serious misunderstanding of Everett's thesis by many of his followers."

Describing the labours of Everett's propagandists:

> Once more, under a question of terminology lies a deep conceptual problem. [Everett's] interpretation in effect has been called by several people, especially DeWitt, one of his main propagandists, the "many worlds (or many-universes) interpretation of quantum theory." The rejection of the postulate projection [*sic*] leaves us with the "universal" state vector. Since, with each successive measurement, this state-vector "splits" into a superposition of several "branches", it is said to describe "many universes," one for each of the branches. Whereas the Copenhagen interpretation would arbitrarily choose "one world" by cutting off all "branches" of the state-vector except one (presumably the one of the "many worlds" corresponding to all possible outcomes of the measurement. Now, my criticism here is exactly symmetrical of the one I directed again in the orthodox position: the "many worlds" idea again is a left-over of classical conceptions. The coexisting branches here, as the unique surviving one in the Copenhagen point of view, can only be related to "worlds" described by classical physics. The difference is that, instead of interpreting the quantum "plus" as a classical "or", DeWitt and al. interpret it as a classical "and". To me, the deep meaning of Everett's ideas is not the coexistence of many worlds, but on the contrary, the existence of a single quantum one. The main drawback

> of the "many-worlds" terminology is that it leads one to ask the question of "what branch we are on," since it certainly looks as if our consciousness definitely belongs to only one world at a time: But this question only makes sense from a classical point of view, once more. (Lévy-Leblond to Everett 1976).

Lévy-Leblond then asked Everett whether he had got it right insomuch as there are no splitting worlds:

> I directly ask your opinion on what I take to be a crucial question concerning the "Everett and no-longer-Wheeler" (if I understand correctly!) interpretation of [QM]. The question is one of terminology: in my opinion there is but a single (quantum) world, with its universal wave function. There are not "many worlds," no "branching." etc. except as an artifact of insisting once more on a classical picture of the world. (Lévy-Leblond to Everett 1977).

In reply, Everett reported that he very much liked the paper, he agreed with Lévy-Leblond's analysis of the relationship between his work and DeWitt's, and he denied that the many worlds terminology was his. Everett wrote: "...I have not done further work in this area since the original paper in 1955 (not published in its entirety until 1973, as the "Many Worlds Interpretation of Quantum Mechanics"). This, of course, was not my title as I was pleased to have the paper published in any form anyone chose to do it in! I, in effect, had washed my hands of the whole affair in 1956" (Everett 1977b).

In a draft of the same letter, Everett said more about how he understood his relationship with DeWitt. Referring to DeWitt's energetic promotion of his work, Everett put it this way: "Far be it for me to look a gift Boswellian writer in the mouth! But your observations are entirely accurate (as far as I have read)" (Everett 1977b, draft).

In agreeing with Lévy-Leblond, Everett is agreeing that the notion of many worlds relies on a mistaken classical crutch and that his own formulation of QM is best understood in terms of a *single, but quantum, world.*

Everett mentions "the world view" one last time in a letter to a physics enthusiast, L. David Raub, in 1980, two years before Everett's death. Everett begins his reply to Raub by stating that he still supports all of the conclusions of his thesis and believes that his approach remains the only entirely consistent interpretation of

QM that explains both the content of the theory and appearances. Explaining why some do not like his theory, Everett says: "It is abhorrent to many individuals that there should not be a single unique state for them (in the world view), even though my interpretation explains all subjective feelings quite adequately and is consistent with all observations" (Everett 1980). Here Everett uses both the relative state terminology of his thesis and mentions the world view. As usual, Everett suggests that what should ultimately matter is that his view is logically consistent and consistent with our observations.

Concluding, from an historical perspective there is evidence that Everett was at least somewhat uncomfortable with DeWitt's description of his project and he specifically points out that many worlds language is not his own proper view, notwithstanding his gratitude to DeWitt for promoting his work.

1.4 Worlds *versus* relative states

There are also theoretically significant reasons for distinguishing Everett's relative states from worlds.

Everett and DeWitt disagreed in the appropriate measure for branches, which for Everett was a *typicality* measure. Both DeWitt and Graham thought of this measure for branches as a 'worlds counting', and this encountered Everett's frustration.

Moreover, DeWitt's worlds are metaphysical structures. Everett's original theory does not speak about worlds, because even if it could be considered evident and also necessary to find a characterization for branches, in order to describe what branches actually are, Everett's theory does not speak about the characterization of branches. Metaphysical claims are central to DeWitt and Graham, while Everett strongly refused any kind of metaphysical commitment. This leaves the question of the characterization of branches open, but also underlines Everett's refusal to accept any metaphysical commitments, which is a very important starting point in Everett's discussion, and one that has often been nuanced in the search of a good interpretation[44].

Apart from Everett's explanation of the nature of physics, which deserves a deeper exploration as we will see in the next chapter, in general he did not believe in the need for any special metaphysical support to a physical theory. For the moment it

[44] See, as a strengthening, Bevers (2010) and Vaidman (2010).

is sufficient to note that in his opinion 'the primary purpose of theoretical physics is . . . to make useful models which serve for a time and are replaced as they are outworn" (Everett 1973a, p. 111).
Resuming, it is not evident as DeWitt claims, that Everett's theory is composed of a meta-theorem necessarily involving a particular kind of metaphysical account for branches. So there is nothing that complies with DeWitt's description in Everett's work, and it is significant to note that there is nothing similar even in Everett's reply to DeWitt's letter to Wheeler. Furthermore, Everett's correspondence with Lévy-Leblond shows how the definition of a *single quantum world* (not our ordinary world, but a *world in quantum superposition*) is more adherent to Everett's own way of thinking than a Many Worlds Interpretation could be.
Two formal elements ordain the final non-overlapping between DeWitt's worlds and Everett's relative states:

- World-like branches *versus* arbitrary relative states;
- Interference phenomena *versus* classic behavior.

For DeWitt, a relative state corresponds to a metaphysically real copy of our physical world, each containing the experience of a definite outcome. Worlds splitting follows as a consequence of measurements, and ensures the existence of determinate records; worlds are simultaneous, non-interacting entities deriving from the decomposition of the wave function. So DeWitt individuates each branch as corresponding to a world, the totality being the universal state. That is, in order to characterize them as worlds, DeWitt describes them as parallel, non interacting, *classic* worlds.
For Everett, whose metaphysical grounding was very modest, but empirical requirements very high, whatever branches could be identified, for they are elements of a superposition, it was a matter of fact that they should be considered equally real: "[f]rom the viewpoint of the theory, all elements of a superposition (all 'branches') are 'actual,' none any more 'real' than another" (Everett 1957d). In other words, the fundamental relativity of states implies that there is nothing in the theory by which one of the elements of the superposition is chosen as "special" or more actual. All elements obey the same dynamical law, independently of one another. There is no preferred way to decompose the absolute state: this means that there is no preferred way to individuate branches, so the characterization of a branch is relative to the choice of a basis, and the correlation model does not

furnish anything like a possible individuation of a world between its structures. This also ensures the possibility of interference effects, which are the basis of the direct empirical consequences of the existence of superposition, at least in principle. Branches are not so evidently world-like.

As we saw in the above quote from Everett's letter to DeWitt, Everett's thinking was more oriented toward the deduction of classical physics from QM rather than the contrary: the microcosm is the basis for understanding our classical, macroscopic world. It is impossible to think of reality as a property of our classical world, while a quantum world is not real just because we cannot describe it in classical terms.

Even if branches are to be interpreted as worlds, they should exhibit *typical* world-like properties, and this should be the case for the overwhelming majority of branches. But Everett's standard of *typicality*, as we shall see later, is independent of the way decomposition occurs.

Relatively recently, another sort of realism has been suggested in order to deduce the existence of many splitting worlds as emergent entities, starting from *decoherence* considerations.

With decoherence, unobservability of interference effects is explained, and classical, or quasi-classical entities emerge from the quantum state: in other terms, the dynamical stability of the relative state is explained through entanglement with environment. Decoherence can be seen as the more *natural* development of DeWitt's interpretation of Everett, and it "furthers Everett's project insofar as it remains in the service of his goal of getting as much as possible from PWM while adding *as little interpretational apparatus as possible* (italics mine)." (Barrett and Byrne 2012, p. 45).

However, there are two reasons for not taking decoherence as resolutive.

All diverse decoherence "techniques"[45] try to explain under what circumstances a superposition of states behaves dynamically as an incoherent mixture of those same states (in order to explain why, at the end, we observe only one single result, or world).

Zeh (1970) firstly recognized that dynamical stability was the key to solve the preferred basis problem in the Everettian context. Here the enviroment is a sort of *superselective* tool, by which only one of the classically well defined properties of the initial superposition of a system is chosen for entanglement with a certain

[45] Saunders (2010a, p. 8) says that decoherence is "more of a heterogeneous collection of techniques than a systematic theory".

environment, and in this way interference phenomena are destroyed, so that each of the initial components will evolve independently[46].

Zeh's definition was still *qualitative*, and succeeding developments went more toward localization of consciousness than in physical features.

Further developments lead to "consistent histories" formalism: here, division between environment and subsystems is inessential, because what brings to observed quasi-classical states is the coarse-graining of certain dynamical variables, which evolve in a time-ordered sequence (Gell-Mann and Hartle, 1990).

But, and this is the first reason to reject decoherence at least in the present context (that of a faithful reconstruction of Everett's own thinking), this leads to the introduction of *probabilities*, because the global state is a superposition of this kind of histories (the wavefunction of the universe, in the Heisenberg picture), and the choice of variables equals the choice of preferred basis.

It will be shown that this kind of scheme is untenable if we want to understand original Everett, because the probabilistic aspect in PWM is reduced to its mathematical usage (see chapter 3 and 4).

Even the more recent developments of decoherence collide with Everett's own proposal. In fact, the probabilistic aspect introduces a sort of damaging *approximation*, since there is "no algorithm for extracting even approximately decohering histories for *any* Hamiltonian and *any* state" (see Saunders 2010a, p. 11).

Wallace (2010a, 2012) proposes a new structural and pragmatic interpretation: structures and patterns (such as consistent histories) are always *emergent* in physics. This kind of *high level* 'emergent' ontology is *routine* in science. All of our observations are structural observations[47]. *Worlds* are emerging entities, as other ordinary physical objects (fluids, crystals, cells, planets, galaxies, etc.), already instantiated in the wavefunction. Decoherence is then an "emergent process occurring within an already stated microphysics: unitary quantum mechanics" (Wallace 2010a, p. 64). Decoherence is a dynamical process whereby certain components of the fundamental state became autonomous of one another. But at the fundamental level, all histories exist. In this view, there is nothing problematic about the approximateness of the decoherence process, because this is an absolutely standard feature of emergence: there are structural facts about many microphysical systems which, although perfectly real and objective, simply cannot

[46]See Zeh (1970, p. 42), who recognized this property in observing chirality of sugar molecules.

[47]For a reply, see Maudlin (2010).

be seen in purely microphysical language[48].

The point is, however, that in PWM decoherence is used to explain the emergence of quasi-classical *worlds*.

The second reason for rejecting decoherence thus understood is the presupposition of a fundamental ontology, which at the end is DeWitt-style revisited metaphysics of worlds: Wallace assumes realism to be the theoretical framework for interpreting PWM, and this, again, exceeds Everettian ideas.

Concluding, for Everett both interpretations (many-mind-like or many-worlds-like interpretations) would have been too strong, because of their commitment to metaphysics.

I feel that this is a fundamental point to consider because Everett never changed his position in respect to his theory[49].

Everett allowed such kinds of definitions in colloquial contexts, in a descriptive dimension, just in order to clarify his theory and mostly its *reality*. But, and this element will be analysed in the next chapter, his empiricism is the reason for not giving a metaphysical characterization for branches.

While there is no single interpretation of Everett that fits in well with all of his published work, marginal notes, and correspondence, a main feature of his project is undeniably clear: Everett's PWM, described using the language of relative states, is committed to show that one obtains the same empirical predictions as the standard collapse theory, and this is confirmed in both his published and unpublished work. As we shall see, Everett's necessity of a good explication for experience was always in his mind, even if "in the history of physics, Everett's theory stands out as an example of how difficult it can be to translate a logically consistent formalism into a explanatory language"[50].

Everett's main task was that to *model observation*, in order to explain the measurement process without including anything magical or mystical. For Everett metaphysics has nothing to do with physics to the extent that a physicist aims to find a satisfactory explanation for perceived reality: QM makes no exception, because it is just a model for our experiences, and ordinary physical systems are

48 Wallace reports an example: a "tiger hunting pattern" could be studied from many different perspectives (its atomic or molecular configuration, etc.), but it is "ludicrously hard work to study tigers in this way. To reach a really practical level of description, we again look for patterns and regularities……those entities are structures instantiated within the molecular physics" (Wallace 2010a, p. 58).

49 See Barrett (2010, p. 39).

50 See Byrne (2012, p. 24).

nothing more than microscopic interacting elements, therefore a quantum explanation must reject all forms of arbitrary division between macroscopic elements and underlying microphysics.

Taking Everett's idea seriously implies accepting Everett's idea on its owns terms "as an understanding of what QM already claims, not as a proposal for how to amend it" (Wallace 2010a, p. 55). The axioms of unitary QM say nothing about 'worlds' or 'branches', because they speak only of a unitarily evolving quantum state.

A very important job to be done is that of justifying our experience with a representation within the theory, showing how one might find our actual experience represented in the correlation structure described by the theory.

Chapter 2

Undeniable but unobservable: branches from an empiricist perspective

So far we see what PWM is *not*.

While DeWitt had a strong metaphysical commitment to the theory of splitting worlds being central in understanding Everett's theory, Everett himself believed that his theory neither required nor supported any particular metaphysical commitments.

Although metaphysical speech is tolerated in discussion and, as we have previously seen, Everett himself often used expressions with a metaphysical flavor ("perfectly respectable worlds", for example, in replying to Podolsky), he never deemed it necessary to give an 'official' version of a possible metaphysical aspect arising directly from his theory. Even when he had the opportunity to discuss a possible completion of PWM, he always assumed that it was absolutely unnecessary, because the "theory itself was complete". By complete, he meant that it gave "a complete account of perceived reality": determinate outcomes, as well as usual statistical predictions provided by standard QM, perfectly in accord with PWM, while the 'mystical' cut between classical observers and microscopic quantum reality introduced by the "popular" (Copenhagen) interpretation could be avoided.

So once clarified that the need for metaphysics was not Everett's original target, we have to understand how one should consider the Everettian system in its totality, and hence to what extent it might be considered sufficiently self-compelling. If the global state, which is the fundamental entity, is to be interpreted without any

metaphysical assumptions, how we should look at the 'Everettian system' from a philosophical, interpretive and epistemic point of view needs to be clarified.
There are two main directions that can be taken in interpreting PWM.
These approaches are linked to specific theoretical assumptions: if one decides to interpret wavefunction as describing our knowledge, or whatever subjective and *local* can be thought about it, then an *anti-realist* position is assumed, because superposition is confined to a region of non-physical, mathematical space. What really counts as real is our perceived reality, so collapse is *factual*.
On the other hand, if one opts for a strong *realist* (metaphysical) position, then the quantum state is expression of splitting worlds, but in this way splitting entities are classic, metaphysical entities[1].
I argue that another route can be traversed, even if it seems paradoxical at first sight: why not assuming the global state as real, without giving any metaphysical significance or further connotation? This is the particular form of Everettian original mixture of realism and anti-realism.
As we saw in the previous chapter, Everett's PWM has to be interpreted starting from what I called the *original core*, which consists of bare formalism (QM taken literally, without postulating any sort of collapse), following which unitary quantum state is the fundamental entity, the relative states being a "consequence" of a literal version of linear dynamics taken in themselves; the deduction of determinate-records and probabilities in the *correlation model*, or the deduction of our classic perceived reality (relative state) as a subjective appearance (implying here even repeated measurements and sequences of different measurements, which lead to observers' memory sequences); the interpretation of these rules, which is related to Everett's own strong convictions regarding the requirements for a physical theory to be good, and which reveals a strong empiricism.
Everett indeed proposes an isomorphic identity between *some* parts of his theory and our perceived reality: in this sense, it is the theory that provides the boundaries between observable and unobservable parts, the latter being not isomorphic to our perceptions as long as the former are shown to be empirically adequate.
In other words, Everett assumed:

- the *global state*, described by the unitary evolution of the wave function to be the real and fundamental entity;

[1] See Saunders (2010a, p. 4).

- the *relative state* one obtains by isolating a single outcome relative to a measurement as a *relative* reality, in the sense that it represents just one branch. In this sense, a relative state is a subjective appearance, because it represents an observing device having a particular outcome.

The theory provides also an explanation for observers possessing a certain memory configuration: it is shown that the theory perfectly predicts an observer having just one particular memory sequence for observations which is in accord with our perceptual experience.
Statistical distributions can be seen to be properly represented in the model, and that is all we require in a theory. The global state, while being unobservable in its totality, is absolutely real, otherwise we would be obliged to introduce a final cut again. It is meaningless to ask which relative state counts as the most real, because *all* relative states are equally real: we could say that the global state is more real than (perceived) reality.
This means that the *original core* of Everett's idea is minimal regarding its interpretation: a detailed explanation of the remaining relative states is impossible insofar as it concludes in a metaphysical explanation. In other terms, any construction that one might make in order to characterize the global state beyond empirical experience is arbitrary.
This is the empiricism that characterizes Everett's way of thinking, testified to by his own declarations about the requirements for obtaining a good physical theory, and also from his deep sympathy respect of Frank. Furthermore, the same uneasiness in the face of strange, mystical and philosophically monstrous divisions introduced by *popular* interpretation testifies to his intention of formulating an empirically adequate theory. Everett's strong desire for a theory, and for his own theory, to be acceptable, is *empirical faithfulness*: once we grant that the theory provides an isomorphic (or, better still, homomorphic as will be explained later) model, we obtain an explanation that is in accordance with our experience, and we can then accept the theory.
Interpreters focused on the analysis of unobservable entities because it is not clear how they are to be intended. This has led to a multiplicity of *Everettian-like* interpretations, each proposing a metaphysical lecture starting from bare formalism. In the previous chapter we saw that the classic version of relative states as being *worlds* is not tenable in DeWitt's original version if we intend remaining faithful to a genuine account of Everett's theory. The same could be verified for other

many-fashioned interpretations[2].
The need for completion derives partly from the particular way in which Everettian ideas were initially disclosed, this means that it is difficult to understand how determinate-record experience has to be framed in PWM frame. Starting from this point, there are other more speculative problems: the existence of parallel entities and their characterization, identity over time, and so forth[3]. But the need to fill in the (apparent) gaps with metaphysical buildings appears to be beyond Everett's original intents.
In fact, Everett was not concerned with speculative problems: even if there is a sense in which the theory could be considered open to future achievements[4], this does not imply a metaphysically oriented completion. Again, interpretations have a value, that often go beyond a mere understanding of Everettian theory, but are distant from the original root.
Moreover, following Everett one discovers that the solution to classic problems of PWM (determinate-records and probability problems) is obtained within the theory itself; but by advocating a methodological position which is misleading, at least at first sight: the theory appears to be too rich in content, by implying a plethora of unobservable, multiple entities whose nature is not at all clear. However this does not mean that the remaining states of the cross-section of wave function have to be discarded as unreal once we have our relative state.
This apparent paradox, in which the global state is undeniable, but still unobservable, is resolved by assuming a modest *Humean*[5] approach: in other words, here I would like to investigate to what extent Everett is really capable of yielding his own interpretation starting from a new 'hermeneutic' analysis. What I am suggesting is a comparison between Everett's ideas regarding physical theories in general and those regarding *structural empiricism*, as proposed by Van Fraassen (2008), with its stronger pragmatism, which might prove useful in understanding extra structure while avoiding metaphysics.
Of course, one could always pose the question as to whether Everett's original core is sufficient by itself, but that is a query we have to postpone.

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[2] For a complete review, see Barrett (1999).
[3] See Saunders, S., Barrett, J., Kent, A., & Wallace, D. (Eds) (2010).
[4] Actually, Everett himself upholds that a theory should be oriented toward the possibility of future predictions by means of the discovery of new phenomena (see Everett 1973a, p. 134). This will be analysed in detail at the end of the chapter.
[5] See Bacciagaluppi (2013b).

2.1 The extra structure

Everettian problems, I feel, have the same unique origin: it is not clear how one should look at what might be called the "extra structure", namely the remaining relative states of the fundamental global state, once we have our own relative state, describing an observer as having a specific memory configuration in accord with usual quantum statistics.
After a first reading of Everett's theory, there is an unavoidable feeling of "strangeness": DeWitt himself witnesses this in saying that "It has been a "vivid shock encountering this multiworld concept. The idea of 100^{100+} slightly imperfect copies of oneself all constantly splitting into further copies, which ultimately become unrecognizable, is not easy to reconcile with common sense." (DeWitt 1970, p. 161).
Once the description of the actual state is acquired, one finds oneself related in one way or another to a proliferation of branches, in which all outcomes find a 'reification' although we cannot find any trace of these states in our ordinary empirical experience.
Extra structure's counterintuitive nature derives from its *unobservability*. This is the main reason that both interpreters and detractors surrendered and qualified Everett's theory as, at best, incomplete: from their point of view detractors found the unobservability of Everettian extra structure to be the final global state's worst characteristic, which candidates it to falsifiability[6], while his followers thought that PWM, in order to be complete, should explain the remaining states, the only way to characterize them being that of finding a new account for the explanation of reality which in the end is a metaphysical *many-fashioned* interpretation of states.
So critics found their main reason to reject Everettian theory in the extra structure because of its strong bond with metaphysical structure; on the other hand, followers of PWM thought it was necessary to add metaphysics to Everettian theory in order to complete it, reading the theory as revealing something new about the world in a metaphysical sense.
All these contributions have something in common: they approach the Everettian "system" with a *realist length*, taking for granted that Everett himself was advocating such a position (and, as explained above, in the case of the anti-realist reader, this is unacceptable, whereas dealing with a realist reader, this needs metaphysics).

