How to Derive the Equilibrium Velocity **Distribution Two Ways** (Neither of Which is What You are Expecting) **Cosmology Summer School** Santa Cruz, July 26, 2013 Tim Maudlin, NYU

The Target

- Statistical Mechanics uses certain statistical techniques to derive "laws" for the behavior of systems with many degrees of freedom. The prototype such system is a box of monatomic gas with something like a mole of atoms. Of particular interest are the "equilibrium" states of such a system. Many "laws", e.g. the "ideal gas law" PV=nRT, describe relations between various (macroscopic) parameters of systems *at equilibrium*. What we are interested in here is a derivation of the velocity distribution of the atoms in our gas at a given temperature T *at equilibrium*.
- But what does "at equilibrium" mean?

Equilibrium

- Some macrostates (e.g. the state of a gas that had been confined to half of a box and then had the partition removed, or of two boxes at different temperatures put in thermal contact) spontaneously *change* with respect to their macroscopic parameters. And in an isolated system, those parameters often settle, after a little time, into a static state in which they persist for very long times. If there is a state into which the system so settles, it is called "equilibrium".
- Note that what counts as "equilibrium" may be sensitive to the definition of "a little time" and "very long times".

Note

• The very definition just given of an "equilibrium" state already has a time asymmetry built into it: "settling into" and "remaining in" denote a temporal progress from past to future. So a certain time-directedness is already built into the very description of the phenomenon we are trying to explain.

The Velocity Distribution

- Isolated boxes of gas tend to have equilibrium states into which they settle. In these states, the temperatures at different locations are uniform, pressures are uniform, densities are uniform (ignoring gravitational effects), the ideal gas law relates these, etc.
- "Tend to" here means: every known isolated box of gas through all history has behaved this way.
- At equilibrium, we expect the pressure, temperature and density to be uniform. But what about the distribution of velocities of the atoms?

A Note About "Uniform"

- The sense in which the density of the gas is "uniform" is this: subdivide the volume V of the box into many, many parts with volume P_i , but each part large enough to contain many, many atoms. At any moment, each such part has a well-defined density: N_i/P_i . The density if "uniform" if these densities are all equal to within some small epsilon. Similarly for temperature, pressure, etc.
- Note that this *requires* there to be many, many atoms. A "single particle gas" makes no sense at all in this context.

Boltzmann 1872

The mechanical theory of heat assumes that the molecules of a gas are not at rest, but rather are in the liveliest motion. Hence, even though the body does not change its state, its individual molecules are always changing their states of motion, and the various molecules take up many different positions with respect to each other. The fact that we nevertheless observe completely definite laws of behaviour of warm bodies is to be attributed to the circumstance that the most random events, when they occur in the same proportions, give the same average value. For the molecules of the body are indeed so numerous, and their motion is so rapid,

that we can perceive nothing more than average values. Once mul compare the regularity of these average values with the armanic constancy of the average numbers provided by statistics, which me also derived from processes each of which is determined by a completely unpredictable interaction with many other factorss. The molecules are likewise just so many individuals having the movaried states of motion, and it is only because the number of them that have, on the average, a particular state of motion is counstant that the properties of the gas remain unchanged. The determine tion of average values is the task of probability theory. IHem. the problems of the mechanical theory of heat are also problems probability theory. It would, however, be erroneous to Ibeliny that the mechanical theory of heat is therefore afflicted withh some uncertainty because the principles of probability theory aree used One must not confuse an incompletely known law, whose vvalidur is therefore in doubt, with a completely known law of the calculation of probabilities; the latter, like the result of any other calculus, i a necessary consequence of definite premises, and is conffirmed insofar as these are correct, by experiment, provided sufficciently many observations have been made, which is always the cases in the mechanical theory of heat because of the enormous number of molecules involved. It is only doubly imperative to handile the conclusions with the mastert . .

The Velocity Distribution

- Our target, then, is in this sense the empirical velocity distribution of a gas at temperature T at equilibrium. Take the set of all of velocities (or speeds) and divide it into ranges such that every range will have many, many atoms. We want to derive the (stable to within epsilon) distribution of velocities in the gas in these ranges at equilibrium.
- That is, we want the distribution that any initial state will tend to go to and remain in.