[6] See for example Tegmark's review of most common worries about Everett's theory (Tegmark 2010, p. 553).

Also to at least understand his original proposal in clarifying and solving apparent problems, it is necessary to avoid a metaphysical suit for the extra structure. And this is possible because Everett himself was committed to a strong empiricism, which is clearly exposed in the second appendix to his long thesis, and which could be considered a *manifesto* of Everettian empiricism.
Again, as exposed in the previous chapter, both the initial rejection and the later acceptance of Everett's PWM suffered from a vitiated lecture, and that is the reason his empiricism was not emphasized at the time, whereas now it is becoming quite clear[7].

2.1.1 Unobservability

Everett's position regarding extra structure is that of advocating an anti-metaphysical methodological position, following which it becomes a *false problem.* For Everett, the extra structure is out of our direct experience, which is limited to only one branch. When a measurement is made, unitary dynamics continues in a superposition of different branches, each reporting a different outcome. If one of these branches (even more than one, as we will see) is demonstrated to exhibit a determinate record consistent with observer's memory configuration, then the model is coherent to experience. That is what occurs once we confront the determinate outcome we obtain with the relative state provided by the correlation model (which is what Everett calls the 'cross-section' of the total wave function). The extra structure, represented by the multiplicity of states which are not observed, *derives* from the theory: in other words, it is the theory itself that provides a model for our experience and a model for the global state (whose totality is, paraphrasing Everett, never accessible to us).
This position seems to be quite problematic at first sight, to the extent that it does not explain the real ontological flow of the extra structure.
The problem seems to have different levels: from a very trivial *psychological* perspective, it seems that the branching process has to be rejected simply because it is not noticeable in a perceptive sense; this in turn introduces a *pragmatic* level of critique, that is, unobservability of extra structure which could be a serious setback for PWM, to the extent that it is not testable. From a deeper *conceptual* point of view, unobservability seems to introduce some sort of falsifiable account,

[7] See Saunders, S., Barrett, J., Kent, A., & Wallace, D. (2010) and Barrett (2009, 2010, 2011a, 2011b, 2012).

capable of invalidating the theory.
This triadic lecture of inconsistency has to do with the methodological position one decides to assume, and of course if one opts for strong realism, then one is forced to introduce a new metaphysics in order to explain how this extra structure has to be qualified, or to abandon PWM as untenable.
Everett was aware of this problem, and explained his solution on different occasions, but his methodological assumption, which is fundamental in understanding his position, was not made clear and public until 1973, the year in which his long thesis was published in the DeWitt-Graham's collection of Everett's works (DeWitt 1973), and even in this publication, Everett's own position was obscured by DeWitt's proper many-world interpretation, as we saw in the previous chapter. The short version, which was the only circulating version until 1973, did not express Everett's position regarding the *status* of general physical theories in general. This is the reason that DeWitt himself, who later became Everett's strongest supporter, initially rejected the theory. Apart from the great 'amending' of 1973, in which he demonstrated all his enthusiasm for PWM, after having transposed it into 'MWI', in his first lecture on the short thesis, DeWitt was very skeptical.
In a letter to Wheeler, after resuming the potential elegance and beauty of PWM[8], DeWitt firstly recognized that the extra structure and branching process could be a real problem; he wrote about his concerns to Everett's advisor clearly pointing out how extra structure could affect the theory:

> What I am *not* prepared to accept relative to the subject at hand, however, is that the temporal behavior of the *superposition* of relative observer states $\Psi^0\ [\alpha_i^{\prime 1},\ \alpha_i^2, \ldots]$ is isomorphic to the "trajectory" of the memory configuration of a real physical observer, whether human or inanimate. As Everett quite explicitly says: "with each succeeding observation. . . the observer state branches into a number of different states". The trajectory of the memory configuration of a real physical observer, on the other hand,

[8] While holding that Everett's ideas "should have no experimental consequences whatever" DeWitt also recognized that a parallel can validly be drawn between Everett's ideas and the theory of relativity" because the "role of the observer relative to the rest of the universe is emphasized in both theories" for two reasons: firstly because "Everett's removal of the external observer may be viewed as analogous to Einstein's denial of the existence of any privileged inertial frame"; also Everett's "scheme is found to contain its own theory of the measurement process, by giving back again –but at a new level– the same elements of statistical interpretation which were put into it at the beginning, just as the combination of the mathematics of a Reimannian space with Einstein's equations gives back again (in first approximation) the laws of motion" (DeWitt 1957). Rovelli (1996) makes the same point.

> does *not* branch. I can testify to this from personal introspection, as can you. I simply do *not* branch. (DeWitt 1957).

In this quotation, it is evident that DeWitt is posing the problem starting from a simple psychological perspective: how can we speak about a branching process if we do not notice anything similar? Direct introspection should be considered the best yardstick to assess the veracity of the branching process.
Everett was aware of this kind of objection. In a letter to Jammer he notes that "the unwillingness of most physicists to accept this theory, I believe, is due to the psychological distaste that the theory engenders overwhelming the inherent simplicity of the theory as a way of resolving the apparent paradoxes of quantum mechanics as conventionally conceived" (Everett 1973b). His direct reply to DeWitt is provocative, at any rate: "I can't resist asking: do you feel the motion of the earth?", and he continues:

> One of the basic criticism leveled on the Copernican theory was that "the mobility of the earth as a real physical fact is incompatible with the common sense interpretation of nature. In other words, as any fool can plainly see, the earth doesn't really move because we don't experience any motion. However, a theory which involves the motion of the earth is not difficult to swallow if it is a completely enough theory that one can also deduce that no motion will be felt by the earth's inhabitants (as was possible with Newtonian physics). Thus, in order to decide whether or not a theory contradicts our experience, it is necessary to see what the theory itself predicts our experience will be. (Everett 1957d).

This first response is in order to show how direct introspection could not be assumed as a criterion for rejecting the branching process to the same extent that it could not be seen as a sufficient reason for rejecting earth movement.
We clearly cannot have direct access to the branching process from a "perceptual" point of view: firstly, and intuitively, because we are related to one single branch, we are in a specific portion of reality, and not outside the universe and Everett simply rejected the "external observation" link to the extent that it was a construal by which the cut between the classic and quantum world was introduced. In order to have an absolute position, we should conjecture an "absolute" external observer, namely outside the universe, and we cannot do that (see the previous chapter, footnote7). Instead we have a correlation model, from which we can find

our actual position, which is always of a relative kind.
From this it derives directly that a further characterization of branches should be of a metaphysical type, since we cannot have direct observation of a branching process.
Secondly and more deeply, awareness of the branching process, intending here experience of *bifurcation*, is not predicted by the theory: the relative memory sequences which express each relative state (branch) do not contain records for branching events, because a *typical* relative memory sequence exhibits the standard quantum statistics. This is a corroborate of the accord between relative state and our proper perceived reality: once we find a relative state which corresponds to our actual outcome, we also find that this relative state represents an observer having a particular memory sequence, which is in accord with usual quantum expectations, and especially with *not* having any feeling of the branching processes[9]. This is our psychological experience as described by PWM; so the theory, far from being at variance with perception, is in accord with our perceptual experience.
Actually, Everett says this clearly in reply to DeWitt:

> I must confess that I do not see this "branching process" as the vast contradiction" that you do. The theory is in full accord with our experience (at least insofar as ordinary QM is). It is in full accord just because it is possible to show that no observer would ever be aware of any "branching", which is alien to our experience as you point out", indeed, he adds, "all the separate elements of the superposition ("branches") individually obey the wave equation with complete indifference to the presence or absence ("actuality" or not) of any other elements. (Everett 1957d).

The crucial point is that Everett saw how the branching process flowed into a multiplicity of states, but it was not a concern because this kind of perceptual objection was not sufficient and above all because perceived reality was totally explained, without recurring to metaphysical completions.
In this sense it is a *false problem*: the theory is not incomplete, or contradictory with the experience, if for experience one refers to immediate, vivid, determinate actual experience, which is perfectly predicted and explained.
Furthermore, it is the theory itself that justifies and contains its own theory of measurement.

[9] See Everett (1973a, p. 68): "In each element of the superposition, the object-system state is a particular eigenstate of the observer, and furthermore the observer-system state describes the observer as definitely perceiving that particular system state".

2.1.2 Testability: a real problem?

The psychological level introduces a more pragmatic level of critique.

Indeed the fact that the extra structure has to do with unobservable entities leads to the problem of *non-testability*: if in our perceptual experience we have no access to the other branches, they may appear to be no more postulated than the collapse of the wave function, and even more problematically, since they are totally foreign to our experience, they could appear dangerously *ad hoc*; this seems to be quite at variance with the possibility of future experimental proof, required for the acceptability of a physical theory.

DeWitt remarked how it is "hard to believe that Everett's ideas, on the one hand, will appreciably affect experimental physics (even cosmic level), although this, of course, remains to be seen" (DeWitt 1957).

Everett clearly did not think that his theory was infected by some form of non-testability.

Firstly, testability is ensured for the region of observability implied by the theory: so it could always be demonstrated that our relative state is coherent with standard predictions of QM, and this not only in the trivial sense that it grants our actual relative state to be found but also a state in which a *typical* observer might find himself appropriately represented (his specific memory configuration)[10]; moreover, Everett also refers to a future possibility of developing new ways of testability (even if in a vague way), by using a distinction between merely descriptive and explanatory theories, including in the former category his own theory.

Everett's position is quite clear: the fact that a physical theory does not go beyond a specific region of "safety", in which already known and observable *data* are found, is not a virtue. A theory which does not prove itself with unobservable entities (certainly unobservable, but maybe in this specific moment!) is just surrendering to the temptation of constructing "perfectly safe theories which will never be open to contradiction. Strict adherence to such a philosophy would probably seriously stifle the progress of physics", while the contrary is true: a theory which is devoted to the main purpose of scientific progress, namely "*the discovery of new phenomena*" , will try to go beyond its boundaries, with "considerations of inductive inference and the factors which influence our confidence in a given

[10] Everett shows how usual quantum predictions are found for observers, repeated measurements, and sequences of arbitrary measurements. We will see how a 'natural' measure for typical branches could be found.

theory" (Everett 1957d) [11], in order to make it applicable outside the fields of its formulation. In other words, a theory should 'take the risk': the fact that physics sometimes has to do with unobservable entities is not as disruptive as it appears, it is a peculiarity of the path of scientific theories towards a new possibility of predictions, always remembering that "the totality of perceived experience is never accessible to us" (Everett 1973a, p. 134).

Non-testability is not enough to reject the global state as unreal: with non-testability we define certain regions of the theory model which do not end up being directly observable. But this is part of a theory's construction, and it might always be the case that those regions will give future access to new areas, namely *new phenomena*.

2.1.3 The transition from possible to actual: the conceptual level

Summarizing, Everett contested the inadmissibility of unobservable entities for two reasons:

1. what appears fundamental to Everett is to deduce our experience starting from the model. Awareness is a psychological problem and it is not sufficient criterion for rejecting the theory, because it is the theory which shows that an observer would never be aware of branching process. This is the only requirement one should need for his theory to be a good one;

2. non-testability again is not a limit, because firstly, it could always be possible for the future to expand our domain of testability, and furthermore, a theory should always test itself with the discovery of new phenomena.

So for Everett unobservability, with related unawareness and non-testability, does not imply non existence of branches.

Even at a more profound conceptual level, unobservability is not a sufficient criterion for classifying extra structure as unreal, at least if one assumes a more prudent (empiricist) position instead of yielding to a strong form of scientific realism, which implies the unreality of branches *or* the characterization of them as *worlds*.

It is starting from this perspective that DeWitt contested the assumption of the

[11] See also Everett (1973a, p. 109), in which Everett refers to the Copenhagen interpretation.

branching process even at this level (and proposed a realist many-worlds interpretation in the end). In the same reply to Wheeler, he said:

> I do agree that the scheme which Everett sets up is beautifully consistent; that any single one of the states $\Psi^0\left[\alpha_i^1, \alpha_i^2, \ldots\right]$, when separated from the superposition which makes up the total or "universal" state vector $\Psi^{S_1+S_2+\cdots+0}$, gives an excellent representation of a typical memory configuration, with no causal or logical contradictions, and which "built-in" statistical features. The whole state vector $\Psi^{S_1+S_2+\cdots+0}$, however, is simply too rich in content, by vast orders of magnitude, to serve as a representation of the physical world. (DeWitt 1957).

DeWitt uses an argument *á la* Occam here, underlining how it is not necessary to assume a structure which appears to be a richer representation of the real world[12]. For DeWitt, even in a deeper sense, the branching process could not be considered as real: the collapse of the wave function is *factual*, it implies the *reification* of one state or another.

The problem with the extra structure is closely related to another problem, i.e. the fact that the branching process should imply the so called "transition from possible to actual", because we 'choose' one branch to represent our relative state while the other structure is 'possible', in the confused sense that its existence is inaccessible.

After having cited Heisenberg, "the probability concept is closely related to the concept of possibility, the 'potentia' of the natural philosophy of the ancients such as Aristotle; it is, to a certain extent, a transformation of the old 'potentia' concept from a qualitative to a quantitative idea", DeWitt manifests his agreement with the orthodox model: "on the other hand, the single quantum jump is "factual" in nature; it "happens" in the same manner as an event of everyday life. That is to say, the real world does not branch. It is constantly in the process from passing from the possible to the actual – not many actual, but one actual" (DeWitt 1957). So for DeWitt, at least at that time, the "real" world is just *one* single world, its state continuously passing from possible to actual[13].

Again Everett denies this as a *false problem.*

[12] In the same letter DeWitt says: "But does Everett's thesis constitute an entirely valid application of Occam's razor? Of this I am not so sure".

[13] Even if limited in some respects, DeWitt indicates how Copenhagen interpretation and Bohm's hidden variables theory succeed in giving a better explanation because they both keep the "transition from possible to actual" in rescue.

Everett insisted on the existence of the other branches, and their *actuality*: all elements of the superposition are “actual”, none any more “real” than another. The “transition from possible to actual” is not problematic for him, since there is “no such transition, nor is such a transition necessary for the theory to be in accord with our experience” (Everett 1957d): this means that unobservability is not at odds with reality, and then actuality is not at odds with unobservability.

Everett replied by asserting that “it is completely unnecessary to suppose that after an observation somehow one element of the final superposition is selected to be awarded with a mysterious quality called “reality”, and the others condemned to oblivion. We can be more charitable and allow the others to coexist – they won’t cause any trouble anyway, because all the separate elements of the superposition (“branches”) individually obey the wave equation with complete indifference to the presence or absence (“actuality” or not) of any other elements”. (Everett 1957d).

This is the core of Everett’s reasoning, perhaps the more problematic point: the transition, which implies the passage from a possible existence (the “potentia” described by Heisenberg) to the “reification” in actual physical states, is denied by Everett, according to whom such a transition is not necessary for the theory to be in accord with our experience: all the branches exist simultaneously, but we just *live* one of them.

In other words, branches cannot be experienced or perceived, even if they are all actual and real to the same extent as the one we live in.

When we speak of ‘characterization’ here we clearly intend a kind of metaphysics, because branches are not directly experienced. It is *nonsense* to ask what kind of branches there are outside, since we should speak only about what is in our actual reality, accessible from an empirical point of view: otherwise we are leaving the door open to a metaphysical explanation.

Summarizing, the Everettian position appears to be very clear in two indisputable points: while it is impossible to infer the *nature* of branching from experience, because we can only infer the nature of perceived reality, it would be wrong to deny the existence of a continuous branching process just because we cannot directly experience it, this kind of overcautious consideration being contradictory to the main purpose of science, which is that of discovering new phenomena.

2.2 Everett's empiricism

Unobservability is a longstanding problem for science: it has to do with the account of unobservable entities on the one hand, and structures (or models) on the other[14].

Normally it refers to microscopic entities[15], such as paramecia and mitochondria or bigger entities, such as galaxies and black holes.

But the same kind of reasoning can be applied to Everettian 'strange' branching entities, remembering that the author himself suggested how the vexed question of the acceptance of his theory came about also for other acquired theories (referring to the Copernican theory and also to Special Relativity); he prefers indeed to insist on the predictive power of the theory and on its ability to justify our experience from within itself.

Everett maintains that extra structure, the branching process and different relative states are unobservable, but this does not imply that they should be considered as unreal. Simply the equation in which real equals observable is wrong. Fundamental to this position is the fact that collapse has no kind of reality, and no way to invalidate the total wave function, but with good reasons for believing that it continues to survive, all branches are to be deduced, even if not observed. This is the reason we cannot find any explanation of extra structure: it is implied in the theory and our experience, but we cannot investigate it.

That is: collapse is unobserved, while branches are unobservable by definition.

Collapse should be observed in our ordinary lives in order to test its validity, because it occurs in our perceptual horizon, while branches due to their nature cannot be observed, because they are out of our perceptual horizon.

So, it appears to be a strong contradiction that we should believe in the reality (actuality) of other branches, while it is incorrect to describe them, under the penalty of building a metaphysical structure.

This apparent contradiction, in which unobservable entities cannot be described but have to be assumed real, is dissipated once we look at Everett's understanding of the main features of a good scientific theory.

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[14] See Chakravartty (2014).

[15] See Alspector-Kelley (2004) for a discussion on the role of measurement devices in detection of unobservable entities, as described by van Fraassen (1980). The Author, in noting that a satisfactory account for unobservable entities is missing, proposes to classify information gathered through the 'unaided eye' by focusing more on 'decision' than on 'discovery': "This is not to say that our decision is arbitrary. On the contrary, we are, I think, guided by fundamentally epistemic considerations" (p. 343).

Everett's discussion of the goals of theoretical physics, towards the end of his long thesis and in reply to DeWitt suggests a *conservative strategy*[16] for how one might understand his deduction of determinate measurement records and quantum statistics.

This strategy provides a concrete sense in which PWM can explain both determinate measurement records, quantum statistics and also constitute a theoretical frame for the extra structure.

As Everett explained in his long thesis, 'the primary purpose of theoretical physics is . . . to make useful models which serve for a time and are replaced as they are outworn": with the totality of reality completely inaccessible to us, we should renounce the temptation of finding a direct, complete explanation for *all* elements of reality (intending here even unperceived reality), but rather stimulate scientific progress through the discovery of new phenomena, while being content with a proper model for our perceived reality.

Indeed, in the second appendix to the long version of his thesis, Everett explains that taking a theory to be descriptive of the metaphysics of the world is a methodological mistake:

> [W]hen a theory is highly successful and becomes firmly established, the model tends to become identified with "reality" itself, and the model nature of the theory becomes obscured. The rise of classical physics offers an excellent example of this process. The constructs of classical physics are just as much fictions of our own minds as those of any other theory [. . .] we simply have a great deal more confidence in them. It must be deemed a *mistake*, therefore, to attribute any more "reality" here than elsewhere. (Everett 1973a, p. 134).

Everett emphasizes how scientific (realist) reasoning tends to identify the model provided by the theory with reality itself, once its predictive power is confirmed in practice. But models are constructions which serve for practice, and there is no reason to identify a model with that reality. Certainly, the model should be in accord with our perceived reality, but it is not taken for granted that the model is identical to reality itself. That is, the theory is not completely identical to perceived reality, because in general a physical theory, with its models, is not a universally valid copy of reality, it is just a representation of our reality by means of description of our empirical *data*.

[16] See Barrett (2011b).

For Everett, a theory provides *models* for interpreting perceived reality. But the model itself is nothing more than a *mathematical structure*: this means that it is a mistake to take the model as reality.
Structure is the way science depicts reality, but is not reality itself: with a certain structure we try to interpret phenomena. Structure is our instrument to embed phenomena into a theory, but there is not a relation of identity between phenomena and model:

> Once we have granted that any physical theory is essentially only a model for the world of experience, we must renounce all hope of finding anything like the correct theory. There is nothing which prevents any number of quite distinct models from being in correspondence with experience (i.e. all "correct"), and furthermore no way of ever verifying that any model is completely correct, simply because the totality of all experience is never accessible to us. (Everett 1973a, p. 134).

The emphasis goes completely on the *correctness* of the theory once we grant that it is able to explain observable reality. The other parts are to be deduced using "considerations of inductive inference and the factors which influence our confidence in a given theory" (Everett 1973a, p. 135). For Everett our perceived reality is totally described by PWM and furthermore the theory tells us that there is a larger part beyond our horizon of perception (for now) which could be inferred. PWM provides a correct explanation of our perceived reality, and experience and theory agree in declaring that apart from our relative state, there is a branching process, because:

- we do not observe collapse so the wave function still 'survives', in one way or another;
- the total wave function is inaccessible to us: the theory says that we experience just our actual branch, and nothing like a branching experience is given for an observer.

So far we have seen that Everett's aim is to introduce a *structure* in which the relative state might be understood. This structure serves as guidance and permits us to understand the passage from classical to quantum world, with all its paradoxes, simply as a *subjective experience*. In this way, it is not necessary to postulate any sort of discontinuous and arbitrary change, because our experience is explained to perfection without resorting to strange, and unobserved phenomena (such as collapse).

Furthermore, our experience is explained starting from the theory, and this is proof of the value of the model, because it is the theory itself which grants the delimitation of observable and unobservable entities. The definition of those areas of the model which describe unobservable entities or structure should be defined, but definition itself has only an operationalist importance, and should not be confused with a universal criterion[17].

That is: the totality of our experience being inaccessible to us, we delimitate our section of accessible reality, which is the observable part of it. It is this part which has to be identical to the theory or, in other words, it should be faithfully reproduced by the theory.