Note on Terminology

• The velocity distribution is, in the technical mathematical sense, a "probability distribution" or a "probability measure". That is, the proportion of atoms in the union of two disjoint velocity ranges is the sum of the proportions in each range, and the sum of all the proportions over all ranges is unity. But the meaning of the distribution has nothing at all to do with "probability". There is a definite, exact, empirical fact about what the distribution is at all times, and we want to derive the distribution that the system settles down to.

Temperature

• One might take the given temperature of the system as a clue to the velocity distribution. Since temperature is a measure of mean kinetic energy, $T = \langle 1/2mv^2 \rangle$, one might guess that most of the atoms should have a speed near $\sqrt{(2T/m)}$. But how near, and with what sort of spread?

Simplifying Assumptions

- To make our problem easier (using assumptions we can ultimately go back and derive), we will assume that at equilibrium the velocity distribution is *isotropic* (in physical space) and the spatial distribution of atoms is *uniform* (in physical space).
- This means that all we need is a *speed* distribution: the velocity distribution follows from the speed distribution and the isotropy.
- Note: the notions of isotropy and uniformity here *require a measure to be defined*, but the relevant measure is just given by the metric of space itself.

Any Guesses?

Three Derivations

- We are going to look at three derivations of the equilibrium velocity distribution:
- Maxwell 1860: "Illustrations of the dynamical theory of gases"
- Maxwell 1867: "On the dynamical theory of gases"
- Boltzmann 1872: "Further studies on the thermal equilibrium of gas molecules"

What These Derivations Are Not

- David has outlined or mentioned two possible approaches to a question like this. Let's recall them.
- Approach 1) choose (how?) some measure over the phase space of the system. For each possible velocity distribution, calculate the measure of the volume of phase space that has that distribution. If one such distribution absolutely dominates in terms of the measure, say that if the dynamics is "random" (ergodic? something else?), then no matter where the system begins, it will "probably" "soon" wander into that distribution and stay there "for a long time".

What They Are Not (Con't)

• Approach 2) Choose a measure over phase space (somehow). Choose a macrostate that specifies a velocity distribution. Use the exact microdynamics to time-evolve the measure restricted to the macrostate in time. If after some period of time, almost all of the time-evolved measure now occupies a macrostate characteristic of a certain velocity distribution, and then stays there for a long time, declare that the equilibrium state for the initial macrostate. Repeat for all possible initial macrostates.

Problems

- For Approach 1, we must somehow justify the use of the measure on phase space and explain what we mean by a "random" dynamics. It is not clear that one could derive anything like relaxation times from this approach. Note that the exact microdynamics plays no role. Given a tractable definition of "random dynamics", perhaps this can be done.
- For Approach 2, we must justify the choice of measure on phase space. The calculation would give relaxation times, based in the microdynamics, which have to be recalculated for each initial state. The relevant calculations are practically impossible.

In Any Case

- None of the three papers we are going to look at proceeds in anything like either of these fashions. Nonetheless, they both derive an equilibrium velocity distribution, and indeed the same equilibrium velocity distribution. Presumably, it is the same one that would be derived using Approach 2, but we can't directly verify this because we can't actually calculate using Approach 2.
- So how do they work?

What to Watch For

• In his 1872 paper, Boltzmann makes an incredibly strong claim about what he has achieved. So strong, in fact, that it is provably false. Both the reversibility and recurrence objections demonstrate it is false. When this was pointed out, Boltzmann quickly saw how to explain what he had done and add the appropriate caveats. But the fact that he made the claim shows that it was not entirely clear even to him what he was doing.

It has thus been rigorously proved that, whatever may be the initial distribution of kinetic energy, in the course of a very long mue it must always necessarily approach the one found by Maxwell. The procedure used so far is of course nothing more thin a mathematical artifice employed in order to give a rigorous moof of a theorem whose exact proof has not previously been mund. It gains meaning by its applicability to the theory of polyatomic gas molecules. There one can again prove that a ortain quantity E can only decrease as a consequence of molecular motion, or in a limiting case can remain constant. One can also move that for the atomic motion of a system of arbitrarily many material points there always exists a certain quantity which, in consequence of any atomic motion, cannot increase, and this mantity agrees up to a constant factor with the value found for the well-known integral $\int dQ/T$ in my paper on the "Analytical proof of the 2nd law, etc." We have therefore prepared the way for an analytical proof of the second law in a completely different way from those previously investigated. Up to now the

Keep Your Eyes Peeled!