For Everett the extra structure has to be taken into consideration to the extent that it is contained and implied in the theory; that is the reason his explanation could result quite incomplete *prima facie*, because he assumes a methodological position from which we can only speak about what the theory says about our viewable reality, as extra structure is unobservable.

We describe the branching process and branching structure only in the parts implied by direct observation. The result is that all the 'philosophical' features that could emerge from the theory are abandoned, because they are the result of the temptation to identify the theory itself with reality. The theory is complete, because it gives a complete explanation of our reality, without resorting to mysterious or strange factors, such as a boundary between the classic-reality and the quantum-unreal.

The fact that Everett did not give further characterization to his branches derives from a precise methodological assumption: his direct reference to empiricism and mostly to Frank, to whom Everett writes seeking approval, testifies to his strong methodological *credo*[18].

Everett took his view of theories to be "very nearly identical" to those held by

[17] Everett exemplifies this with special relativity: "the critical examination of what quantities are observable in a theory does, however, play a useful role, since it gives an insight into ways of modification of a theory when it becomes necessary. A good example of this process is the development of Special Relativity". But he reminds us that "such successes of the positivist viewpoint, when used merely as a tool for deciding which modifications of a theory are possible, in no way justify its universal adoption as a general principle which all theories must satisfy" (Everett 1973a, p. 136).

[18] In 1957 Everett corresponded with several supporters of his theory. See Everett's correspondence with Margenau and with Frank.

Philipp Frank, one of the founders of the Vienna Circle.
Frank was broadly empiricist, and characterized his philosophical position as a fusion of positivism, pragmatism and operationalism, holding that 'the spirit of all these schools of thought is one and the same' (Frank 1946, p. 3)[19].
Everett may have read at least four of Frank's works (Frank 1946, 1950, 1954, 1955). His link to Frank is testified to by a letter in which he expressed his full agreement with his philosophical position.
His devotion to Frank is clearly admitted:

> I think that it will be quite interesting to see the various reasons for accepting or rejecting the theory that will be advanced by various physicist. [..] because of its particular nature this theory is especially suited to a number of points which can arise in any discussion of the nature of theories in general. If you have any interest in the matter [PWM] with respect to the more general questions of the philosophic, psychological or sociological reasons which influenced attitudes toward physical theories, I will be happy to send you copies of the comments and criticism that I receive. (Everett 1957c).

Everett refers to Frank's idea of validation of scientific theories (Frank 1954). Frank identifies as good virtues for a physical theory "agreement with observations" and "mathematical simplicity"; but since they are always unachievable targets, it is sufficient for our physical theories to be, at best, in agreement with observable facts and sufficiently simple to be useable: the most we can hope for obtaining a compromise between both criteria. Frank thus concludes that a theory cannot be 'the truth"; rather, it is an instrument that serves toward some definite purpose, a tool that produces other tools according to a practical scheme. Moreover Frank's considerations about psychological, sociological and contextual factors in the acceptance of a theory show how a theory's acceptance is often connected more to its compatibility with common sense than to its simplicity or empirical adequacy. Frank words resound those of Everett:

> Looking at the historical record, we notice that the requirement of compatibility with common sense and the rejection of "unnatural theories" have been advocated with a highly emotional undertone, and it is reasonable to

[19] Frank found significant philosophical agreement with Carnap's recent work and with the pragmatic tradition represented by C. S. Peirce, William James, and P. W. Bridgman.

> raise the question: What was the source of heat in those fights against new and absurd theories? Surveying these battles, we easily find one common feature, the apprehension that a disagreement with common sense may deprive scientific theories of their value as incentives for a desirable human behavior. In other words, by becoming incompatible with common sense, scientific theories lose their fitness to support desirable attitudes in the domain of ethics, politics, and religion. (Frank 1954, p. 9).

Everett probably refers to this when he writes:

> I am an example of a certain type of theories – a completely abstract mathematical model, which is ultimately put into correspondence with experience. It has the interesting feature, however, that this correspondence can be made only by invoking the theory itself to predict our experience – the world picture presented by the basic mathematical theory being entirely alien to our usual conception of "reality". The treatment of observation itself in the theory is absolutely necessary. If one will only swallow the world picture implied by the theory one has, I believe, the simplest, more complete framework for the interpretation of quantum mechanics available today. (Everett 1957c).

These ideas are quite in accord with Everett's own way of thinking about physical theories, and contribute to encompass him into this form of empiricism. As pointed out in Barrett, "that Everett described his view of the nature of physical theories as being very nearly identical with Frank's, situates Everett's understanding in a relatively clear historical context." (Barrett 2011a, p. 697).
Indeed, even Frank refused to commit himself with metaphysics since he regarded "metaphysics as a direct interpretation of scientific principles in terms of the language of everyday life experience. 'Interpretation' means translation. Metaphysics attempts a translation of the basic principles of science, but not according to a strictly fixed dictionary; the univocal relation between a term and its translation has been replaced by an analogical relation. But we cannot tell by any exact criterion what is a 'correct' analogy" (Frank 1955).
Also, Frank divided theories into three essential parts: *equations*, *logical rules* for manipulating them, and *semantic rules* that tied the language to the formal theory. Semantic rules have operational meaning, since they allow us to check if what is held in the theory, in a formal theoretic language, can be confirmed experimentally. If there is agreement, then the system as a whole is confirmed. This kind

of scheme is almost identical to Everett's own, which will be exposed in the next paragraph.

2.3 Isomorphism *versus* homomorphism

When DeWitt qualifies the whole state vector as "simply too rich in content" he is referring to the lack of *isomorphism* between the theory and our real world, so that the 'excessive' richness is inexplicable once one looks at the world as it appears. This is the reason why DeWitt ended with a many-worlds theory, in which each world is isomorphic to the relative states indicated by the theory.

After his first reading, DeWitt noted how Everett's system "contains all possible branches in it at the same time. In the real physical world we must be content with just one branch. Everett's world and the real physical world are therefore not isomorphic". He also added that "certain terms, such as "isomorphism", would seem to be useful under almost any conceivable circumstances and yet retain meanings which are held within fairly rigid bounds. I am afraid that it is at precisely the most crucial point in Everett's argument where many people, including myself, will be unable to swallow your implication that the word "isomorphism" applies" (DeWitt 1957).

The point here is that DeWitt simply misunderstood Everett's original purpose.

While speaking of an *isomorphic* model for the world, he ignores the fact that Everett himself explained this in the appendix of his long thesis, specifying that he preferred the word *homomorphism*: as said above, he did not require every element of the total superposition to be in perfect correspondence with the perceived world. It was sufficient for him to find a concrete relative state appropriately represented in the model provided by the theory. Everett does not identify "correspondence"[20] with identity as a mapping between the theory in itself and perceived reality. Rather, he advocates a one-to-one correspondence, or identity, between the model, which describes our relative state and perceived reality, which is better described as an *homomorphism*:

> By isomorphism we mean a mapping of some elements of the model into elements of the perceived world which has the property that the model

[20] DeWitt refers, in using the term identity, to the short thesis (Everett 1957b) which is the only one he had read. In the long thesis, Everett himself clarifies how the word isomorphism is to be intended. See Barrett (2011b).

> is faithful, that is, if in the model a symbol A implies a symbol B, and A corresponds to the happening of an event in the perceived world, then the event corresponding to B must also obtain. The world homomorphism would be technically more correct, since there may not be a one to one correspondence between the model and the external world. (Everett 1973a, p. 133)[21].

Indeed, according to Everett's approach to physical theories in general, the only requirement for a scientific theory to be a good theory is *empirical faithfulness*, which implies that there must be a sort of 'isomorphism' between a mathematical model and the perceived reality.
Everett then continues describing how a theory should be:

> Every theory can be divided into two separate parts, the formal part, and the interpretive part. The formal part consists of a purely logico-mathematical structure, i.e., a collection of symbols together with rules for their manipulation, while the interpretive part consists of a set of "associations" which are rules which put some of the elements of the formal part into correspondence with the perceived world. The essential point of a theory, then, is that it is a mathematical model, together with an isomorphism between the model and the world of experience (i.e., the sense perceptions of the individual, or the "real world" depending upon one's choice of epistemology). (Everett 1973a p. 133).

Rather than describing the metaphysical structure of the world, a successful physical theory for Everett is supposed to be somehow isomorphic to the world of experience. A theory has:

- A *formal* part, consisting in purely logico-mathematical structure, and rules for their manipulation;
- An *interpretive* part, consisting in a set of associations (in this part correspondence between the formal part and the perceived world is ensured).

This isomorphism (homomorphism, as we saw above) is a mapping between the model and the world as we see it.
This means that the only requirement of a theory could be its *empirical faithfulness*

[21] See also Barrett and Byrne (2012, footnote on p. 169).

to perceived reality, and theory acceptance implies the belief in its faithfulness, and nothing more: we should renounce finding something like the truth, and in the belief in truth.
Everett underlines that *homomorphism* is more correct, because there will be elements of the model which do not find correspondence in the perceived world.
This is the crucial point, because with homomorphism Everett recognized that the extra structure was not isomorphic to perceived reality. Isomorphism was to be understood between the relative state and the branch of total superposition.
Everett clarified his position: the theory he advocated provided a model which perfectly explained our determinate outcome without using the collapse postulate (in the sense already described: collapse is introduced simply as a tool, for operationalist purposes, but it has to be understood *locally*).
The theory then implies a model for other structures of the global state, which *are not isomorphic to our perceived reality.* This was the sense in which DeWitt spoke about the necessity of an isomorphic explanation, which he could not find in the edited version of the short thesis. DeWitt is misunderstanding Everett when he says "it concerns the question of what is meant or may be meant by the word "correspondence" – a better word would be "isomorphism", but you seem to have avoided it – particularly when applied to the ensemble of Everett's relative state vectors $\Psi^0[A,\ B,\ C\ldots]$ as compared with the experience of a real physical observer" (DeWitt 1957).
Actually, DeWitt will then opt for a global isomorphism, ultimately ending up with a many-worlds interpretation. Everett however was searching for a different mapping: isomorphism, i.e. the one-to-one mapping was between the perceived state and the predicted state, as being *typical.* Homomorphism indicates the correspondence between relative-determinate-record states (all the possible relative-record states) and the global state.
In replying to DeWitt he writes:

> First, I must say a few words to clarify my conception of the nature and purpose of physical theories in general. To me, any physical theory is a logical construct (model), consisting of symbols and rules for their manipulation, some of whose elements are associated with elements of the perceived world. If this association is an isomorphism (or at least a homomorphism) we can speak of the theory as correct, or as faithful. The fundamental requirements of any theory are logical consistency and correctness in this sense. (Everett 1957d).

It is homomorphism which grants that the theory is faithful to our perceived reality.
Everett's faithfulness concept, together with his idea of model is in accordance with the empiricist structuralism description of what is required for a theory to be *empirically adequate*.

2.4 Everett meets Van Fraassen

As mentioned above, Everett's ideas of physical theories in general are also comparable to empiricist structuralism. Indeed his description of model and, mostly, the implicit criterion of pragmatic selectivity makes the theory comparable to Van Fraassen's empiricist structuralism, as proposed in *Scientific Representation* (2008).
Even in the primitive formulation of *The Scientific Image* (1980) there are points in common, but the introduction to a pragmatic criterion of selectivity in *Scientific Representation* is the key to understanding Everett's apparent incomplete position regarding unobservable entities.
Van Fraassen rejected Everett's interpretation on various occasions[22], but he understood it *à la* Bell[23], and not under the light of the latest discoveries (Barrett and Byrne 2012)[24]; Everett's archive provides new important interpretative roots, which are mostly oriented toward a strong empiricism[25].
Even for Van Fraassen "to believe a theory is to believe that one of its models correctly represents the world. You can think of the models as representing the possible worlds allowed by the theory; one of these possible worlds is meant to be the real one. To believe the theory is to believe that exactly one of its models correctly represents the world (not just to some extent, but in all respects)" (Van Fraassen 1980, p. 47).
This means that "a model often contains much that does not correspond to any observable feature in the domain... The model's structure must be taken to reveal

[22] See Van Fraassen (1972, 1980, 1991a, 1991b).
[23] See Bell (1987) and Van Fraassen (1991a, p. 273), which clearly refers to Bell.
[24] See Osnaghi (2008) for a very precise description of analogies between Everett and Van Fraassen in contrast to Copenhagen interpretation, in which the Author shows "the import of the early debate on Everett's thesis for the current discussions of the measurement problem, and, more specifically, for any account and solution of the problem along van Fraassen lines" (p. 157).
[25] See Barrett and Byrne (2012, "Conceptual Introduction").

structure in the observable phenomena, while the rest of the model must be serving that purpose indirectly. It may be practically as well as theoretically useful to think of the phenomena as embedded in a larger – and largely unobservable – structure." (Van Fraassen 2008, p. 87).

Van Fraassen recognizes that the model itself is not an entire copy of perceived reality, he also sees how unobservable entities could constitute a serious danger for an empiricist account of science.

Van Fraassen says: "with human participation entirely foreign to the context, what can constitute adequacy or truth except a direct theory-model to nature relation? How can we assert that those unknown phenomena fit models of our theories, except in a sense that implies the metaphysical realist's postulation of 'structure in nature', consisting of universals or the like? That rhetorical questions present the first of the main dangers for the empiricist that I want to take up here" (Van Fraassen 2008, p. 246).

To avoid this danger, the focus should be on *empirical adequacy* more than the notion of *truth*. It is not wrong for a theory to describe unobservable parts of the world, since "physical theories do indeed describe much more than what is observable" but what matters is "empirical adequacy, and not the truth or falsity of how they go beyond the observable phenomena.... empirical adequacy: because it relates the theory to the actual phenomena, it does not collapse into the notion of truth" (Van Fraassen 1980, p. 64).

The theory is an artifact, constructed to aid us in understanding nature, not to usurp the function. The target of a theory is the understanding of phenomena, and the construction of a model is the vehicle. Even "sophistication"[26] is positive if it introduces a future possibility for better practical gain, so Van Fraassen is in agreement with Everett in asserting that a theory could certainly have parts which are not yet directly testable, but could constitute a future possibility. For the moment, the exceeding structure serves to reveal structure in the observable phenomena even *indirectly*.

In an empiricist setting of structuralism "scientific theories are viewed precisely as *theories*, not as our sole wherewithal for getting around the world" (Van Fraassen 2008, p. 238). Van Fraassen objects to the structuralist view, opposing the slogan "all that science describes is structure" (which implies that structure as we know it is effectively a natural phenomenon) his own empiricist *motto*, "all we know *through* science is structure", referring solely to the mathematical aspect of

[26] See Van Fraassen (1980, p. 68).

scientific representation, which is our proper standard for the understanding of nature, not nature itself. Those words echo Everett: "[W]hen a theory is highly successful and becomes firmly established, the model tends to become identified with "reality" itself", but it has to be remembered that a theory is just a "logical construct (model), *some* of whose elements are associated with elements of the perceived world" (Everett 1973a, p. 136).

Science designates those areas of the theory which are *isomorphic* to our experience, and empirically adequate. "What is the proper form of acceptance: belief that the theory as a whole is true: or something else?....to accept a theory is (for us) to believe that it is empirically adequate – that what the theory says *about what is observable* (by us) is true." (Van Fraassen 1980, p. 18).

The theory draws a picture of the world. But it is science itself that "designates certain areas in this picture as observable. The scientist, in accepting the theory, is asserting the picture to be accurate in those areas. This is, according to the anti-realist, the only virtue claimed which concerns the relation of theory to world alone. Any other virtues to be claimed will either concern the internal structure of the theory (such as logical consistency) or be pragmatic, that is, relate specifically to human concerns" (Van Fraassen 1980, p. 57). Because, in order to be accepted, a theory should be correct in what it says about *observable phenomena*. That is, the only belief involved is that relative to observable phenomena. But, to delineate what is observable, however, we must "look at science – and possibly to that same theory – for that is also an empirical question"[27].

This does not obliterate of course the distinction between what is observable and the unobservable extra structure, but at the same time, it does not invalidate, nor obliges one to adhere to a specific metaphysical assumption. Van Fraassen's position overlaps that of Frank, and *mutatis mutandis*, Everett's *credo*: a theory could certainly take into consideration unobservable entities, which may become testable in the future; empirical adequacy should be reserved for phenomena that are saved even by unobservable parts of a theory, in an indirect form.

Furthermore, the empirical importance of a theory (i.e., its reference to our perceived reality) is defined from within the science, because it is science itself that delineates the boundary between observable and unobservable entities.

Our conceptual framework changes, even if the real world is always the same.

[27] Van Fraassen also says that this "might produce a vicious circle (the hermeneutic circle) if what is observable were itself not simply a fact disclosed by the theory, but rather theory-relative or theory-dependent. [..] I regard what is observable as a theory-independent question. It is a function of facts about us qua organisms in the world". (Van Fraassen 1980, p. 57).

2.5 Pragmatic selectivity

So the "model", in Van Fraassen's and Everettian language, is a useful representation, which has to be isomorphic to the ensemble of data we utilize to formulate a good hypothesis. But this simply implies an isomorphism between the theoretical model and the data themselves. What has to be specified is isomorphism with the phenomenon one wants to save and the theoretical construal one intends to use, because structures can never have a representation in nature, especially when they relate to phenomena that are never actually represented.
To say that a model has a structure which is isomorphic to that exhibited by phenomena does not mean that phenomena share the same, identical structure, which equals to say that representation is not mirroring, or copying, reality. In other words, the abstract structures are the embedding of chaotic, brutish, arbitrary phenomena. Phenomenon does not have in itself structure: structure pertains to the *selection* of a certain class of phenomena.
The development of a theory follows a triadic trend:

1. The first stage is represented by the single, observable *phenomenon* which is subject to measurement;

2. The second stage is represented by the *appearances* they (the phenomena) present in progressively more abstract outcomes;

3. The *theory*, in which appearances are embedded, is the last stage.

Only the representation of phenomena by means of *appearances* should be isomorphic to the model, the other parts of a theory serve that purpose indirectly.
This description is diachronic: indeed it has to be remarked that a phenomenon (or a class of phenomena) is always selected by a certain theory, because our language and our attention are theory-laden; even when using theory-laden language, a scientist always has the possibility of "bracketing" ontological implications of the theoretical world-picture in which he is immersed (Van Fraassen 1980, p. 81).
The synchronic level introduces the most important aspect: a theory is a *user-dependent* tool. In '*Scientific Representation*' (2008), Van Fraassen revisits his concept of empirical adequacy, moving towards a stronger pragmatism.
What constitutes a relation between a theory and phenomena is always a 3-place relation: the object, the subject and its selective depiction. Representation is in

general three-faceted: reality, appearances (indexical judgments), theory.

We can explain the embedding of phenomena into models just by pointing out that the relationship between the abstract structure and our perceived reality is due to our selective attention to certain aspects and to the representation of them in certain ways.

This is not restrictive: from an empiricist point of view there is no difference between the phenomenon itself and the phenomenon as represented *as thus or so*. And also, it does not imply incompleteness: the essential indexical does not reduce the universality (more adequately, "maximal intersubjectivity" (Van Fraassen 2008, p. 182)) of our theory, if it is said to be empirically adequate.

As van Fraassen remarks "science presents a picture of the world which is much richer in content than what the unaided eye discerns. But science itself teaches us also that it is richer than the unaided eye can discern. For science itself delineates, at least to some extent, the observable parts of the world it describes" (Van Fraassen 1980, p. 59).

The term "observable" is putative, and has nothing to do with existence, because it has no ontological relevance. That is: what counts as an "observable phenomenon is a function of what the epistemic community is (that *observable* is *observable-to-us*)" (Van Fraassen 1980, p. 19).

Observability is variable. It is functional to what the epistemic community retains to be observable: "the structures definable from measurement data are a subclass of the physical structures described. It is in this way that science itself distinguishes the observable which it postulates from the whole it postulates. But since science places human observers among the physical systems it means to describe, it also gives itself the task of describing anthropocentric distinctions" (Van Fraassen 1980, p. 59). In this sense there is a distinction between the phenomena and the "trans-phenomenal" (Van Fraassen 1980, p. 59) in the scientific world-picture, which expresses an anthropocentric confine between what is already knowable (empirically accessible) and what is not yet included in our horizon of perceptions.

In this sense, realism is much more limiting than an empirical assumption of sciences, because the latter permits us to explore more portions of reality than realism does. Physics gives us representations of nature for the empiricist, while for the realist it is a copy of phenomena.

From a purely foundational point of view, theoretical models depict the 'underlying reality'.

But from an epistemic point of view, *some* elements or substructures of those models are meant to represent the observable phenomena – the empirical substructures. Finally, it is a requirement for the theory to at least predict, and if possible to "derive" the appearances, that is to say, the contents of measurement outcomes. This division corresponds to three ostensibly different domains:

- Theoretically postulated reality;
- The observable phenomena;
- Appearances.

In other words, phenomena can be measured and observed in many different ways, because measurement outcomes provide different perspectival appearances of phenomena. So to say that a theory must 'save phenomena' is not the same as saying that a theory must be in accord with experimental and observational results, since measurement outcomes do not show what the phenomena are but how they appear: appearances are always limited, just as the observer cannot see what is behind his back[28].

We cannot transcend observable aspects of reality, which is the act of looking at (or measuring, creating appearances of) phenomena. We could save phenomena, and represent reality as it appears in our observation; but reality is made of observable things, we cannot see reality as it appears from inside. In this sense a theory is nothing more than a model, with a certain isomorphism to perceived reality.

If our model is coherent with perceived reality, namely if the model provided by the theory is *empirically adequate*, than we have a good theory.

For Van Fraassen a measurement is "an operation that locates an item in a logical space (provided by the theory to represent a range of possible states or characteristics of such items)" (Van Fraassen 2008, p. 164)

A theory is a set of theoretical models, which have to fit observed and observable phenomena. So the elements of a theory are:

[28] See Van Fraassen (2008) in explaining how appearance is "compromised" in QM, he says: "This will still be very puzzling [...] if you identify reality with the theoretically postulated reality. Think of this, however, as an empiricist must: the theoretical representation, in which no object state is localized in a finite region of space for more than an instant, is only theory. As is all of our theoretical science. The reality to which it is accountable is only the observable part of the world, and for us that implies that what it is in practice directly accountable to are the appearances-the outcomes of the measurements and observations that are actually made." (p. 308).

- Data model:

starting from the *datum* (data), which is the row result of a measurement, we obtain a smoothed-out summary of the information that emerged from all these data. In data model one summarizes the relative frequencies found;

- Surface model:

mathematically idealized form of data model, in which we replace the relative frequencies counts by measures with a continuous range of values (*probabilities*);

- Theoretical model:

1. a family of M observables (physical magnitudes) each with a range of possible values;
2. a set S of states;
3. a stochastic response function P_s^m for each m in M and s in S, which is a probability measure on the range of m.

Since the description of phenomena is already in practice by means of 'data models' and 'surface models', data and surface models must be isomorphic embeddable in theoretical models.
The reported relative frequencies, in their own summarized form constitute the data model, obtained from the repeated applications of a single measurement. What the theory confronts is abstracted from many data models. The abstracting "is an idealizing, an extrapolation to a form that could not be reached in actual practice" (Van Fraassen 2008, p. 172). This idealization, which is not yet a theoretical structure, is the surface model.
The characterization of this scheme is different to the former form of empiricism, proposed in '*The Scientific Image'*.
The self-declared "Wittgensteinian move" consists then in the following:

> The *theory to phenomena relation* displayed here is an embedding of one mathematical structure into another one. For the data model—or, more accurately, the surface model—which represents appearances, it is itself a

> mathematical structure... Construction of a data model is precisely the selective relevant depiction of the phenomena *by the user of the theory* required for the possibility of representation of the phenomenon... *There is nothing in an abstract structure itself* 'in a context in which a given model is *someone's* representation of a phenomenon, there is *for that person* no difference between the question *whether the theory fits that representation* and the question *whether that theory fits the phenomena.* (Van Fraassen 2008, p.).

The bottom line is that reality consists of smelly, colorful, noisy (observable) phenomena, while appearances are the way phenomena 'look like' in a given measurement set-up, and hence from a particular vantage point. We 'save phenomena' by embedding perspectival appearances (as given by a certain instrument, measurement set-up, or frequencies in a data model) into another abstract structure, the surface model, which 'smoothes' and 'idealizes' the measurement outcomes, and eventually embed the surface model into theoretical models (such as for example Copernicus's geometric models in astronomy).
By using a pragmatic criterion of selectivity, Van Fraassen proposes a non-pejorative sense of "subjective", because the essential indexical has to be labelled as something subjective. To use a theory or model, to base predictions on it, we have to *locate ourselves* with respect to it. We have to locate our situation in the theory's *logical space*, in a way that is similar to our "we are here" with respect to a map.

2.6 Everettian pragmatism

Going back to PWM, with the Everettian theory the relative state is compatible with the phenomena we observe, then we infer from its relativity that there is a connection with other structure. We have no reason to postulate the collapse of the wave function, because we do not have anything that could explain such a disruptive observation, or better, this privileged and anthropocentric position.
We infer collapse while we should infer that our vision is a partial one, and in this way we absolutize our position. And moreover, no matter how the rest of the global state has to be declined, because it is inaccessible (not in the usual sense, but in a deeper way: branches are genuinely *unobservable*, while the motion of the earth is *inexperienced*); empirically speaking, we should be faithful to our portion of observed reality. And faithfulness with our portion of accessible, testable reality

leads to faithfulness to PWM, since we actually perceive one relative state.
Everett himself accepted that we can find our relative state appropriately represented in the correlation structure, so his understanding of actual experience can be put into correspondence with this latest form of *structural empiricism*, and could be considered in the frame of Van Fraassen's *empirical adequacy*: the problem in figuring out 'extra structures' coming from the model, which is the reason for metaphysical interpretations, can be solved by using *pragmatic criteria* to choose in which way the theory should correspond to empirical evidence.
In this way, Everett's interpretation can also be considered an example of van Fraassen's theory.
Indeed there is in Everett's theory an *implicit criterion of pragmatic selectivity.*
That PWM is faithful to our determinate measurement records and to statistical distribution of these records simply amounts to the fact that one can find an *isomorphic substructure to our statistical experience* with determinate records in the model of PWM. The point is that determinate record and probability problems are solved here in the sense that one can find our actual empirical records and our empirically supported quantum expectations in the correlation model of PWM. And furthermore, the fact that there is much more than just our actual determinate records in the correlation model is something that Everett can, and does, embrace when he speaks about homomorphism; but he prefers to focus on our actual experience and in doing this he simply chooses a *pragmatic criterion of selectivity*: empirical faithfulness as Everett understood it "did not require every element in the theory's representation of an observer's experience to correspond to the observer's experience; rather, it was enough that there was an element of the empirical substructure associated with the observer that represented the observer's experience [..] The *surplus* empirical structure was not necessary to explain our particular experience, but he also insisted that its presence did not undermine the theory's explanation since one can find our particular experience appropriately represented in the correlation structure" (Barrett 2011b, p. 704).
In PWM, the *data model* could be represented by the relative frequencies one obtains from elaborating raw data, coming from repeated measurements.
More specifically, one can think of relative states as what one gets when one chooses a physical system S and a state of that system ϕ_S then ignores all components of the entangled global state of the world that characterize S as being in any state other than ϕ_S. If the single remaining component also characterizes the system R as being in state χ_R, then we say that the state of R is χ_R relative to the state

of S being ϕ_S. Given this picture of such relative states in the correlation model, "addressing the probability problem requires only slightly more subtlety" (Barrett 2010, p. 233).

In other words, the relative state is the phenomenon, which comes out of a measuring process.

The *surface model*, which is an idealization of what we observe with relative frequencies, permits us to find in the correlation model our standard expectations.

Indeed, there is a parameter determined by the correlation model that covaries with our standard quantum statistical expectations. It is not the norm-squared of the coefficients of the global state, but it is closely related[29].

In other words, the parameter $|a_k|^2$ can be taken as representing the degree to which the result k *is expected given the usual quantum statistics*. The faithfulness of PWM with respect to the usual quantum statistics simply consists in *finding such a parameter in the correlation model*. And, taken together, that PWM is faithful to our determinate measurement records and the statistical distribution of these records simply amounts to the fact that one can find an isomorphic substructure to our statistical experience with determinate records in the model of PWM.

With the surface model we can give a meaning to data models: in other words, phenomena are recognized as appearances, and in this way they are collocated in a precise region of our perceptual horizon.

The theoretical model provided by Everett is then an explanation of how our expectations co-vary with usual quantum statistics; using Everett's words, the theoretical model provides a correlation model with which we can find ourselves as *located* in the global state, as *typical* elements of it.

We found a frame in which our appearances are explained.

What mattered to Everett was the empirical faithfulness of PWM, which for him involved showing how one might find our actual experience represented in the correlation structure described by the theory. He did not pretend that PWM was the only theory capable of explaining QM, because, as we saw above, he did not believe in anything like 'the correct theory'.

The only requirement for a theory to be a good theory is logical consistence and

[29] See Barrett (2010, p. 233): "Consider the state of the observer and her object system relative to the observer having a record that the measurement was performed but not relative to any particular record of the result. One might renormalize this relative state, then think of it as a state describing the superposition of possible measurement records that would result from a simple measurement interaction on the linear dynamics. Suppose the coefficient associated with the term characterizing the notebook as recording the result k is a_k. Our quantum expectations for result k covary with the parameter $|a_k|^2$ ".

isomorphic adherence with the perceived world. Everett took PWM to be empirically faithful because the values of our measurement records are represented as relative records in the correlation structure characterized by the theory and there is a measure of typicality over relative measurement records that can be determined from the correlation structure alone and that co-varies with standard quantum expectations.

Everett recognized that there was also excess structure represented by PWM insofar as it provided representations of both our actual measurement records and measurement records that do not appear to us therefore unobtainable, this did not bother him since he simply required that one was able to discover our actual experience in the model and be able to understand the experience as being appropriately *typical*.

Typicality is the justification for inferring the relativity of the single state and its coherence with standard quantum expectations.

In this sense, then, Everett explained both determinate measurement records and standard quantum statistics while avoiding the embarrassment of having to insist on a particular metaphysical interpretation of branches. Moreover it is the same theory which affirms unawareness of any sort of branching event.

The pragmatic criterion for acceptability is also confirmed by what Everett indicated as virtues for a theory to be good, in agreement with Frank and Van Fraassen. While he held that one can only require that a physical theory has to be logically consistent and empirically faithful, he also believed that there were various optional, but desirable, pragmatic virtues.

These other virtues make a theory "more" acceptable than others: in a cost-benefit analysis, Everett indicates consistency, simplicity, comprehensiveness and pictorability (a particularly detailed intuitive understanding of the theory's model).

Everett took his relative-state formulation of PWM to be acceptable because it was consistent and empirically faithful, also it was superior to other consistent interpretations of quantum mechanics because it was simpler, pictorable and more comprehensive. PWM as he presented it is indeed *consistent*, it is arguably *simple*, intending here conceptual simplicity rather than ease in use, and it is *comprehensive* in the sense that all physical systems are precisely treated in the same linear way. Comprehensiveness increases our *confidence* in a theory, because its predictive power is more ostensive than the previous ones.

Predictive power is in fact connected to the ability to create new predictions, "unsuspected before the formulation of the theory" (Everett 1973a, p. 134), which is preferable to the mere description of already known phenomena, described as "the role of compactly summarizing known results" (Everett 1973a, p. 134). This "*inertia* of theories" is the stimulating part of scientific research and "supplies a greater motive to theory construction than that of aiding the engineer" (Everett 1973a, p. 134).

Everett in this way justifies the usage of unobservable entities, which are introduced to expand our possibility of prediction:

> It is necessary to say a few words about a view which is sometimes expressed, the idea that a physical theory should contain no elements which do not correspond directly to observables. This position seems to be founded on the notion that the only purpose of a theory is to serve as a summary of known data, and overlooks the [..] discovery of totally new phenomena. The major motivation of this viewpoint appears to be the desire to construct perfectly 'safe' theories which will never be opened to contradiction. (Everett 1934, p. 136).

Explanatory power is not "pure science", but an application of a scientific theory: "it is a use of science to satisfy certain of our desires, and these desires are quite specific in a specific context. The exact content of the desire varies from context to context." (Van Fraassen 1980, p. 156).

The pragmatic dimension of such virtues is implied by "use and usefulness of a theory. In other words, once a theory is evaluated as empirically adequate, more than belief is involved: to accept a theory signifies to make a commitment, a commitment to the further confrontation of new phenomena" (Van Fraassen 1980, p. 88), and these virtues permit us to compare the theory following its usage.

Summarizing, for Everett the branching process is unobservable for direct, immediate reasons: the theory does not provide anything like a branching experience for the memory configuration of observers systems, and this means that it is perfectly coherent with our "factual" experience. It is the theory in itself which indicates the parts of it which designate observable areas: the only thing we can claim is that branches exist, but since the same theory claims that "external" branches are unobservable, it is meaningless to ask what kind of nature corresponds to branches, unless we wish to become involved in metaphysical assumptions, which is exactly what Everett did not intend doing.

With PWM we allow a comprehension of phenomena which is opened to future possibilities, starting from the assumption that the observable is a perspectival, contextual appearance, and allowing for a more general and less anthropological vision of reality, provided that a certain portion of the global state is empirically inaccessible.

Empirical ‘minimalism’ is what is needed together with theoretical ‘inertia’. The fact that a theory has some degree of “sophistication” (intending with this term “introduction of detours *via* theoretical variables to arrive at useful, adequate, manageable descriptions of the phenomena”) does not automatically mean that such a theory is not valid or should be seen as metaphysically filled: “the ‘metaphysical baggage’ will of course not be used when the detour pays off; it is reserved for those detours that yield no practical gain. Even useless metaphysical baggage may be intriguing, however, due to its potentialities for the future[30]”. This kind of sophistication is functional to the usage of the theory, and metaphysics appears here in case the detour is not capable of producing a ‘pragmatic’ pay off, *via* a better predictive power.

For Everett the other interpretations of QM were even more metaphysically committed and limited to the explanation of already established phenomena.

Concluding, there is textual evidence that something like Van Fraassen’s *empirical adequacy* is what Everett had in mind as the proper standard for the empirical acceptability of physical theories more generally, it is closely related to the type of explanation that has a long history of being taken seriously by both physicists and philosophers of science. This notion of faithfulness might be considered *weak* because it allows for *surplus* empirical structure that does not correspond to experience (the notion of Everett’s homomorphism), but after all, in judging the “empirical adequacy of a physical theory one always has some degree of freedom in choosing what aspect of the theory’s model should correspond to empirical evidence, how it should correspond, and how empirical evidence should itself be represented” (Barrett 2011b, p. 707). These degrees of freedom are justified by a pragmatic criterion of selectivity, which is essential in van Fraassen’s and Everett’s, account of empirical adequacy.

As Van Fraassen remarks “Advocates of the Appearance from Reality Criterion will not be satisfied with QM. Some of the resistance may be explained in this way. Even more so the attempts to provide the theory with an interpretation

[30] See Van Fraassen (1980, p. 68) in which the author indicates as a good example the “hidden variable theories in QM: “Such hidden variable models have much extra structure, now looked upon as ‘metaphysical baggage’, but capable of being mobilized should radically new phenomena come to light”.

that restores obedience to the Criterion at some 'deeper' level. Conversely, irenic acceptance of the theory would seem to signal an attitude that is content without any sustained attempt to satisfy that Criterion. This, it seems to me, should allow us to draw the right moral about what are and what are not norms that govern scientific practice". (Van Fraassen 2008, p. 308). Everett shares the same attitude.

If one is tempted to say that Everettian QM is a weak and *ad hoc* explanation, it has to be remarked that it depends on the theoretical approach one decides to adopt: a "broadly Humean view" is satisfied in PWM and in its empiricist understanding. Furthermore, standard quantum mechanics is more *ad hoc* than PWM in Everett's opinion, since it limits its epistemological valence to the classical world, which is absolutized starting from the perspective of the observer, whereas Everettian quantum mechanics constructs a 'democratic' view of measurement, insisting on interaction as correlation between systems; it is untenable to think about a measurement as a final cut in the sense that it is of course, but only from a phenomenological perspective.

Starting from a 'modest' approach of probabilities in the Everettian case, an "alternative strategy" for navigating branching universe will be proposed in the following chapters.

Chapter 3

Everettian *pseudo*-probability

Everett speaks about a qualitative and a quantitative dimension of explanation, once empirical adequacy is ensured.
How can be possible to explain the everettian relative state, without assuming a metaphysical commitment *à la* DeWitt, *quantitatively*?
So far we saw how by looking at Everett's own convictions about physical theories in general it is clear why his own theory could be understood without recurring to a special sort of metaphysics; however, once granted that it is perfectly possible to understand the global state and the derivation of relative states as cross sections, choosing only one among the many possible expansions of the global state, it remains to be understood how this assumptions are declined *in practice*.
In the previous chapter we see how the relative state formulation is capable to explain our determinate experience, which consists of apparently single outcomes when a measurement is made, but we took for granted that, in practice, it could be done: in other words, once clarified that phenomenological and subjective experience is just a relative reality, and once explained that it is so because of unawareness of branching process and the dialectics between unobservability and reality of branches (at a deeper level, if we look at the system from an empirical point of view this perspective is possible and also productive in the sense of future achievements), PWM is then perfectly coherent with perceptions.
If we assume that our state is a relative state, in which determinate records are only appearances, it has to be clarified not only why experience is quite at variance with PWM (in a philosophical way, it has been explained in the previous chapter), but also why experience seems to be perfectly compatible with statistical assertions of Process 2.

Furthermore, in admitting that it is quite possible to have a state which is not straightforwardly coherent with the statistical assertions of standard QM (because all possible states are real, even those in which atypical statistics are observed) then an explanation of how it is possible that this could happen is required.
Now it remains to be seen if PWM could bear the burden of more 'technical' implications without assuming further metaphysical assumptions, focusing on the model, rather than on experience: once found that experience could be appropriately represented in the correlation model, and once clarified that extra structure does not prevent PWM from being empirically adequate, because there is an isomorphic part in it which is perfectly compatible with our experience, now it is necessary to see whether the model can also "explain itself".
Our experience is in fact coherent with statistical assertions of standard QM, and the deduction of the standard model, that at this stage appears to be a "special case" of the more global PWM (as the theory capable of explaining the global state of the universe), should give the reasons behind these statistical predictions, in one way or another, to be complete.

3.1 Old-new problems

There are two practical, concrete and interconnected problems related to the deduction of assertions of standard QM from PWM. As Everettian literature underlines[1], determinate records and the probability problems are the two main hitches in acceptance of PWM, the first concerning the difficulty of justifying one single outcome once established that the superposition principle is the global law, while the second problem deals with the framing of probabilities in a deterministic and causal context, not only in its epistemic weight, but also in its practical meaning (in the end, we *use* probabilistic assertions of the standard formalism when we make a measurement even in an everettian context).
Of course, these are classical problems, as mentioned earlier; but once Everett's original thoughts regarding physical theories in general are taken for granted, and once clarified what could be understood to be his understanding of PWM from an epistemic point of view (and this implies acceptance of relative state formulation as expressing the irreducible subjectivity of observers, without further metaphysical characterizations), the problems came back in a new guise.

[1] See Barrett (1999), and see also chapter 1.

The first problem arises once one tries to deduce the determinate records from the global superposition, and it is strictly connected with the problem of finding a *single* trajectory capable of explaining a *single* memory configuration (a branching observer with his own identity) pertaining to one *single* observer, who will eventually inhabit a branch.

The second problem is that these single records seem to mirror the statistical assertions of Process 1, which is precisely the *probability problem* in Everettian QM.

In this chapter I will try to deal with Everettian problems in the new context of Everettian empirical faithfulness, and show how it is possible to find subjective appearances even in a mathematical, pragmatic way within the theory itself, without recurring to additive factors.

It will be shown how Everett himself proposes a *natural* measure for branches, covariant with the square amplitude, which grants the recovery of statistical assertions and memory configurations *without* recurring to "strange' factors even in a quantitative way.

The quantitative aspect is particularly important with regard to the probability problem, because if it is true that only Process 2 counts as real, while Process 1 is just a manifestation of subjective appearance, it has to be noted that if the probabilistic aspect is illusory, then we require an explanation as to why we still observe it, as observers with specific, time-wise ordered and apparently serial memory configurations (capable of restoring sequences of serial events, while constantly branching from one state into a multiplicity of different but equivalent states). More profoundly, the kind of measure that might substitute probabilities will be explained, and what is its meaning.

3.2 Continuity: identity, memory configurations and uniqueness over branches

The determinate-records problem refers to the experience of the *singularity* of one specific outcome after a measurement is made. Typically, a good observer (which could be a human being, or any kind of servomechanism able to register the outcome with some specific devices) will exhibit one single outcome after a measuring process.