- So here are some questions:
- At what point, and in what way, does some timeasymmetric assumption appear in the reasoning?
- At what point, and in what way, does any "probabilistic" or "randomness" assumption appear in the reasoning? How is it justified?

Maxwell 1860

- Maxwell's 1860 derivation of the equilibrium velocity distribution is amazingly short, and amazingly puzzling.
- All that Maxwell assumes is that the equilibrium velocity distribution has two formal properties:
- 1) It is isotropic.
- 2) The velocities of a particle in orthogonal directions are uncorrelated.

Clarification of Terms

- It is clear what "isotropic" means here: for any given speed, the number of particles moving in any particular (coarsegrained) direction is the same, within epsilon. (In a oneparticle gas, this cannot possibly obtain!)
- The condition of the velocities in orthogonal directions being uncorrelated also has an exact meaning. Take, for example, the overall distribution of X-velocities. Now restrict attention to all the particles with any given Y-velocity, and look a the distribution of X-velocities in that subgroup. It should be the same (within epsilon) for all Y-velocities. So knowing the Y-velocity of a particle gives no information about its X-velocity (or Z-velocity), for any orthogonal directions.

The Theorem

- Now given these two assumptions, there is a rather amazing mathematical fact:
- There is only one form of velocity distribution that has both of these formal features!
- The velocity distribution in every direction (hence X-velocity, Y-velocity, and Z-velocity for any choice of X-, Y-, and Z-axes) must be Gaussian: $f(v) = Ae^{-Bv^2}$.
- The speed distribution is therefore $g(s) = A's^2e^{-B's^2}$.
- The temperature fixes the B, normalization the A. Done.

WTF?

- On the one hand, the status of Maxwell's 1860 theorem *as a theorem* is clear: it is, indeed, a purely mathematical result, proven with certainty. Any velocity distribution that satisfies Maxwell's two formal criteria must be of this form.
- But that just makes more urgent the completely unaddressed question: Why assume that the equilibrium distribution, defined as the distribution systems tend to, satisfies these formal criteria? So far, we have been given no reason at all to expect this!

What Maxwell Said (per Strevens)

- Why isotropic?
- "the directions of the coordinates are perfectly arbitrary" (Maxwell 1860, p. 381).
- Why uncorrelated?
- "the existence of the velocity x does not in any way affect that of the velocities y or z, since these are all at right angles to each other and independent" (p. 380)

Dialogue on Humeanism, Laws, and Mechanical Explanations

 Hypothetical Mad-Dog Humean: WFT do you mean "WTF?"? Maxwell has provided us something short and pithy that you can write on a T-shirt, that is also very informative! Who cares how, or whether, he "justifies" the principles of the derivation? The only justification for such a claim, the only justification there could possibly be, is *empirical* justification! The question is: is Maxwell's claim true or not. If it is true, and pithy, and informative enough, then ipso facto it is a law of nature, and has all the explanatory power that any law of nature has. Period. End of Story.

• Worried More Moderate (or Sane) Humean: Somehow that just seems wrong to me. The claim that all boxes of gas at equilibrium just have this velocity distribution doesn't somehow have the right form to be a law of nature: it may be a true generalization, but if so it itself stands in need of explanation. Let's see if I can justify, from Humean principles, my squeamishness. Maybe Maxwell's claim is pithy, and true, but not informative enough. After all, not that many things in the world are boxes of gas, and further not all boxes of gas are even at equilibrium.

• HMD Humean: Let's take these in turn. It's true that not that many things in the world are boxes of gas, but many are and, in principle many more could be. If there happened to be more, is there some tipping point at which it becomes a law? If so, how do you know we not at that point? Or even better, maybe we can expand Maxwell's distribution to cover *stars*, which are sort of confined boxes of gas! There are lots and lots stars! Maybe a simple extension covers most of what there is!