This means that if we start with a superposition of different states for a specific operator (the measured system not being in an eigenstate of the specific measured

quantity) we expect the system, once the interaction has occurred, to exhibit just one of the possible final states of the initial superposition.
Standard formalism assigns the responsibility of *actualizing* just one of the possible eigenstates of the original superposition to the collapse of the wave function, which obeys the Born rule for ascribing probabilities to different coefficients. The irreducible experience of the singularity of just one outcome is the more striking evidence for believing that something like Process 1 has occurred, meanwhile non-awareness of branching collides with our evidence in this case.
The strength of this experience irreducibly seems to witness the correctness of standard QM, while the superposition principle appears to be at least at variance with our experience.
But not only in this sense does superposition seem to be counterintuitive: at the level of the measuring process *uniqueness* of observers is implied without discussion, and obviously related coherence with different memory states is implied and implicitly ensured in standard QM.
In other words, any observer will be seen as a single physical system with single time-ordered records, which are furthermore compatible with the statistical assertions of Process 1[2]. PWM gives a completely different account of reality, in which observers are constantly branching, and nothing like identity, or singularity, could be univocally assumed.
At this point, it seems that isomorphic identity between relative states and our subjective experience is not sufficient for rejecting Process 1, at least if we do not find some kind of *measure* with which we can legitimately "cut" a cross section from the total wave function in a non arbitrary way, and then reconstruct a single *trajectory* over the total branching structure with which we could explain continuity for observers and between observations.
In other words, what it needs is a *quantitative*, in addition to *qualitative*, description of relative states and related statistical assertions.
So, firstly it is necessary to understand how it is possible for observers and their memories to find a sort of 'continuity' in the branching structure.
Single-multiple states which emerge from the branching structure have particular

[2] At this stage, it seems that the discussion of the previous chapter is repeated, and also the 'old' question of the branching process related to an observer rather than to a 'world'. Actually Everettian problems seem to be recurring: once clarified in which sense one problem could be resolved, it comes back while trying to solve another one. I feel that it depends on the difficulty of finding an appropriate explanation which could 'restore' the common sense, and also by the fact that the same problem could be faced starting from different levels. But if one is able to find a measure (which is exactly what is going to be done) the problem could be solved.

features: from an external point of view, they are no more than relative states resulting from the cross section of the global state; at the same time, each of these relative states describes systems with a certain memory configuration and certain determinate experiences, which seem perfectly continuous and linear (in a 'geometric' sense) while we are told that each trajectory follows a "life tree". In fact, from a global-state point of view, nothing prevents *any* possible relative state from existing, and nothing like a linear trajectory is depicted.
Furthermore, against this proliferation of similar states, common predictions of the standard probabilistic interpretation of QM appear to be perfectly in line with experience.
Continuity, in this specific sense, implies that any observer system is supposed to possess *memories*, i.e. "parts of a relatively permanent nature whose states are in correspondence with the past experience of the observer" (Everett 1973a, p. 64), because it is by examining the content of a memory that we can infer that something like 'subjective appearance' has taken place.
Memory also implies that the observer's specific present actions are determined not only by its present sensory *data*, but by the contents of its past memory as well. This appears more evident for sequences of measurements, in which the past results will condition future experiments: the action of an observer at a given instant can be regarded as a "function of the memory contents only, and all relevant experience of the machine is contained in the memory" (Everett 1973a, p. 64).
Everett resumes his idea in a strong metaphor (which, as mentioned in the first chapter, was avoided in the final version of his thesis on Wheeler's advice):

> As an analogy one can imagine an intelligent amoeba with a good memory. As time progresses the amoeba is constantly splitting, each time the resulting amoebas having the same memories as the parent. Our amoeba hence does not have a life line, but a life tree. The question of the identity or non identity of two amoebas at a later time is somewhat vague. At any time we can consider two of them, and they will possess common memories up to a point (common parent) after which they will diverge according to their separate lives thereafter. We can get a closer analogy if we were to take one of these intelligent amoebas, erase his past memories, and render him unconscious while he underwent fission, placing the two resulting amoebas in separate tanks, and repeating this process for all succeeding generations, so that none of them would be aware of their splitting. After a while we would

> have a large number of individuals, sharing some memories with one another, differing in others, each of which is completely unaware of his "other selves" and under the impression that he is a unique individual. It would be difficult indeed to convince such an amoeba of the true situation short of actually confronting him with his "other selves". The same is true if one accepts the hypothesis of the universal wave function. Each time an individual splits he is unaware of it, and any single individual is at all times unaware of his "other selves" with which he has no interaction from the time of splitting. (Everett 1955c)

The *splitting amoeba* well represents the branching of the observer, while retaining awareness of a time-ordered, serial memory.
However, continuity is also related to the object system state: without postulating that a previous measurement affected in practice the system, no other predictions about future arrangements could be made (for example without preparing the system we cannot make predictions about measurements of quantities that are not in eigenstates, etc.).
Something like *memory configurations* (description of events time-wise ordered for the observers even if we are considering servomechanisms) must be found in PWM, it is also necessary to justify in some way the legitimacy of the linear trajectory expressed in the global state (remembering that the global state, expressing a perfectly real superposition, in no way gives a criterion for judging as more real than others some of the singular decompositions); paraphrasing Orwell, it seems that "all branches are real, but some branches are *more real* than others".
Everett recognizes that some 'special' property of observers' systems should be recognized in order to make the correct deductions about the appearance of phenomena as subjective experiences.
These properties should be found within PWM itself:

> In order to accomplish this [i.e. to make deductions about the appearance of phenomena to observers] it is necessary to identify some objective properties of such an observer (states) with the subjective knowledge (i.e., perceptions). Thus in order to say that the state O has observed the event α, it is necessary that the state of O has become changed from its former state to a new state which is dependent upon α. (Everett 1973a, p. 63-64)

This special property is identified with *correlation*, by which the state of a system will be changed by interaction with another system, and vice versa.

Correlation is an *extensive* concept: not only is the correlation between systems indicated as the most important feature, but also, and mostly, the correlation between different states, at different times, is fundamental in understanding continuity. In this way continuity and singularity over time are justified within the relative state formulation. This is the profound reason as to why Everett affirms a strong relativity of states: systems are always interconnected by continuous interactions, and the only isolated system could be understood to be the universe as a whole[3]. This sort of relative correlation[4] is also referable to the role of the observer, played occasionally by systems, which derives uniquely from the perspective one decides to observe from: if correlation is implied by interaction, then all 'contacts' between systems produce some correlation (in no other way can we describe an object except through correlation; this is indisputable for quantum objects, but Everett applies the same principle, and the same PWM, to all physical objects); and such a correlation could occasionally be a measurement process, in the Everettian sense of a "natural" process:

> We now consider the question of measurement in QM, which we desire to treat as a natural process within the theory of PWM. From our point of view there is no fundamental distinction between "measuring apparata" and other physical systems. For us, therefore, a measurement is simply a special case of interaction between physical systems – an interaction which has the property of correlating a quantity in one subsystem with a quantity in another. (Everett 1973a, p. 53)

This produces the effect that "interacting systems are continually "measuring" one another" (Everett 1973a, p. 53).

Correlation is then *primitive* in respect to other apparently more cogent properties; for this reason the role of the 'external' (preeminent and macroscopic) observer, which was the ultimate reason for believing in a drastic change of the observed system, is relativized only if all assertions of the standard model, even in those cases in which a system with a memory configuration was implicitly considered as the observer (noting that in standard QM interactions between classical objects and microscopic system are always of this nature), are deduced with PWM using some sort of quantitative measure.

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[3] This is the reason for the primary reception of PWM as a good quantum cosmology theory (DeWitt 1973), but also for more recent relational versions (Rovelli 1996).

[4] See Rovelli (1996).

In other words, if Everett succeeds in explaining why, *quantitatively*, the observer system *perceives* a determinate record coherent with his serial memory, even if what counts as genuinely real is a life tree, then the theory is quantitatively self-contained (which is equal to explaining analytically and mathematically the unawareness of the branching process described in chapter 2). In technical terms it is equal to finding, within the theory, a correlation model by which memory sequences are perfectly explainable.
The problem is that of treating interactions between systems within the scheme of PWM, so that memory configurations are deduced and interpreted as subjective experiences.
Let us look at Everett's own proposal in detail.
In order to give back the idea of a memory sequence, the state function of an observer has to be described as a sequence of events A, B, $\ldots$, C, of the form $\psi^O_{[A,\ B,\ \ldots,\ C]}$ which represents an observer with a certain sequence of memories (ordered time-wise). A "good" measurement will be one in which the observation of a quantity A with eigenvalues $\{\phi_i\}$ over a system S, and the state of an observer $\psi^O_{[\ldots]}$, which has the initial form:

$$\psi^{S+O} = \phi_i \psi^O_{[\ldots]}$$

will be changed into a new state:

$$\psi^{S+O'} = \phi_i \psi^O_{i[\ldots,\alpha_i]},$$

where α_i characterizes the state ϕ_i. If the system state is an eigenstate, it will be unchanged, whereas the observer will register the value; if the system state is not an eigenstate of that quantity A, but a general state $\sum_i a_i \phi_i$, the final total state will be:

$$\psi'^{S+O} = \sum_i a_i \phi_i \psi^O_{i[,\ a_i]}$$

which expresses a superposition in which each element represents a correlation between a particular observed system state and the object system state as definitely perceiving that specific eigenstate.
With PWM a strong correlation between the object-system state and the observer state is ensured: there is no longer any independent state for the observer or for

the object[5].

This is also the case for more than two interacting systems and for repeated measurements[6]: it follows directly from the superposition principle that each element of the above equation describes the object-system state in a particular eigenstate for the observer, and the observer-system state as definitely perceiving that particular system-state.

Repeatability is the consequence of the relativity of states, which expresses a correlation between a certain eigenvalue and an observer whose memory registers the single outcome.

The final superposition of a composite system *S+O*, after a sequence of measurements in which the quantity A is measured over S_1, S_2, S_n will be:

$$\psi_r = \sum_{i,\ j,k} a_i a_j a_k \phi_i^{S_1} \phi_i^{S_2} \phi_i^{S_r} \psi^{S_{r+1}} \psi^{Sn} \psi^{O}_{ijk[\alpha_i^1, \alpha_j^2,, \alpha_k^r]},$$

which describes each element of the global state as having a definite memory sequence $[\ldots \alpha_i^1,\ \alpha_j^2,\ ,\ \alpha_k^r]$and the relative states of each object system as corresponding eigenfunctions $\phi_i^{S_1},\ \phi_j^{S_2}, \ldots\ \phi_k^{S_3}$.

Every element of the resulting final superposition will describe an observer with a memory configuration, in which the earlier memory will coincide with the later, because memory states are *correlated*.

This is the point in which epistemic contents of PWM find a mathematical and formal answer: because of the superposition principle, an element of the total superposition is appropriately represented with a relative state, and *as* a relative state, by which it is correlated to the state of the object system, while the global reality (the final global wave function) describes a multiplicity of states.

Relativity of states ensures the continuity between present, past and future states, perfectly representing the subjective experience, which is a serial connection of events ordered time-wise. Metaphorically speaking, correlation is the 'scissors' which 'cuts' an apparently linear trajectory from the branching global reality.

That is the reason each initial observation appears to be the consequence of a 'jump' into an eigenstate (when the object system state is not in an eigenstate of the quantity being measured, so the initial state had to be a superposition of eigenstates of the observed property).

[5] This is, in brief, the relative state formulation: from the Superposition Principle Rule 1 and Rule 2 are derived (Everett 1973a, p. 67).

[6] That is Rule 2 in Everett's language: any number of observation processes in any combination could be explained, by applying Rule 1 (by which an observation, in brief, will lead to a superposition) to any kind of measurements (Everett 1973a, p. 67).

Correlation arises from interaction between systems, and preserves "consistency when several observers are present and allowed to interact with one another (to "consult" one another) as well as with other object-systems" (Everett 1973a, p. 13).

Everett devotes large parts of his work to developing a *correlation model*, which turns out to be the heart of his relative state formulation, using probability theory's concepts "without, however, making any reference to probability models" (Everett, 1973a p. 13). The mathematical definitions are then used in terms of information and correlation, without giving them the same probabilistic significance.

Correlation and related probability distributions are then "phrased into information language" (Everett 1973a, p. 34): Everett always distinguishes his own interpretation of quantum formalism from the standard probabilistic interpretation, specifying that the measure used to define correlation, mathematically, equals probability theory, without in any way assuming the same connotation. This allows us to treat interactions simply as relative states, and measurement processes as a particular case of interaction.

Everett then dedicates the third chapter of his doctoral thesis to elaborating a quantitative definition of correlation. He thought that this part of his work was the central core of his ideas, even if in his short thesis it was cut out.

In order to "develop a language for interpreting our PWM for composite systems we will find it useful to develop quantitative definitions for such notions as the "sharpness" or "definiteness" of an operator A for a state ψ, and the "degree of correlation" between the subsystems of a composite system or between a pair of operators in the subsystems, so that we can use these concepts in an unambiguous manner" (Everett 1973a, p. 16).

Everett refers to Shannon's Information Theory (Shannon and Weaver 1949) for developing his correlation model.

The information is essentially a measure of the "sharpness of a probability distribution, that is, an inverse measure of its spread" (Everett 1973a, p. 16). For example, if we have a single random variable X, with distribution $P(x_i)$ the information of X, I_x, will be:

$$I_x = \sum_i P(x_i) \ln P(x_i) \; = Exp\left[\ln P(x_i)\;\right].$$

This means that information is a function of the probabilities alone, and not of any possible numerical values of a random variable.

Once affirmed that after an interaction isolated systems are no longer depicted and describable, because in general anything like an isolated system is no longer found, the state function of a composite system leads to *joint distributions* over subsystem quantities, rather than independent subsystem distributions.

Two variables are correlated if one learns something about one variable when one is told the value of the other variable.

If we have a *joint distribution* $P(x,\ y)$ over two random variables X and Y, then the information relative to one of the two variables also changes the information regarding the other. Indeed if we do not know the value of Y, then the probability distribution of X will be (*marginal* or *a priori distribution*): $P(x) = \int (x,\ y)\, dy$ (the same could be done for Y symmetrically) and our information about X is $I_X = \int P(x) \log P(x)\, dx$.

But if we are told that Y has value y, the probability distribution for X changes to the *conditional distribution* $P_y(x) = P(x,y)/P(y)$, with information $I_X^y = \int P_y(x) \log P_y(x)\, dx$.

The expected change in information about X, given the value of Y, will be:

$$C(X,Y) = \int P(y)\left[I_X^y - I_X\right] dy.$$

This quantity is zero if and only if the two variables are independent, and is otherwise strictly positive, ranging to $+\infty$ in the case of a functional dependence of x on y (perfect correlation).

Correlation is "absolute rather than relative quantity, in the sense that the correlation between (numerical valued) random variables are completely independent of the scale of measurement chosen for the variables" (independence of the coordinate system, or of space-time coordinate transformations) (Everett 1973a, 25).

The relative state function gives the correct *measure* of conditional distribution for all operators, while marginal distributions *cannot* generally be represented by state functions, but only by density matrices.

A state of a composite system leads to *joint* distributions over subsystem quantities which are generally not independent. Conditional distributions and expectations for subsystems are obtained from *relative states*, and subsystem marginal distributions and expectations are given by density matrices.

There does not, in general, exist anything like a single state for one subsystem of a composite system. That is, subsystems do not posses states independent of the state of the remainder of the system, so that the subsystem states are generally *correlated* (Everett 1973a, p. 43).

Conditional distributions and expectations for subsystems are obtained by relative states, which are coherent superpositions, and subsystems marginal expectations are given by density matrices, which are statistical mixtures. Once we know the result of a measurement performed upon a system S_1, then we know the conditional distributions and expectations.

Therefore there exists in general no state for S_1 alone, which correctly gives the marginal expectations for all operators in S_1; even though there is no single state, there is always a mixture of states[7].

Due to the fact that nothing like a single and independent state for a subsystem exists after an interaction, one can arbitrarily choose a state for one subsystem and be led to the relative state for the other subsystem. The correlated state of two subsystems is represented by a pure state (and this follows directly from the superposition principle).

3.3 Everettian '*pseudo*-probability'

Continuity is then associated with correlated states, and is explained without recurring to the collapse of the wave function, identifying single states with branches of a global life tree whose singularity could be associated to a measure for conditional distributions, which grants for their coherence. Once granted that correlation implies the relativity of states, one single state is "chosen" in direct experience. We find ourselves in this singular situation: even if one accepts all possible branches as real, only one at the end is experimented by direct observation; one feels comfortably nestled on one's own branch, so one's state could be pompously taken as the "best of all possible states" (in the sense of "more probable"), and this is not yet a *quantitative* criterion by which we might ensure neither the existence of other more or less different relative states and different branches, nor the 'primate' to our own branch.

What is missing at this level is a more precise definition, in numerical terms, of the global state (in terms of branching structure).

[7] This is called the "canonical representation": a pair of operators for two subsystems are perfectly correlated, i.e. there is a one-one correspondence between their eigenvalues. This depends on representing a composite state as a superposition, while the marginal expectations of single subsystems states are given as a mixture. (Everett 1973a, p. 45-52).

Strictly speaking, there is no fundamental reason why, at this point, we are allowed to think of relative states, intending here *all* of them, as the real entity: in a universe which is constantly branching, in the end, there is no real reason, it seems, to push towards the construction of relative states which are moreover coherent with our perceived reality, or, better, in doing this it seems that an *ad hoc* criterion has been adopted. To do so with a justification we should be able to quantify the global state too.

Once we take our state to be just one of the possible decompositions of the initial superposition, we do not say *anything* about the way in which the global wave function relates to singular states: there needs to be a sort of *measure* over branches in order to ensure an internal coherence to the theory; furthermore, this kind of measure should justify the usage of probabilities, once affirmed their mere operationalist role.

The initial, classical Everettian probability problem returns in a new, more subtle form, once continuity and identity over branches is explained.

A sort of justification in using probabilities is needed in order to understand why our actual relative states are chosen as the "more probable". The problem is to clarify the role of related probabilistic assertions, which always appear coherent with standard QM, and are always coherent with our observations.

In standard QM, our statistical predictions are perfectly compatible with measurement outcomes, and this is the major evidence of the intrinsically stochastic nature of quantum world.

With this consideration probability problem makes its entrance in Everettian QM: putting the issue in other terms, it equals saying that in a world with no probability at all, it is difficult to understand which is the *weight* of our relative state, once affirmed that all states are equally real, and also why we should promote our world to the *status* of 'more probable' than others, while reality is given to all possible worlds. In the end it seems at best tautology asking what the possibility of something which is actual is, and at best it seems nonsense to ask to quantify probabilities in this contest. Wallace calls this the *Incoherence problem*: "What is in dispute is why, and how, this physical magnitude can be probability. One might ask: how can it make sense for anything to 'be probability' in a theory where all possible outcomes occur" (Wallace 2010b, p. 227).

The probability issue in Everettian contest is subtle: it is not just a matter of coherence with our experience, because in this case Everettians would have no need for probabilities (since for every outcome regarded as possible, the theory entailed

that that outcome would occur); the problem is that "to provide an interpretation in which the statistical analysis of the outcomes of repeated experiments provides empirical support for the theory" (Greaves and Myrvold 2010, p. 266).
Conceptually speaking, in PWM the role of probability is completely changed: in standard QM probability expresses the very real essence of the world, while in PWM it is a *mathematical tool*, used only "for operationalist purposes":

> Furthermore, for almost all of the "branches" of his "life tree" that we might consider, the frequencies with which the observer sees the various results of his measurements will follow the probabilistic law of [Process] (1). Therefore, for practical considerations, an observer is justified in using [Process] (1) for calculations; not because the system undergoes any such probabilistic jumps, but simply because he himself will split into a number of observers, to each of which it appears that the system underwent a probabilistic jump. (Everett 1955c).

In its standard formulation, quantum theory seems to imply a strong probabilistic aspect of reality. Probability in that contest is an *objective* feature of reality, and reality appears to be stochastic.
Looking at the philosophical conception of probability, Everett refers to "two types of probability, which may be called subjective or objective probabilities respectively. A subjective probability refers to an estimate by a particular observer which is based upon incomplete information, and as such is not a property of the system being observed, but only of the state of the information of the observer. An objective probability on the other hand is regarded as an intrinsic property of a system, i.e., to what may be called "really" random processes" (Everett 1955b). Objective probabilities are invariant, since they have to be conceived as 'properties', so that they do not depend upon observers' information. But if two observers ascribe different probabilities to some aspect of the same system, then "at least one of these probabilities is subjective!" (Everett 1955b).
For Everett, in standard QM probabilities are imposed as objective "by criterion", even if they have some limitations.
In fact, taking the state function as the expression of objective probabilities of the results of any measurement makes standard QM inconsistent: we can exemplify this by imaging a situation in which a measured system S_1 possesses the state function ψ_1 which gives the objective probabilities (neither 0 or 1, since the state is not an eigenstate of the observed quantity) of the results of the measurement;

but if we now consider the system before the measurement as a total system S_2, it also possesses a state function ψ_2, which is "strictly determined by our initial ψ_2 so long as there are no outside interactions, so that in particular ψ_2 for some time after the measurement has taken place is strictly determined by its value before the measurement. We now consider what this later ψ_2 may say about the configuration of the recording device" and, Everett notes, "if it gives a probability mixture over the configurations, then clearly these probabilities are of the subjective type, since they refer to something which actually exists in a pure state, because in reality the configuration of the recording device has already been determined. On the other hand if this later ψ_2 gives the exact configuration of the recording device, then clearly the outcome of the measurement was determined before it took place, since the later ψ_2 was strictly determined by the earlier, in which case the probability given by ψ_1 was not objective" (Everett 1955b)[8].

For the two systems probability has a different meaning in this context: while for S_1 expresses objective probabilities, ψ_2 expressing a probability mixture gives a subjective meaning to its probabilities. Objective probability does not invariantly depend upon the object, but rather depends upon the observer's choice, and so it is an *epistemic* feature deriving from subjective ignorance. Objective probability relates to the intimate constitution of the object, it has ontological relevance.

For Everett, in PWM the usage of probabilities is limited to their subjective meaning (what is called 'epistemic probability') expressing subjective ignorance (in the specific sense of momentary relative-state ignorance) and referring more to their functional valence from a mathematical point of view.

Everett states his purpose by saying that PWM is a "causal theory which postulates the existence of some sort of wave function for the entire universe, and for which [Process] (2) alone holds" (Everett 1955b). This means that "by assuming the general validity of PWM, without any initial statistical interpretation, we obtain a theory which is in principle applicable to all natural processes, and furthermore one which even leads to probabilistic aspects on a subjective level in a rather novel way (i.e., that we are able to deduce that [Process] (1) will appear to hold to observers" (Everett 1955b).

Everett argues that PWM reduces QM to Process 2, which is a perfectly deterministic and straightforward explanation of the global state, while probability is just another *appearance* once we found the cross section of the total wave function

[8] Everett explains this with an example: if we try to guess which card will be chosen between a shuffled deck of cards, from a subjective point of view, an observer will have 1/52 of possibility of finding ace of spades; objectively, the possibility amounts to Ω (see Everett 1955b).

as representing one single state: it is a sort of '*pseudo*-probability'.
Now what is missing is a *formal* account of this appearance.
The fact is that for Everett subjective reality could *appear* stochastic and perfectly explainable with Process 1, but the global state which is the reality in a stronger sense is deterministic and linear, and explicable with Process 2 alone (the linear evolution of the wave equation), seems to require a more technical explanation: an instrument more general than probabilities is needed, which serves as a *counting measure*, which will be a more general rule, applicable to all processes.
What Everett wishes to obtain is a configuration for trajectories of (all possible) observers through the life tree with specific numerical values. It is now important to deduce the same statistical predictions of standard QM, without giving them a probabilistic significance: what Everett does is to justify its operationalist usage, for predictive purposes, while taking them not as probabilities, but as a *counting measure*. In order to give a quantitative account of PWM, Everett searches for a general scheme for assigning a measure to the elements of a superposition of orthogonal states.
Technically, PWM affirms that the measure over branches covaries with the *square amplitude*. The parameter determined by the correlation model alone covaries with our standard quantum statistical expectations, and it is closely related to the norm-squared of the coefficients of the global state.
So the correlation model provides a parameter following which we can build on our standard expectations, which are in harmony with usual quantum statistics, even if they have no probabilistic significance. As said at the end of the previous chapter, faithfulness accorded to PWM simply consists in finding such a parameter in the correlation model. And once granted that this is all we can expect, we can also ask more of PWM in quantitative terms, by trying to frame this parameter and specifying in what sense it covaries and mostly what kind of meaning has to be ascribed, once we renounce anything like probabilities.
In other words, *branch amplitude* translates into *branch weight*, and no more in branch (outcome) probability.
With the association of the measure over branches with the square amplitude we have not succeeded yet in explaining what kind of measure we are dealing with: in other terms, once affirmed that the square amplitude is the more natural measure for the elements of the superposition, we gave them a number, without clarifying what this number stands for.

3.4 The evidential problem

The association between probability and branches measure is still not clear: "how does probability fit into the story? It is not disputed what physical magnitude is supposed to be (or to stand for), probability: the probability of a branch is supposed to be its weight (i.e., its mod-squared amplitude) [...] one might ask what kind of argument can be given to justify the claim that mod-squared amplitude is probability". Wallace calls this the *quantitative problem* (Wallace 2010b, p. 227). This kind of argument directly introduces us to what has been called the *Evidential problem*, which is the most dangerous threat to Everett's interpretation: the problem is not just to derive the correct probabilities within the theory, but either making sense of ascribing probabilities to outcomes in a situation in which all outcomes exist, and then to consider them as evidence for confirming the theory (remembering that "this sort of empirical testing of probabilistic claims forms a substantial part of the evidence we have for accepting quantum mechanics as a theory that is empirically superior to classical mechanics" (Greaves and Myrvold 2010, p. 265)).
There are two different but strictly correlated points:

1. First, the legitimacy in using the Born rule should be proved, and also it is necessary to see what kind of measure it equals to (probabilities, or a good substitute of them). The same problem could be posed in different terms: how are we to act rationally, if we interpret QM along Everettian lines? Suppose we are faced with a choice between, say, disaster on the spin-up branch and disaster on the spin-down branch. Given only that, whichever choice we make, there will be a disaster branch and a non-disaster branch, how could we ever have grounds for choosing? (call it the *Quantitative problem*, Following Wallace (2010), or the *Practical problem*, following Greaves (2007); both relate the *Equivalence claim*: once established that all outcomes exist, there is no reason for preferring one result to another. Whatever plays the role of probabilities depends only on the quantum mechanical amplitudes: these sort of 'functional probabilities' depend solely on the absolute squares of the amplitudes);

2. Second, this proof must also apply as confirmation for Everett's theory, *via* empirical testing, as in standard QM, in which usually the statistical analysis

of the outcomes of repeated experiments provides empirical support for the theory: this has been dubbed the *Evidential problem*[9].

The evidential problem refers to the possibility of testing Everettian assumptions, and harks back to the problem of empirical adequacy.

Detractors find in probability, using its reliance on empirical evidence, another way out to disconfirm PWM, given that "empirical texting of probabilistic claims forms a substantial part of the evidence we have for accepting QM as a theory that is empirically superior to classical mechanics" (Greaves and Myrvold 2010, p. 265).

Imagine an exemplified situation, in which an Everettian observer (who takes PWM as the true theory) is going to perform a spin-up/spin-down measurement. How should the observer, prior to the splitting, describe the future of her branch? He can think in terms of *subjective uncertainty*: both spin-up and spin-down might happen, but not both.

Or, he can think in terms of *objective determinism*: spin-up will happen and so will spin-down, but not both.

While the last option is universally seen as viable, the first one received disagreement. Both 'Everettians' and 'anti-Everettians' affirm that the Everett interpretation is defensible if and only if subjective uncertainty is viable. Thus, Everettians tend to defend an account of subjective uncertainty to show how Everett interpretation is prosecutable. Anti-Everettians tend not to dispute the claim that if an account of subjective uncertainty were available, then the Everett interpretation could be defended; they argue that it is anyhow incoherent and that, in its absence, one cannot defend the Everett interpretation.

Several attempts have been made to solve the evidential problem.

In the context of what has been dubbed the "fission programme"[10], it has been argued that probability problems (and the evidential problem) could be solved *via* considerations of rationality and behavior (which refer to Lewis' Principal Principle): in other words, whatever probability is, it is assumed as rational to use it as a guide to action.

Some of the participants on the fission programme tried to solve the evidential

[9] Greaves (2007) correctly called it "the epistemic problem: how can we justify believing the theory on the basis of our empirical evidence, if we interpret quantum mechanics along Everettian lines? Given only that the theory predicted that the evidence that we have in fact observed would occur on some branch (and that the same is true of every 'other' possible string of evidence) how can we reasonably take our evidence to confirm the theory?" (p. 3).

[10] See Greaves (2004).

problem by using a decision-theoretic strategy without giving a specific account for probabilities (Deutsch 1999, Wallace 2003, 2007, 2010); others assume that in Everettian QM there is room for genuine uncertainty (Wallace 2006, Saunders 2005, Saunders and Wallace 2008)[11]. These solutions will be examined in the following sections.

A different approach treats the problem from a more general perspective, aiming to demonstrate that a confirmation theory for both Everettian and non-Everettian QM is possible (Greaves 2007, Myrvold and Greaves 2010). Here the confirmation-theoretic role of chances (CC), which states that if S observes something to which a theory T assigns a higher chance than that assigned by other rival theories, then T is confirmed and is applied also to branching theories. Instead of CC, there will be a principle of confirmation-theoretic role of branch weights (CW): if S observes something to which theory T assigns a branch weight higher than the average-chance-or-branch-weight assigned to the same event by rival theories, then theory T is confirmed for S, relative to those theories.

Objective chances have a role in our updating, but they are eliminable: agents with suitable preferences will act *as if* they believe that there are objective chances associated with outcomes, following what a theory T says about these chances (Greaves and Myrvold 2010). Rational agents follow branch weights as if they were chances, and "the occurrence of events to which the theory ascribed a weight higher than the average chance-or-weight ascribed by rival theories increases rational degree of belief in Everettian theory" (Greaves and Myrvold 2010, p. 301). In detail, a rational agent will take relative frequencies data from repeated measurements as informative about values of branch weights, in the same way that they are informative about chances of outcomes. Then the framework recalls Savage's axioms for preferences between wagers: first, it is argued that the rational constraints on the agent's preferences can also apply to experiments thought of as branching events, for maximizing expected utilities; second, in learning the results of experiments, the agent updates his *quasi-credences*, to distinguish them from ordinary credences, and quantify one's concerns, and this is a "*caring measure*", rather than a measure of uncertainties, with Bayesian conditionalization, in the same way as in non-Everettian theories; third, with de Finetti's representation theorem is shown that for an exchangeable sequence of experiments, the agent's credences (quasi-credences) function is a weighted average of certain extremal functions that can be thought of as "objective-chances-or-branch-weights" associated

[11]Note that Greaves (2007) schematizes the problem in semantic terms.

with the outcomes of experiments[12]. Once established that all outcomes exist, there is no reason for preferring one result to another. Whatever plays the role of probabilities depends only on quantum mechanical amplitudes: these kinds of ‘functional probabilities’ depend solely on the absolute squares of the amplitudes. A rational agent should adopt a *caring measure*, contra the equivalence claim: a choice should be made between, for example, two alternative outcomes.

Decision-theoretic axioms and representation theorems applied to PWM are structurally identical to those applied to non-branching theories, and this solves *prima facie* the evidential problem. There is a pervasive structural analogy between chance theories and branching theories, which reflects the analogy between chances themselves and branch weights.

Everett’s interpretation is *no worse off* than any other theory in respect to the philosophy of probability: an Everettian agent would behave just as any other agent would do in the context of rival theories.

Nevertheless, Greaves and Myrvold’s proposal encounters objections.

Part of the critics relates to the caring measure: in a certain sense it seems that the problem has just been postponed. One can always ask: of what kind of credences are caring measures functions, if probabilities are no longer in the field?

It has been objected that branch weights are *not* probabilities, and it seems unconvincing to claim that the agent acts *as if* he believes that branch weights are analogous to physical chances[13].

In addition to this objection, even if now the only rational constraint is based upon the use of the Born rule in guiding agent’s actions, and one can feel free to establish one’s own *preferences* according to them[14]. The functional role of probability now depends upon what is reasonable *for me* to care about. And it is not evident what is reasonable for me to care about.

Moreover, the information one obtains is self-locating, and cannot possibly be relevant to theory confirmation. There is nothing that is evidentially relevant to the truth of PWM. As shown in the previous chapter, such a principle, as Myrvold and Greaves remark, “cannot be sacrosanct”; true, but the claim that “there are in any case many known counterexamples” (Myrvold and Greaves 2010) does not

[12] The demonstration of the confirmation theory is in Greaves and Myrvold (2010), and a slightly different version could be found in Greaves (2007).

[13] See Myrvold and Greaves (2010) for a more detailed description of this objection.

[14] Albert vividly explains how an alternative ‘caring measure’ could be perfectly based upon ‘fatness’: “I decide that the degree to which it is reasonable for me to care about what transpires on some particular one of my future branches ought to be proportional to how fat I am on that branch” (Albert 2010, p. 361).

seem very convincing on this question. Rather, as said before, a new perspective of empirical adequacy would be preferable.

Another important and related objection applies to all possible approaches to Everettian QM, that is, the indisputable fact that there are branches in which non-quantum statistics are observed:

> There are future copies of us who are bound to observe frequencies that do not match the quantum probabilities. (Hemmo and Pitowsky 2007, p. 348)

This amounts to claiming that even for a rational agent which observes *typical* frequencies of results (i.e. whose statistics conform to the Born Rule) the adoption of quantum probability as subjective probability for modeling future actions is completely arbitrary, since there should be *atypical* observers (future copies of us).

Myrvold and Greaves reply that an agent always learns from his experience, and in so doing he is updating his beliefs by conditionalizing on observed data, which is preferable to any other strategy. I think that the reply missed the point: it perfectly explains our branch, but one can still ask what happens in a *non-typical* branch. The discussion about Everettian typicality needs further explanation, and will be addressed in the next chapter.

3.5 Decision-theoretic approach

Greaves and Myrvold try to demonstrate the validity of Everett's theory defining a theory-general confirmation theory in which the principal principle governs quasi-credences instead of credences. The advantage is its universality, it could solve (or better, it is structurally adequate to solve) the evidential problem, because it is applicable to all physical theories.

The ultimate trend is to encompass probability without giving it an objective meaning.

A brilliant solution consists in considering Everettian 'pseudo-probability' in the light of a *decision-theoretic strategy*. This proposal was advanced firstly by Deutsch (1999) and today the strongest proponents are Wallace (2003, 2006, 2010b, 2012), and in part Saunders (2005), even though the latter proposes a different reading, as we will see on the following section.

All accounts of decision-theoretic strategy accord in considering a particular version of the axioms of probability: rather than focusing on probabilities themselves, which are obviously bracketed in a PWM contest, this approach stresses the importance of the *rationality* of agents, which is considered the adherence to probability measure without assigning probabilistic significance (the revisited Born rule). These are in part *pragmatic* constraints, focused on the kind of actions that a rational agent should choose if PWM is the correct physical theory.
In fact, all these contributors agree in assuming the truth of PWM without additional demonstrations, and take for granted the validity of Everett's interpretation in a strong realist version.
Following the decision-theoretic approach, probability in PWM gets its meaning from the rational preferences of agents: a rational agent who knows that the Born rule's weight of an outcome is p is rationally compelled to act as if that outcome had probability p. Even more specifically an agent that does not assume Process 1 to be valid, "or any other probabilistic postulate, but does believe the rest of quantum theory, necessarily makes decisions as if (1) were true" (Deutsch 1999, p. 2). In other words, a rational agent would act *as* probabilistic assertions of Process 1 were true, but without giving them a probabilistic significance.
In classical physics, in situations of perfect knowledge (where one knows all the variables that can produce one outcome, and can calculate how they affect it), a rational agent will choose one outcome among the available possibilities, calculating the highest value.
In QM the situation is quite different: an association between the highest value and one specific outcome could be missing, firstly because a given option might generate a 'range' of possible outcomes, and also knowledge of all the variables is not sufficient to predict which outcome will be effectively observed. These are the reasons why probability is introduced in standard QM, with Process 1.
Decision-theoretic strategy used in this sense, in general, affirms that PWM does *not need probabilistic axioms*. An Everettian agent could be defined as an agent who genuinely believes only in Process 2 (a supporter of PWM, in more general terms), and whose rationality refers to "a strictly non-probabilistic restriction of classical decision theory"[15] (Deutsch 1999, p. 13); he then will decide upon the

[15] Deutsch (1999, p. 2) writes: "The decision maker is rational in the standard decision-theoretic sense, except that, to avoid circularity, we must omit from the definition of 'rationality' anything that refers directly or indirectly to probabilities. In particular we must not make the standard assumption that a rational decision maker maximizes the expectation value of his utility. In this approach, such propositions are to be proved rather than postulated. 'Rationality' in this restricted sense means conformity to a set of constraints on a decision maker's preferences. For

prediction of outcomes following Process 1 ("as if those outcomes were determined by stochastic processes, with probabilities given by axiom (1)" (Deutsch 1999, p. 13)), maximizing the expected value of his utility in a *Savage-style* Representation Theorem: the measure over branches plays the role of credence measure.

If the decision-theoretic strategy succeeds in demonstrating the validity of the Born rule deprived of probabilistic assertions, then "quantum theory permits what philosophy would hitherto have regarded as a formal impossibility, akin to deriving an 'ought' from an 'is', namely deriving a probability statement from a factual statement. This could be called deriving a 'tends to' from a 'does"' (Deutsch 1999, p. 9).

To depict it more clearly with an example, imagine a classical bet context of coin tossing: suppose a coin is to be tossed in five minutes time, and suppose that an agent bets five dollars (at even odds) that it will be heads. Looking solely at the pragmatic interests of the agent, imagine taking into consideration the only two possible sets of results:

1. The agent wins: the coin lands heads, in this case obviously the agent will be pleased;
2. The agent loses: the coin lands tails, in this case the agent will be less delighted, but, as Wallace remarks: "he may well still regard the bet as having been the right choice *given his information before the coin toss*" (Wallace 2010b p. 229, italics is mine).

The agent, in other words, weights the cost-benefits of his betting with the information about the possibility of ending with heads or tails. Decision theory formalizes the weighting in assigning some utility to payoffs, in the form of a real number: $V(+5)$ stands for receiving five dollars, while $V(-5)$ stands for losing five dollars, also $V(0)$ stands for neither gaining nor losing it.

Now, if $Pr(H)$ is assigned to heads, the risk should be taken only if:

$$\Pr(H) \ \times V(+5) + (1 - \Pr(H)) \times V(-5) > V(0).$$

In other words, once a probability is assigned to each outcome, the agent assigns

example, his preferences must be transitive: if he prefers A to B, and B to C, then he must also prefer A to C. Transitive preferences can be summarized by assigning a real number – a utility value – to each possible outcome, in such a way that the decision maker prefers higher-valued outcomes to lower-valued ones".

a utility to each bet, and chooses the "option that maximizes expected utility with respect to those assignments" (Wallace 2010b, p. 230)[16]. For any preference there is a *unique* probability function so for one bet there is only one expected utility (the higher, the better), and this directly connects to the rationality of an agent (if the agent's preferences cannot be so represented, then the action is to be considered irrational)[17].

In the Everettian version of the coin toss, instead of two different possibilities, there are two interestingly different sets of branches: the head-landed branches, whose denizens are winners, and the tail-headed branches, where inhabitants are losers. In this picture the rational action of the agent changes: now he has to consider a different cost-benefit analysis, based upon the *weight* of the branches on which the head is up[18].

An Everettian agent (again, an agent who assumes that PWM is correct) knows that a measurement will produce a splitting, after which there will be multiple outcomes, and different versions of the agent[19]. The relative weights of branches

[16] It has to be remarked that "Prima facie, it isn't at all obvious that he should try to maximize expected utility rather than, for example, maximize utility with respect to the square of the probability function; or maximize the logarithm of utility; or just maximize the utility of the least desirable possible outcome" (Wallace 2010b, p. 230).

[17] There is an important remark to make here: "There is an important weakness in the decision-theoretic argument which needs to be stressed. It is proof that rational agents must bet according to some probability function, but it is silent on the connection between that function and the 'real' probabilities. No decision-theoretic principle is contradicted by an agent who assigns probability 99/100 to an apparently fair coin landing 'heads', for instance. A minority of advocates of the decision-theoretic approach simply deny that there is any such thing as 'objective' or real probability; the majority just take it as a bare postulate that an agent should conform his subjective probabilities to the objective probabilities when he knows the latter. This weakness actually rather undermines the use of probabilities as a criticism of the Everett interpretation, even without the arguments in this paper: if classical probability can give no justification of its probability rule, why ask the Everettian for such a justification?". Wallace soon adds that "we can prove and not merely postulate the link between those probabilities and the quantum mechanical weights" (Wallace 2010b, p. 231), intending a merely operationalist usage of probabilities.

[18] A sceptic may argue that the status of the agent, in the Everettian context, is completely changed, and it enters the decision, because there are several copies of observers now. This does not change things however, because what really counts is the future gain, no matter how many copies of the rational agent could be produced: "an Everettian agent can be in a decision problem just as can a non-Everettian" (Wallace 2010b, p. 232).

[19] It is extremely important to remark here that the ontological frame in which Wallace (and also Deutsch) fits the decision-theoretic strategy is the decoherence-version of PWM, which imposes the acknowledgement of a certain structure on the Hilbert space. The resolution of the identity on the Hilbert space implies "a decomoposition of the space into subspaces, with each subspace π corresponding to a possible macrostate. The choice of macrostates is largely fixed by decoherence, although the precise fineness of the grain of the decomposition is under-specified. . . we call a macrostate available to an agent if there is an available act which, when performed, leaves some of his future selves in that macrostate" (Wallace 2010b, p. 233). The model is artificially precise then, because the decomposition will appear precisely specified.

are given by the Born rule, applied to the post measurement state of the system and of the measuring device (considering here the 'factual' importance of the Born rule: after a measurement has taken place, by considering the result on my own branch I can account for weights, and rationally update my credences). Two different categories of axioms will explain the agent's acts: "*richness*" axioms, concerning which acts are available to the agent (how rich the structure of the set of acts is), and axioms of "*rationality*", connected to the agent's preference order. The richness axioms are:

- *Reward availability*, in which all rewards are available to the agent at any macrostate, including the set in which all the agent's future selves are rewarded;
- *Branching availability*: an agent can always choose an act with different macrostates as possible outcomes, and give to each a specific weight p;
- *Erasure*: given a pair of states $\psi \in E$ and $\phi \in F$ in the same reward, "there is an act $\hat{U}$ available at E and an act $\hat{V}$ available at F such that $\hat{U}\ \psi = \hat{V}\phi$" (Wallace 2010b, p. 235). Erasure implies that an agent can forget any facts about his situation that don't concern things he cares about;
- *Problem continuity:* For each event the set of available acts is an open subset of the set of unitary transformations from E to $\mathcal{H}$[20].

The axioms of rationality are 'dictates' which dominate an agent's rational actions: the first is that an agent's preferences must follow a total *ordering* of his actions (it is not possible to violate an ordering); the other implies *diachronic consistency*, in which an agent will prefer the succession *U-V*[21] to *U-V'* if he knows that his successors will prefer *V* to *V'* (even if I could *locally* violate this constraint, for example in telling my friend not to let me order another drink after my second, but those are to be considered irrational, conflictual, and localized situations)[22]. Now, the decision-theoretic strategy applied to PWM implies that a rational consideration is that of assigning to a set of branches of relative weight w a probability w: the rational agent acts *as if* the Born rule were true. And this is enough to

[20] For a more accurate description of axioms of richness, see Wallace (2010b).

[21] U here stands for unitary transformation: since in PWM preparations, measurements, and payments made to agents are all physical processes, for simplicity any sequence preparation-measurement-payments can be represented by a single unitary transformation.

[22] See Wallace (2010b, p. 236) for a more accurate description of the dictates of rationality.

justify the general role of probability in PWM.
Certainly an agent can refer to the Born rule for maximizing expected utility, and that would be a possible strategy, which equals Everettian agents to non-Everettians: in other words credence tracks the Born rule in the same way as in classical decision-theoretic contest credence tracks chance, so the squared modulus of the amplitudes plays the same role of chances in a one world picture. That is equal to saying that the Lewis' Principal Principle (which in this non-Everettian scenario is the quantitative link between chances and subjective degrees of belief) is derived in Everettian QM *via* the Born rule.
But, adds Wallace, this is also the *only* possible strategy: any other strategy rather than the Born rule violates some rational constraint on action. Wallace then gives an axiomatised version of Deutsch-Wallace Quantum Representation Theorem[23], which formally demonstrates the unique validity of the Born rule as a rational constraint.
Summarizing, this genre of approach tries to justify the usage of probability by using constraints of rationality, without giving *classical* decision-theoretic-like arguments for doing it (i.e., arguments directly referable to probability).
In this sense probabilities are *functions* of branch amplitudes, and are no longer seen as fundamental; rather this sort of "reduced" to "emergent features" (Saunders 2010b, p. 182).
The rational agent is then no longer compelled to refer to probability in a profound sense, mostly because probabilities in the Everettian contest are no longer asymmetric. The older idea of symmetric probability (related to the idealized throw of dice of the 17th century) made every possibility *equiprobable*. Real physical facts (or real dices!) are then shown to be dependent upon initial conditions, and symmetry is broken: we have to impose a certain probability distribution on the initial conditions, and "any prospect of a reductive analysis of probability is lost" (Wallace 2010b, p. 262). In Everettian QM the symmetry is still maintained: all outcomes are actual, and there is no requirement for the perceived outcome to break the symmetry (the program of deriving probabilities from a symmetric reality is then viable).
In general, these efforts to explain the use of the Born Rule in Everett encounter severe critics (Kent 2010, Albert 2010, Price 2010).
Criticism refers to the inability to prove the correctness of Born rule in PWM using an account of rationality instead of a chance notion.