- WMM Humean: Look, even if we can somehow properly regard many more things as effectively boxes of gas, there is another problem: not that many are *at equilibrium*. So the covered class is small in that respect too.
- HMD Humean: You are overlooking the meaning of *equilibrium*. Equilibrium, as we said, is the state that all (quasi-isolated) systems *aim at* and *tend to*. Given that meaning, Maxwell's claim has empirical consequences not just for systems at equilibrium, but also for systems out of equilibrium, and hence for all (quasi-isolated) systems. And at least the whole universe is a perfectly isolated system! So the scope is very large.

• WMM Humean: Wait a minute: this is getting worse and worse! Pointing out the meaning of "equilibrium" in this way makes the content of Maxwell's claim wider but also teleological. No proper law of nature should have the form of saying what a system aims at to tends to! We want mechanical explanations, not teleological explanations, and in any mechanical explanation the later states of the system are accounted for in terms of the earlier states, not vice-versa! It may be a consequence of the laws of nature that gases tend to a certain end state, but to accept that itself as a *law* would be to reject much of the Age of Reason!

• HMD Humean: Where the hell does such a constraint on the nature of a proper explanation and hence a proper law come from! We want pithy and we want informative! You are in the grip of some old-fashioned, long refuted idea that there is some fundamental direction of time (from past to future) which induces a relation of cause and effect in which causes precede their effects, and proper, mechanical explanations are causal! Away with all that rubbish! It would, obviously, rule out teleological explanations, but since there is no direction of time the very definition of "teleological" makes no sense.

Dialogue End

- Sane Non-Humean: Hey, guys, I think there *is* a fundamental direction of time, and that causes always precede their effects, and that when we look for "mechanical" explanations we are asking for accounts in terms of how the microscopic constituents of things interact as time goes forward to produce certain sorts of outcomes, where the future states are all *derived* and cannot play the role of *targets*. So if you have any sort of intuition that we are looking for "mechanical" explanations, you are tacitly presupposing my view.
- HMD Humean and WMM Humean (in unison): You maniac!

Maxwell 1867

• "The only case in which I have determined the form of this function is that of one or more kinds of molecules which have by their continual encounters brought about a distribution of velocity such that the number of molecules whose velocity lies within given limits remains constant. In the Philosophical Magazine for January 1860, I have given an investigation of this case, founded on the assumption that the probability of a molecule having a velocity resolved parallel to x lying between given limits is not in any way affected by the knowledge that the molecule has a given velocity resolved parallel to y. As this assumption may appear precarious, I shall now determine the form of the function in a different manner."

Maxwell 1867: Strategy

• Maxwell's 1867 derivation of the velocity distribution formula proceeds along entirely different lines. It begins with the microdynamics, or rather with an assumption about the result of the microdynamics in a suitably dilute (and in another way suitably non-dilute) gas: most of the time, any given atom will travel at constant speed in a straight line, so its velocity is constant. The velocity only changes if there is a collision between two atoms. A collision occurs when they get sufficiently close.

Strategy

- In order to determine how the velocity distribution of the gas *changes* over a period of time, then, all one needs to know is
- 1) How the velocities of two colliding particles change when they undergo a certain sort of collision and
- O 2) How many collisions of the various possible sorts take place over that period of time.
- The first is rigorously derived from the microdynamics.

Collision Dynamics

- The first step is to figure out what happens in a certain kind of collision. What do we mean by "a certain kind of collision"? That depends on the dynamics: we have to specify enough information about the collision situation to use the dynamics to determine the outcome. Since we are assuming a regime in which most of the time the particles are on inertial trajectories, the "incoming" state is given by specifying the initial velocities of the two particles before the collision starts, plus whatever more information is needed to determine the outgoing velocities.
- What is this extra information for Newtonian dynamics?

Masses and Impact Parameter

- To solve the problem in Newtonian mechanics, we need to know the masses of the two particles and the relative orientation in space of their initial positions (and the force law, including charges needed to determine the forces, of course).
- The relative orientation, in turn, can be specified by one: the impact parameter, which is the spatial vector from M_1 to M_2 at their instant of closest approach assuming no collision (i.e. assuming inertial motion).