[23] See Sections 7-8 (Wallace 2010b, pp. 240-254) for the formal proof.

Even if the effort is commendable, and is a “sensible project” with “sensible motivations” (Kent 2010, p. 308), they result unconvincing: there is still no evidence for believing that a decision theoretic approach could give the same sense to the Born rule as in the standard one-world QM; at the end of the day the Born rule still refers to probabilities, and furthermore, it is in standard QM that probabilities are expressed in their strongest form. As mentioned in Myrvold and Greaves confirmation theory, it seems that, despite efforts to yield probability into a branching context, they are in the end still limiting.

Besides, decision-theoretic supporters all start from a very inflexible requirement: they all require us to *imagine* an Everettian observer, who already believes in PWM.

PWM is in this way *presupposed*. This is something quite different to Everett’s own deduction of the Born rule in the statement of PWM itself, which is fully in line with his search for a self-compelling theory.

As Albert remarks “the questions to which this program is addressed are questions of what we would do if we believed that fission hypothesis were correct. But the question at issue here is precisely whether to believe that the fission hypothesis is correct!” (Albert 2010, p. 359). In order to establish if PWM can be tenable, one just has to look at its *empirical adequacy*[24] (see chapter 2), while decision-theoretic programme takes *betting-style decisions* in the first instance. And that is “crazy”, using Albert’s words, because even if it could be demonstrated that rationality of Everettian agents amounts to bet as any other (one-world) agent would do (following the Born rule, as if Process 1 were true), “that would merely show that circumstances can be imagined, circumstances which are altogether different from those of our actual experience, circumstances in which the business of betting X has nothing whatsoever to do with the business of guessing whether or not X is going to occur, in which, as it happens, we would bet just as we do” (Albert 2010, p. 359).

Apart from a different criterion for establishing appearances and attributing empirical adequacy to them (which obviously denies the existence of any sort of branching process), the argument is correct, I think: decision-theoretic strategy, and also

[24] Albert does not know, however, that PWM is capable of empirical adequacy: “And that experience is of certain particular sorts of experiments having certain particular sorts of outcomes with certain particular sorts of frequencies – and not with others. And the fission hypothesis (since it is committed to the claim that all such experiments have all possible outcomes with all possible frequencies) is structurally incapable of explaining anything like that.” (Albert 2010, p. 359).

Greaves-Myrvold confirmation theory, merely use the same non-Everettian rational constraints for actions applied in an Everettian context, claiming a universal application of it, and taking this rationality as a prior criterion for judging the validity of the Born rule.
In doing this, PWM is no longer self-compelling, and needs an extra postulate which might appear to be something like:

> *When deciding to bet between two possible outcomes, the rationality of agents (who believe in PWM) will impose acting maximizing expected utilities following the coefficients indicated by the Born rule*

which is equal to saying that the preferences (even rational, but always preferences) of agents are in line with the Born rule. This is a *partial* explanation of the Born rule in PWM, because it is equal to saying that it is correct because it has *normative* valence for rational agents. The contrary should be done: because of the correctness of the Born rule, an agent should certainly make his decisions following it. Rationality of agents could certainly be based upon the discovery of the correctness of PWM, but an element of explanation of the Born rule in PWM is missing.
Furthermore, rationality in a branching context might appear to imply "something new for agents to have preferences *about*" (Price 2010, p. 378).
Again, something quite new seems to be required, to justify an operationalist use of probabilities.

3.6 Ignorance and chance

In Deutsch-Wallace's account of Everettian probability something in the real, physical world provides a guide for rational decision regarding maximizing expected utilities.
There still remains a problem, however: fission programme contributors focus on the translation of classical rational constraints in an Everettian picture, without worrying about the kind of underlying probabilities (that they take, it is not so important: they are replaced by the Born rule, so their *normative* power is ensured while avoiding the embarrassment of explaining their meaning).
This means that PWM refers solely to the link with statistics and rationality alone

("if it is to mean anything" as Saunders remarks (Saunders 2010b, p. 183)).
But, even if the Deutsch-Wallace approach solves the *practical problem*, i.e., the "normative problem of how agents who believe they live in a branching universe ought to achieve their ends (without any prior assumption about probability)" (Saunders 2010b, p. 184), the solution remains limited to agents who assume PWM to be a true universal theory; but it has "no direct role in confirming or disconfirming EQM as one of a number of rival physical theories by agents uncommitted to this truth" (Saunders 2010b, p. 184), and this leaves the *evidential problem* unsolved.

With their more general Bayesian confirmation theory, Greaves and Myrvold give a more general account of representation theory, applicable both to Everettian and non-Everettian (or better still, to quantum and non quantum) contexts; but again they do make reference to a notion of probability which requires further explanation.

The route for chance and ignorance in Everett has been traced mostly by Vaidman (1998) and Saunders (2005, 2008, 2010b), among all.

One can think of the possibility of uncertainty in an Everettian universe: if chance has to be thought of as a physical magnitude, it should, at least indirectly, be measurable, like anything else.

What Vaidman and Saunders try to do is add a definition of what chances really are or are meant to be in an Everettian picture: in a one-world theory it is easy to demonstrate that they link to uncertainty. In an Everettian, deterministic QM in which all outcomes exist and, once accepted, are known to exist, the link is not clear.

At this point it seems that these authors are denying what Deutsch-Wallace and also the more general Myrvold-Greaves Representation Theorems aim to do: the reduction of probability to a mere quantitative measure, deprived of any stochastic significance. In the end, isn't it a privilege to abandon an indeterministic account of QM to demonstrate how a deterministic, straightforward causal theory, such as PWM might succeed in explaining all physical facts without the embarrassment of having to do with uncertainty?

But the reasons for deriving a notion of uncertainty from chance in Everett are subtle.

Saunders' "cautious functionalism" aims to demonstrate that with a reduction of probabilities as emergent from the branching structure, the *evidential problem* is solved, and the derivation of the Born rule *via* Wallace's Representation Theorem succeeds in giving to PWM the same quantitative results that probabilities do in classical contexts. Furthermore, the task of identifying objective chances in Everett is solved at least as in the classical case with Myrvold and Greaves' analysis:

> As an objection to the Everett interpretation, the problem only arises if it is granted that branches and branch amplitudes cannot be identified with chance and probabilities (which we take to mean are not in fact quantities that satisfy (i), (ii), (iii))[25], precisely the point here in contention. Show that they can, and the evidential problem simply dissolves. Or rather, since it can be read as a problem for every chance theory, it will have the same status in the Everett interpretation as it has in any other physical theory of chance, to all of which Greaves and Myrvold analysis applies. (Saunders 2010b, p. 185).

The link to uncertainty relates to *ignorance* of rational agents even in Everettian QM: Saunders argues that an Everettian agent has the same ignorance about the future as a non Everettian, if we think of it as an "in-branch" problem. The presupposition in which only one outcome happens (of two mutually incompatible, observable outcomes) is applicable also to Everettian in-branched events, since an Everettian observer can never witness two outcomes simultaneously. So, if chances are identified with the functions of branch amplitudes, the former will always be associated with incompatible observable outcomes. Ignorance is used here in Vaidman's sense (Vaidman 1998): an agent is ignorant one moment *after* the measurement has taken place (before looking at measuring apparatus).
It in this sense that Saunders' 'relativity' of ignorance should be read. Once identified chances with functions of branch amplitudes it is shown that:

1. Chances will be associated with incompatible observable outcomes, only one of which can happen at one time;

[25] With (i), (ii) and (iii) Saunders refers to three known statements about probabilities in general: (i) the link between chance and statistics ("chance is measured by statistics, and perhaps, among observable quantities, only statistics, but with high chance"); (ii) the link between chance and subjective degrees of belief, or credences (the Principal principle); (iii) the link between chance and uncertainty (see Saunders 2010b, p. 181).

2. They may not be measured in a way that is not itself chancey, and single cases cannot be measured at all, they can only be measured by running a chance (branching) process repeatedly[26];

3. None of these facts can be explained by any conventional physical theory of probability (rather, they are presupposed).

Alice is an Everettian observer who is going to perform a measurement, she is convinced that PWM is true. She will know that after measurement, each of her 'copies' will have a single, different result in each branch, but she does not know which particular result she will obtain until she looks at the measuring device (Vaidman closes Alice's eyes, in order to make the example more vivid). This state of ignorance refers to in-branch information, while looking at the global state, she is able to say quantitatively what results she is likely to obtain.
In other words, ignorance refers to the *actual* post-measurement situation of the *actual* branch in which a measurement has just been performed: the indexical, self-locating knowledge in this specific sense is missing, and relates to the impossibility of saying *who* and *where* Alice is to be collocated between the possibilities of the global wave function. In this sense, branching events are "algorithmically uncertain too – they are *indeterministic*" (Saunders 2010b, p. 193)
This kind of momentary ignorance translates into uncertainty depending on the reading one decides to give to PWM: using consistent histories formalism, as Saunders does, this momentary lack of certainty becomes the classical uncertainty related to probability.
If a 'person' (or 'thing') is taken to be a branch-part or ordered pair ($\langle \beta, |\alpha\rangle \rangle$) where $\alpha \in \beta$, then she/it is a sequence of configurations α obtained by fine-graining of β[27].
In this respect Alice is genuinely uncertain before the branching process, and gains knowledge, specifically self-locating knowledge, but in this case it is knowledge about her *consistent history*. Vaidman's ignorance is then applied to the whole branching process, whose dynamics are defined over time and not instantaneously (as Saunders remarks, "the idea of a decoherence basis, defined at an instant of

[26] It refers to the no-go fact by which "there is a no probability meter that can measure chance on repeated trials with chance equal to one" (Saunders 2010b, p. 185).

[27] Consistent histories formalism gives a different representation for subjects, and objects: following the Heisenberg-picture vector signifies treating Alice "strictly as a momentary thing" ($|\beta_j\rangle = P_{\beta_j}(t_j)\,|\Omega\rangle$, where Ω is the universal state); in the consistent histories formalism Alice is represented by an ordered triple $(\beta_j, \beta, |\alpha\rangle)$which means treating Alice as a fine-grained sequence of configurations over trajectories of β.

time independent of what comes before and what comes after, is a fiction" (Saunders 2010b, p. 192)).

In this sense, Vaidman's ignorance concept is extended over time, not limited to a specific instant: ignorance *á la* Vaidman is ignorance that each Alice was not already limited to an extraordinary and momentary blindness.

Even if Everett advocated a sort of subjective ignorance in preferring a subjective (epistemic) definition of probability as we saw previously, this account of ignorance surely depends on the metaphysics of personal identity one decides to adopt, and of course this was not an Everettian concern, to the extent that continuity was easily explained with correlation and relativity of states (in an 'Everettian-style way', avoiding unnecessary speculative problems, in line with his strong empiricism).

But it has the virtue of stressing the uncertainty concept in a context in which it apparently seems that no space for ignorance, apart from post-measurement occasions, is left. So the measure of branch amplitudes arising in the representation theorems express genuine credences, deriving from genuine ignorance about histories

Is the probability problem solved? Something puzzling and unconvincing seems to emerge from this approach.

Papineau (2010) argues that orthodox ignorance-based thinking about probability is incoherent in an Everettian context and it is a "disservice": it could be the case, if it takes a strong metaphysical installation, and it is the case, if we consider that ignorance *á la* Saunders pays lip service to a strong metaphysics of identity. And furthermore, it opens the road to a very difficult issue, that of overlapping versus divergent worlds[28]. Underlying these approaches lies the biggest danger of becoming related to the many minds theory. In fact, they appear to be irreducibly subjectivist approaches: if "one fleshes their ideas out into a fully coherent and complete interpretation, one would necessarily arrive either at the many-minds interpretation or something even worse"[29].

These new indexical uncertainties are strongly related to an undisputed priority attributed to the subject: "completely new centers of subjectivity, from the standpoints of which completely new and previously unformulable indexical questions can come up" (Albert 2010, p. 368).This kind of ignorance is not 'genuine' uncertainty. As Albert notes, it refers to the measurement setting, and "has nothing

[28] See Saunders (2010b) for more details.
[29] See Kent (2010, p. 311).

whatever to do with objective metaphysical features of the world, and they are not the sorts of uncertainties that can only arise by means of forgetting" (Albert 2010, p. 368).

The other problem relates to statistics: it is not obvious why we observe only the more probable outcomes. What about the less recurrent (atypical), again?

It seems that subjectivity, or rationality, deserves a more general explanation.

I feel that the point is that all these approaches reduce decisions of rational, in-world agents (regardless of the branch they inhabit) to a context in which what should count as real is the global state.

In one way or another, the problem with Everett's original proposal is not centered. Even regarding the probability problem Everett in fact gave a more *pragmatist* account focused on the utility of squared amplitude once affirmed that what counts as real is the global state. It is the global state which gives reason to the correlation between states, and it is also the Born rule which gives the counting measure. There is no need to find anything like a privileged observer, and even if those approaches aim to explain the Born rule globally, they all take for granted an assumption which is at the very basis of Everett's own thinking, and that, if stressed, could also give the reason for believing in the Born rule as the correct counting measure replacing probabilities: the Born rule expresses degrees of *typicality*, similar to classical statistical mechanics.

Once this assumption is established, then decision theoretic strategies could be seen as a normative strategy, in a subjective way. What has to be said firstly is that prior to decision theoretic accounts there is the more general concept of typicality: following the coefficients of the square amplitude, the overwhelming majority of branches are typical.

Chapter 4

Typicality: all branches are equal, but some branches are more typical than others

We can see how the most recent developments of Everett's notion of probability try to justify the usage of Born rule giving a pragmatist explanation, but turning it into a *subjective* approach.
Even if they all have the merit of explaining why, from a subjective point of view, it is acceptable to use the statistical assertions of Process 1 for "operationalist purposes", they do not succeed in explaining why the world is rather understandable as a deterministic, linear, isolated system following the superposition principle (Process 2) *globally*, because they all start from the assumption that the world *is* definitely explainable with a superposition of all possible states (they take it for granted, assuming that an Everettian agent *already* believes PWM is true).
A *bridging* element between the pragmatic, subjectivist dimension of decision-theoretic approach and the global state is missing, I argue: in fact, decision-theoretic links to probabilities should be followed 'later', intending that they should arrive at a later time, only when explanation of what the Born rule essentially stands for, if not probabilities, has become clear.
This bridging element, following Everett's original purpose, is *typicality*.
Typicality turns out to have the same mathematical advantages of probability measure, without the same epistemic valence. Of course justification is required in order to use it. If typicality is not a measure of probability, the kind of measure we have must be explained (or, what typical is).

4.1 Measure *versus* probability: the missing piece

In the previous chapter we saw Everett's purpose in using probability theory as a mathematical tool. Mathematically, the square amplitude covaries with our expectations.

It is now necessary to understand whether the mathematical equivalence translates into something different, before using the Born rule pragmatically, for subjectively orienteering our predictions.

With the search for a *measure* which mathematically equals probability, without in any case implying it in an epistemic sense, Everett introduces the notion of *typicality*. The *measure* equals the probabilistic assertions of Process 1 without giving it the same valence of objective probability:

> In order to establish quantitative results, we must put some sort of measure (weighting) on the elements of a final superposition. This is necessary to be able to make assertions which will hold for almost all of the observers described by elements of a superposition. In order to make quantitative statements about the relative frequencies of the different possible results of observation which are recorded in the memory of a typical observer we must have a method of selecting a typical observer. (Everett 1973a, p. 123).

This is precisely the sense in which Everett distinguishes between *measure* and *probability*: with the first term he refers to branches counting, while with the second term he refers to objective probabilities. The concept of measure itself excludes a probabilistic usage of the squared amplitude, in objective terms.

It will be shown that the parameter which covaries with the square amplitude is the expression of *typicality*: the coefficient simply expresses a counting measure related to the degree of typicality of each state (or branch). In other words, what Everett introduces is a method of selecting a *typical* observer.

A *typical* element of the final superposition will perceive an apparently random sequence of definite results after an observation: for a *typical* element of the final superposition, it will appear that assertions of Process 1 are perfectly compatible with experience, and this derives directly from the relative state formulation, i.e. from the superposition principle.

4.2 Typicality in PWM

Why is it the case that for each state we can associate a *value*?
Furthermore, the second problem related to probability is more subtle: where this measure for branches came from - why do certain branches have a certain coefficient while others have a different number?
Everett takes the square-amplitude distribution to be used in his PWM, with no reference to probabilistic assertions. What does this measure mean in Everettian context?
If the total wave function is perfectly deterministic and stochastic elements are avoided, because all states exist simultaneously in the sense described in the previous chapter, and relative states are just a derivation of the cross section of the global state, which appropriately represents the appearance of determinate phenomena to observers, it remains to be understood why it seems perfectly evident that 'reality' (meaning *global* reality) follows the coefficients of the Born Rule.
In a very subtle form, "in order to make quantitative statements about the relative frequencies of the different possible results of observation which are recorded in the memory of a typical observer we must have a method of selecting a *typical* observer" (Everett 1973a, p. 124).
Typicality in Everettian contest is the *bridging element* between perceived reality and what the theory says about the entire universe. But it relates to the global state rather than being a subjective feature: in other words, the difference between decision-theoretic-like links and Everettian typicality is that it expresses a *real* characteristic of the world, from the global perspective, rather than from the subject's point of view.
Almost all branches exhibit the same statistical predictions of standard QM, and they are *typical* branches, but some branches will have different predictions, and they will be *atypical* branches.
Through the counting measure, or better the weight, of branches, which mathematically equals the square amplitude we can quantify *typical elements* of the final superposition exhibiting standard statistical distributions.
So the square amplitude expresses the degree of typicality, following which the agent will model his degree of belief.
The problem of identity over branches, which has been deduced as a consequence of correlation between states, is explained using a *measure of typicality*, which also grants the possibility of finding one's memory configuration in the life tree, in the

same way as in classical statistical mechanics a trajectory over the phase space is considered typical.

Typicality allows the deduction of statistical assertions from within the theory itself: the theory should be capable of explaining why statistical assertions appear to be valid to *almost all* of the observers described by elements of a superposition. Furthermore, "maverick" elements, or trajectories, are described as being simply *atypical*, and this is a good explanation for unusual statistics (which have always worried non-Everettians).

Our deductions must hold for almost all observers described by elements of the superposition; this means that the memory sequence of a typical observer should be coherently represented by a quantitative measure:

> In the language of subjective experience, the observer which is described by a typical element, $\psi_{ij\ldots k}$, of the superposition has perceived an apparently random sequence of definite results for the observation. It is furthermore true, since in each element the system has been left in an eigenstate of the measurement, that if at this stage a redetermination of an earlier system observation (S_l) takes place, every element of the resulting final superposition will describe the observer with a memory configuration of the form $[\ldots,\alpha_i^1,\ldots\underline{\alpha}_j^l,\ldots\ \alpha_k^r,\ \underline{\alpha}_j^l]$ in which the earlier memory coincides with the later-i.e., the memory states are correlated. (Everett 1973a, p. 70)

Everett says that, speaking in terms of subjective experience, the observer that is described by a *typical* element of the superposition has perceived a supposedly random sequence of definite results for the observation (while they are all actually real in the global state).

Such a typical element will perceive that something like a collapse has occurred after measurement:

> It will thus appear to the observer which is described by a typical element of the superposition that each initial observation on a system caused the system to "jump" into an eigenstate in a random fashion and thereafter remain there for least, the probabilistic assertions of Process 1 appear to be valid to the observer described by a typical element of the final superposition. (Everett 1973a, p. 123)

This amounts to saying that in the *overwhelming majority* of cases, the relative frequencies one obtains will be, on average, coherent with statistical mechanics.

Everett was very focused on this concept: in the long thesis, in the short version and during the Xavier conference[1] he tried to defend his solution regarding the probability problem by assuming that typical states are simply more "frequent" than atypical ones.
But in order to explain why 'typical observers are typical (in a mathematical sense)' it is again necessary to have a general scheme for assigning a measure of typicality to relative states, without using *ad hoc* explanations. Everett demonstrates that the norm squared measure is the more *natural* measure for giving typicality a quantitative meaning.
In fact, in order to give strength to typicality measure, it is necessary that a criterion of consistency be satisfied: after having affirmed that not only "a typical branch of results will be precisely what is predicted by ordinary QM" but furthermore that "as the number of observations goes to infinity, almost all branches will contain frequencies of results in accord with ordinary QM", Everett explains that "to be able to make a statement like this requires that there be some sort of a measure on the superposition of states. What I need, therefore, is a measure that I can put on a sum of orthogonal states" (Everett 1962, p. 275).
The *criteria of consistency* of such a sum is sufficient to justify the usage of the square amplitude:

> There is one consistency criteria which would be required for such a thing. Since my states are constantly branching, I must insist that the measure on a state originally is equal to the sum of the measures on the separate branches after a branching process. Now this consistency criterion can be shown to lead directly to the squared amplitude of the coefficient, as the unique measure which satisfies this. (Everett 1962, p. 275).

The only measure turns out to be the square amplitude, since the measure assigned to a trajectory at one time shall equal the sum of measures of its separate branches at a later time: in order to be consistent, typicality should satisfy *additivity requirement*. Analogously to the classical case, in which conservation of probability has to be held, in PWM it is necessary to grant that the sum of measure is coherent. The unique measure which guarantees the additivity requirement is the square amplitude, as in classical statistical mechanics the only measure which guarantees conservation of probability is the Lebesgue measure.
The deduction of a measure assigned to the superposition of orthogonal states

[1] See Everett (1962).

requires that a positive function M of the complex coefficients of the elements should be declined in $M\left(a_i\right)$ for the element ϕ_i. From a superposition $\sum_{i=1}^{n} a_i\phi_i$ of orthogonal states, we take a subset $\alpha\phi' = \sum_{i=1}^{n} a_i\phi_i$ and we regard it as a single element $\alpha\phi^{'}$; the additivity requirement imposes that the measure assigned to $\phi^{'}$ shall be the sum of the measures assigned to the ϕ_i (from 1 to n), so that:

$$M\left(\alpha\right) = \sum_i M(a_i).$$

This is precisely the additivity requirement which we imposed and which leads uniquely to a choice of square measure amplitude.
This measure then assigns to the *i, j, . . . , kth* elements of the superposition:

$$\phi_i^{S_1}, \phi_j^{S_2} \dots \phi_k^{S_r}, \psi^{S_{r+1}} \dots \psi^{S_n}\psi^0_{i,j,\dots,k\left[\dots,\alpha_i^1,\alpha_j^2\dots\alpha_k^r\right]}$$

the measure

$$M_{i,\ j,\ \dots,k} = \left(a_i a_j \dots a_k\right)^* \left(a_i a_j \dots a_k\right).$$

This is a product measure ($M_l = a_l^* a_l$), so that the measure of a particular memory sequence is the product of the measures for individual components of the memory sequences.
The analogy between this measure and the probability theory of random sequence is now evident.
This means that probability theory is equivalent to Everett's measure theory *mathematically* (is has to be translated into measure-theoretic language). The measure over the branching trajectory should equal the sum of measures of the single branches at different times. This is precisely what is obtained by using the squared amplitude measure:

> If we were to regard the $M_{i,\ j,\ \dots,k}$ as probability for the sequence $[\dots, \alpha_i^1,\ \alpha_j^2, \dots,\ \alpha_k^r\ ]$ then the sequences are equivalent to the random sequences which are generated by ascribing to each term the independent probabilities $M_l = a_l^* a_l$. Now the probability theory is equivalent to measure theory mathematically, so that we can make use of it, while keeping in mind that all results should be translated back to measure theoretic language. (Everett 1973a, p. 126).

For longer and longer sequences of observations each memory sequence will produce the appearance of a randomly generated sequence, except for a set of a total measures which tends to zero as the number of observations becomes unlimited:

> Hence all averages of functions over any memory sequence, including the special case of frequencies, can be computed from probabilities $a_i^* a_i$, except for a set of memory sequences of measure zero.
> We have therefore shown that the statistical assertions of Process 1 will appear to be valid to almost all observers described by separate elements of the superposition, in the limit as the number of observations goes to infinity. (Everett 1973a, p. 127).[2]

The same measure is applied to arbitrary sequences of observations and to situations in which different observations are performed upon the same system.
Independent probabilities of Process 1 for each initial observation, and transition probabilities for succeeding observations upon the same system are then used for calculating the averages of any function over a memory sequence[3].
The scheme is the same in situations in which more than one observers are performing a measurement[4].
In order to respect the consistency criteria, typicality is then deduced from the theory as a simple measure for branch weighting.
Typicality is then *prior* to the square-amplitude, which is used to establish a criterion of consistency to branch weight measure.
With this *natural* and *unique* measure the value assigned to a particular memory sequence is simply the product of the measures for individual components of that memory sequence.
In this sense probability theory is equivalent to this measure theory from a mathematical point of view, and it is for this reason that we can make use of it for operationalist purposes (also for updating our expectations).
Summarizing, for almost all the elements of the superposition, it will appear that statistical assertions of Process 1 are correct, except for a set of elements with memory sequence zero; the frequency of results will be exactly the same of QM: almost all the branches will contain frequencies of results in accord with ordinary

[2] See Barrett (1999, "Limiting properties of the bare theory") for more details.

[3] Everett specifies how "the result is equally true for arbitrary sequences of observations", that is, observations of different quantities upon the same system will lead to the same measure because of application of Rule 1 and Rule 2 above mentioned (see Everett 1955d for further details).

[4] See Everett1973a, Section 3, p. 78 for the description of observations with several observers, when communication between them is allowed (with communication, i.e. transfer of impulses from the magnetic tape memory of one mechanical observer to that of another, observers become correlated).

quantum predictions.

Typicality is the recipe to assume that probability is not an objective feature of QM, but rather a subjective characteristic (in terms of decision-theoretic axioms, if you will) which depends only upon a relative state; it is also a natural justification for the quantitative valence of PWM.

4.3 The analogy with the classical case

The main argument for taking typicality as a justification of the Born rule is that it reflects classical statistical mechanics.

Everett indicates typicality as an analogous to the classical statistical mechanics, where the trajectory of systems in the phase space is measured on the phase space itself.

As in thermodynamics, even in PWM typicality is a measure of *trajectories* (in the classical case, over the phase space, in the quantum case as described by Everett, over the "life tree") which refers to the *overwhelming majority* of the system taken into consideration.

Typicality in classical statistical dynamics refers to Lebesgue measure for phase space, which is the *natural* measure of probabilities (which are to be intended as epistemic probabilities):

> The situation here is fully analogous to that of the classical statistical mechanics, where one puts a measure on trajectories on systems in the phase space by placing a measure on the phase space itself, and then making assertions which hold for "almost all" trajectories (such as ergodicity, quasi-ergodicity, etc.). This notion of "almost all" depends here also upon the choice of measure, which is in this case taken to be the Lebesgue measure on the phase space. (Everett 1973a, p. 72).

That is, the choice of measure is free, but the reason one chooses the one for which the overwhelming majority has nonzero amplitude in classical statistical mechanics, rather than assigning nonzero measure to "maverick" (atypical) trajectories is a strong one:

> One could, of course, contradict the statements of classical statistical mechanics by choosing a measure for which only the exceptional trajectories

> had nonzero measure. Nevertheless, only the Lebesgue measure on the phase space can be justified by the fact that it is the only choice for which the conservation of probability holds ("Liouville's theorem"), and hence the only choice which makes possible any reasonable statistical deductions at all. (Everett 1973a, p. 72-73).

"Conservation of probability", is ensured in the classical case; analogously, in PWM the additivity requirement quoted above is satisfied: the measure of one trajectory at one time will equal the sum of separate branches at a later time; in the same sense in classical phase space the density distribution over phase space is ensured and also Lebesgue measure is the only measure that satisfies Liouville's theorem. So, even if the choice for one measure is not a constraint, it is the only reasonable choice, because it is only with this measure that the additivity requirement is satisfied.
The analogy with the classical case justifies usage of typicality in PWM. But where does typicality in both contexts came from?

4.4 Typicality in classical statistical mechanics

Everett clearly refers to classical statistical mechanics to construct his analogy.
In this ambit, the concept of typicality derives from Boltzmannian statistical mechanics.
Statistical mechanics represents a bridge between macroscopic and microscopic levels of the description of a system, and the measure in statistical mechanics is a "crucial level-bridging ingredient" (Werndl 2013, p. 3).
Over the years it has been proposed to interpret this measure as a typicality measure: typical states show a certain property if the measure of the set that corresponds to this property is one or closer to one.
Here, a measure is defined over possible states of an isolated (bounded in a container) system, consisting of n particles, by which an approach to equilibrium is explained[5]. Obviously, the system can be regarded as an ensemble of microelements, as well as a unique macroscopic system.
Microscopically, the system is represented by a point $x = (p_1, \dots p_n, q_1 \dots q_n)$ where q_i stands for the position of the i-th particle and p_i stands for the momentum.

[5] See Werndl (2013, p. 4) for a description of a Boltzmannian system.

The microstates x are elements of the *6-n* dimensional state space Γ (which represents all possible position and momentum coordinates for all the particles). The system starts in a certain condition (microstate x) and its evolution is governed by Hamilton's equations; the solutions of these equations are the phase flow on the energy hypersurface ϕ_t (because the energy is conserved, the motion of the system is confined to a hypersurface Γ_E).
So, $\phi_t(x)$ gives the microstate of the system.
The *macroscopic* state M_i $(1 < i < n\ )$ expresses parameters such as local pressure and local temperature. For each macrostate there corresponds a macroregion Γ_M which consists of all $x \in \Gamma_E$ for which the macrostate takes those parameters.
Two macrostates are important: the "equilibrium state M_{eq} and the macrostate at the beginning of the process M_p. The macroscopic evolution of the system with initial microstate x is given by $M_x\ (t) = \mathrm{M}(\phi(x))$" (Werndl 2013, p. 5).
To characterize thermodynamic-like behavior[6], entropy should be introduced.
The Boltzmann entropy of a macrostate M_i at time t is definable as $S_Bt :=$ $S_B\left(M_{x(t)}\right)$, where $x(t)$ is the system's microstate at t and $M_{x(t)}$ is the macrostate corresponding to $x(t)$ (see Frigg 2009, p. 998).
The initial state M_p (also called the *past state*) is, by assumption, a low-entropy state; we expect Boltzmannian entropy of a system initially prepared in such a state to increase, reach its maximum quickly, and then remain more or less in this state for a long time (the dynamics will carry the system's initial state into $\Gamma_{M_{eq}}$ and keep it there for a long time).
The 'problem' emerges when one asks why the system behaves in a thermodynamic-like way.
The crucial point in statistical mechanics is the difficulty in examining the same systems at two very different *levels*. Macroscopic irreversibility derives from microscopic reversible dynamics: there is something like a "dramatic decimation" (Volchan 2007, p. 808) of degrees of freedom in passing from 6-N degrees of freedom to a macrostate involving few variables.
The reduction from a microscopic to a macroscopic level suggests that some kind of *averaging* should be involved which points to the role of statistics: " as there is no intrinsic randomness in the system at hand the use of statistics and probability might be better understood through typicality" (Volchan 2007, p. 808).

[6] A specification is needed: "because the same measures are employed in Boltzmannian statistical mechanics for questions regarding equilibrium and non equilibrium, the interpretation as typicality measure is relevant in both contexts. In general, much more is known about equilibrium SM than non-equilibrium SM. In particular, it is a central aim of non-equilibrium SM to explain thermodynamic-like behavior, but this is extremely difficult" (Werndl 2013, p. 6).

Standard Boltzmannian response is to introduce probabilities, arguing that a system of this kind is overwhelmingly likely to evolve in a thermodynamic-like way: the difficulties in analyzing initial conditions lead to the introduction of statistical assertions.

Typicality is then introduced to eschew probabilities: the system's behavior is typical, and that is the only reason why thermodynamic-like behavior is overwhelmingly observed.

As Volchan (2007, p. 803) observes "instead of indicating the presence of a random ingredient in the system, it [typicality] is taken as a yardstick in probing the relative size of some sets of microstates of interest (in particular, initial conditions) in the geometrical arena of phase-space".

The underlying concept is that probability, after Kolmogorov's axiomatization, has become a branch of pure mathematics: the old jargon of pre-axiomatized era (such as "sample", "occurrence", "trials", etc.) is not, strictly speaking, part of probability theory. In particular, "there is nothing intrinsically random "about random variables": they are just real valued measurable functions, those one expects to find in real analysis" (Volchan 2007, p. 804)[7].

Now, in statistical mechanics typicality makes its entrance to justify the usage of "pure" mathematics, i.e. the mathematical part of probability theory, without applying to it a stochastic significance (exactly what Everett wanted for his PWM).

Formally, typicality has been defined as a *relational* property P of e, which e possesses with respect to a set Σ to which it belongs (Frigg 2009, p. 1000): e is typical if most members of Σ have P and e is one of them. Derivatively, the set Π is typical if the overwhelming majority of its components have P.

Now, a measure of typicality could also be introduced to explain this sort of averaging, that is, to indicate the *relative size of sets of states.*

It is important to remember that "regarding the measures of interest in Boltzmannian SM, note that for Hamiltonian systems the uniform measure μ defined on Γ (the Lebesgue measure) is invariant under the dynamics; this result is known as Liouville's theorem (Petersen 1983, 5). μ can be restricted to a normalized measure μ_E on Γ_E, which is also invariant under the dynamics. Γ_E is called the microcanonical measure and is the standard measure used in Boltzmannian SM. Hence the question is how to justify Γ_E as typicality measure" (Werndl 2013, p.

[7] Volchan also adds: "coin-tossing is usually taken as the epitome of a random phenomenon. However, real coin-tossing is purely mechanical process that should in principle be modelled using rigid-body Newtonian dynamics plus the initial conditions; and its relation to randomness and unpredictability is a non-trivial matter".

5).
This means that typicality measure of an initial set of states should equal the typicality measure of the evolved set.

4.5 A general typicality measure: PWM explained

But typicality itself stands for a good justification. As Volchan says, "many tough questions are still to be addressed. For example, as typicality is relative to the measure used, how does one justify a particular choice? Under what criteria?" (Volchan 2007, p. 13).
A lot of work has been done recently on justifying application of the concept in classical statistical mechanics. Some of these accounts are unconvincing.
Zanghì (2005) for example explains that reaching the equilibrium distribution is inevitable since the overwhelming majority of microstates have this distribution. But, as Frigg (2009) notes, "typical states do not automatically attract trajectories".
Several attempts to justify it as a good measure for thermodynamic systems have been made; these examples, and its usage in other versions of QM (in pilot-wave theory) gave a new insight into using it as a counting measure also in Everettian QM.
The search for a more general criterion for evaluating typicality measure suggests that the same description of typicality in classical statistical mechanics could be applied more generally to measure-theoretic dynamical systems: measure-theoretic dynamical systems theory is indeed a mathematical tool by which the dynamic evolution of systems could be analysed starting from their *initial conditions.* In this case, typicality is "less problematic here than in statistical mechanics, in which a stronger conceptual justification is required. Now proponents of the typicality approach argue that typical initial states show thermodynamic-like behavior, and hence that an explanation of thermodynamic-like behavior can be given in terms of typicality" (Werndl 2013, p. 3).
Recently, Werndl proposed a justification of the usage of typicality as a good measure for both statistical mechanics and measure-theoretic dynamical systems, because once a justification is granted for the first set, then this is self-compelling for dynamical systems too: statistical mechanics is a special case of measure-theoretic dynamical systems, even if the former could not be conceptually assimilated by

the latter.
Werndl says:

> Statistical mechanical systems $(\Gamma_E; \Sigma_{\Gamma_E}; \mu_E; \phi_t)$ are (continuous-time) measure-theoretic dynamical systems (where Σ_{Γ_E} is the Lebesgue σ-algebra of Γ_E). This just means that the $(\Gamma_E; \Sigma_{\Gamma_E}; \mu_E; \phi_t)$ satisfy the formal definition of a measure-theoretic dynamical system. It does not imply that statistical mechanics is reducible to dynamical systems theory in any sense (the physical postulates of SM are not part of the mathematical framework of dynamical systems theory). (Werndl 2013, p. 7)

I feel that Werndl's proposal could well support Everettian systems too.
The kind of justification for typicality should satisfy requirements, i.e. *uniqueness* and *plausibility*:

- the premises imply that there is a *unique* typicality measure or a set of typicality measures which agree on which sets are typical/atypical (here any measure of this set can be used as typicality measure);
- the premises are at least potentially more plausible than just postulating that certain measures are a good choice of typicality measures.

What Werndl proposes is to look at the measure of measure-theoretic dynamical systems as a typicality measure.
Her argument goes as follows. The initial probability distributions of interest are translation-continuous (Typicality I, or "atypical means measure zero"), or translation-close (Typicality II, or "atypical means very small measure). A typical measure should be connected to these initial probability distributions in two ways:

1. if the set is of probability zero (typicality I) or very small probability (typicality II), for all probability distributions, then the set is atypical;
2. if, on the contrary, for all probability distributions, the set is typical if it is of probability one (typicality I), or very high probability (typicality II).

Furthermore, typicality measures should be invariant. All the typicality measures "agree on which sets are typical" (Werndl 2013, p. 28).

Now, my point is that in PWM this kind of justification is correct, that is, once its validity is demonstrated, its validity in PWM is also ensured.
I argue that in the contest of PWM, typicality has the same valence: it also could be seen as a “level-bridging” element but here between our experience and the branching process, because even PWM is concerned with two different levels of description.
Initially we focus on the explanation of our subjective experience, which is a fine-graining of the global wave function, and which is the equivalent of the microscopic description in classical statistical mechanics.
But once we examine the branching process, we can interpret it using a mathematical tool, which appears to be the “unique measure”.
As in statistical mechanics, even in Everettian QM, what makes typicality a unique measure is its invariance and its reliance on initial probability distribution.
Everett’s PWM, in the end, concerns *deterministically evolving dynamical systems*, made up of branches whose evolution has to be clarified.
Understanding the ‘behavior’ of “most” initial conditions, and knowing their initial probability distributions (by preparing the system), could give us the sense in which Werndl speaks about typicality measure: the coefficients express only the measure of typicality of subsets (rather than the single outcomes).
The last point of contact is *invariance*. The additivity requirement could be seen under the same light.
Typicality is an epistemic ‘trope’: it is an assemblage of concepts, methods and argumentative patterns that organize well-established mathematical practices into a specific epistemic story of equilibrium.

One final remark: “one could argue that it would be irrational to adopt a probabilistic model if one were not to assume also that the actual world is typical, but that also seems to be a (merely) pragmatic consideration. If all typicality means is the assumption that the initial conditions are such that they lead to the desired behavior, then it is hardly explanatory of that behavior [. . .] It seems to me that this line of thought, de-emphasising as it does the explanatory role of probabilities, on the whole favors a pragmatist reading of probabilities in Everett” (Bacciagaluppi 2013).

CONCLUSION

The aim of the present work was that of finding the original core of Everett's PWM.

Once established that the construction of a metaphysics of 'Many Worlds' was not in Everett's desires, the reconstruction of his own interpretation has been settled. What emerges is an empirically featured PWM, that is, a pragmatic attempt to formulate a universally valid QM without the introduction of further elements: the formalism is capable of yielding its own interpretation, once it is accepted that the world in its totality obeys the same quantum dynamical law.

Two different dimensions have been investigated, following Everett's own description: from a *qualitative* (speculative) point of view the first task was to see up to what extent Everett's empiricism is maintainable.

From an empirical point of view the theory does not violate the dictates of empirical adequacy, once that the boundaries of its legitimacy are defined within its isomorphic identity to perceived reality. The so-called extra-structure derives from the model, and is the expression of the global state, which is not accessible to our direct experience.

Even if this kind of explanation seems contradictory *prima facie*, because of the proliferation of apparently unobservable entities, it has been shown that it could be put into correspondence with a form of empirical reasoning which justifies a criterion of pragmatic selectivity.

Another level of investigation was needed, in order to accomplish the justification of PWM: a more technical, *quantitative* dimension has been introduced.

Under this light, classical Everettian problems have been revisited, in order to verify if PWM alone is able to bear the burden of explaining appearance of determinate records and probabilities.

Correlation and entanglement are the sufficient instruments that Everett uses for explaining the appearance of perceived experience.

Uniqueness and continuity over branches are then *deduced* from within the correlation model, so that quantitatively the results match the qualitative dimension.
The last problem, which is the vexed question in the tradition of Everett's interpretation, is the justification of probabilistic assertions: in other terms, why probabilities?
Everett aimed to formulate a QM without stochastic elements[8], so it was necessary to give an explanation of why statistical assertions of Process 1 appear to be valid to almost all *typical* observers.
For Everett the usage of Born rule was limited to operationalist purposes, again on a subjective, local level. Probabilities are just mathematical tools used for predictions, but they have no longer stochastic meaning: in this sense I called them 'Everettian *pseudo*-probabilities'.
But this is not a quantitative explanation yet: in order to justify probabilities a *deduction* or *reduction* to the model is needed.
Everett shows how the square amplitude is the more natural measure for branch counting, expressing the degree of typicality rather than real, objective probability.
The analogous with the classical statistical mechanics has been investigated, and it emerges that typicality in that context is becoming a bridging element between macroscopic and microscopic explanations.
What is convincing, in my opinion, is that in PWM as Everett originally intended it, a minimum criterion of empirical acceptability is needed: once we find our relative state appropriately represented in the model, then we can appropriately, and pragmatically, infer its typicality.
I think that a development on the justification of typicality deserves more attention, both in classic statistical mechanics and in PWM; also *dynamical invariance* and its connection to typicality and phase space (or branching global state) should be better analysed.
Concluding, the kind of explanation that has been proposed here can be considered *weak*, or an *ad hoc* attempt to read Everettian QM without metaphysics as a valid scientific theory simply by limiting its metaphysical weight, and confining it to a region of unobservability which is given by definition. It seems that a sort of dangerous circularity affects this kind of reading: it might appear that it forces the interpretation of PWM in order to 'limit the damages' of a literal reading.
Moreover, the problem of empirical evidence is not solved, in the end.

[8]The first title for his dissertation was "Quantum Mechanics without probabilities".

And the same could be said for typicality: it is a *simple*, *safe* and *pragmatic* concept. Again, it could certainly be considered a very weak concept, or an *ad hoc* tool for justifying the use of the Born rule without committing to probabilistic assertions.
But, one can always argue, the same could be said for standard QM and its paradoxes: the resolution of these miliar problems amounts to the philosophical perspective one decides to adopt, and certainly realists will probably never be satisfied with this genre of approach (and neither anti-realists, if non-Everettian).
The weakness of such an interpretation depends on the kind of explanation one wants to obtain: surely, if the goal is that to reach 'the truth', starting from a realist account of experience, then Everettian framework is untenable.
Nevertheless, if one starts from a *minimal* perspective (and it is now clear that this was Everett's own position), this sort of Humean approach is adequate, because it gives a complete explanation of perceived reality while avoiding the embarrassment of commitment to metaphysics, on the one hand, or the introduction of drastic and postulated dichotomies, on the other hand; if we look at PWM from an empiricist point of view, what counts is the pragmatic valence of typicality measure.
Besides I want to remark once again that the present exposition really aims to present what appears to be (at least, to me!) the real and original Everettian core.
A simple correlation model with an associated measure for typicality was all that Everett would have wanted from his theory.

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