Two Views

• There are two convenient ways to picture the collision: in the frame in which the total momentum of the pair of particles is zero, and in the frame in which M₁ is at rest. These are obviously intertranslatable. What does the collision look like in each frame?

In the Center of Mass Frame



In the frame in which M_1 is initially at rest



Epsilonics

• We want to calculate how many collisions of a certain kind happen in a given period of time dT. "Of a certain kind" means: a particle with mass M₁ in the small range of velocities from V_1^{in} to V_1^{in} + dV collides with a particle of M_2 in the range of velocities from V_2^{in} to V_2^{in} + dV with an impact parameter between **B** and $\mathbf{B} + d\mathbf{B}$, in a time between T and T + dT. All of the d's are finite and chosen large enough so there will be many such collisions in the given time, and small enough that the range of outgoing velocities is small. This requires that there be many particles of each type, not too dilute.

Input from Step I

The dynamics tells us what the outgoing velocities of the two 0 particles in such a collision will be (within epsilon). So if we knew how many such collisions took place between T and T + dT, we would know how many M_1 particle that originally had velocity V₁ⁱⁿ no longer have V₁ⁱⁿ on account of such a collision (so how the total number of M_1 particles with V_1 ⁱⁿ has been reduced in that time due to these sorts of collisions) and how many M2 particles that originally had V₂ⁱⁿ no longer have V₂ⁱⁿ due to such collisions. And since we know what the outgoing velocities are, we would know how many more M_1 particles have V_1^{out} at the end of the time due to such collisions, and how many more M₂ particles have V_2^{out} due to such collisions.

Integrate Over All Possibilities

• Now if we have these transition numbers, we just integrate over all possible collisions to get the total change in the velocity distribution for each type of particle. We would therefore have derived an equation for how the velocity distribution changes with time. That equation happens to be time asymmetric, and to have the feature that no matter what the initial velocity distribution happens to be, it will relentlessly approach, and eventually get arbitrarily near to a particular velocity distribution: the one that Maxwell derived in 1860! We have proven that the Maxwell distribution is the equilibrium distribution in our sense!

The Passage in Maxwell 1867

On the Mutual Action of Two Systems of Moving Molecules.

Let the number of molecules of the first kind in unit of volume be N_1 , the mass of each being M_1 . The velocities of these molecules will in general be different both in magnitude and direction. Let us select those molecules the components of whose velocities lie between

$$\xi_1$$
 and $\xi_1 + d\xi_1$, η_1 and $\eta_1 + d\eta_1$, ζ_1 and $\zeta_1 + d\zeta_1$,

and let the number of these molecules be dN_1 . The velocities of these molecules will be very nearly equal and parallel.

On account of the mutual actions of the molecules, the number of molecules which at a given instant have velocities within given limits will be definite, so that

$$d\mathbf{N}_1 = f_1(\xi_1\eta_1\zeta)d\xi_1d\eta_1d\zeta_1\dots\dots\dots(2)$$

We shall consider the form of this function afterwards.

Let the number of molecules of the second kind in unit of volume be N₂, and let dN_2 of these have velocities between ξ_2 and $\xi_2 + d\xi_2$, η_2 and $d\eta_2 + \eta_2$, ζ_2 and $\zeta_2 + d\zeta_2$, where

 $d\mathbf{N}_2 = f_2(\xi_2\eta_2\zeta_2)d\xi_2d\eta_2d\zeta_2.$

Continued

The velocity of any of the dN_1 molecules of the first system relative to the dN_2 molecules of the second system is V, and each molecule M_1 will in the time δt describe a relative path $V\delta t$ among the molecules of the second system. Conceive a space bounded by the following surfaces. Let two cylindrical surfaces have the common axis $V\delta t$ and radii b and b+db. Let two planes be drawn through the extremities of the line $V\delta t$ perpendicular to it. Finally, let two planes be drawn through $V\delta t$ making angles φ and $\varphi + d\varphi$ with a plane through V parallel to the axis of x. Then the volume included between the four planes and the two cylindric surfaces will be $Vbdbd\varphi \delta t$.

If this volume includes one of the molecules M_2 , then during the time δt there will be an encounter between M_1 and M_2 , in which b is between b and b+db, and φ between φ and $\varphi+d\varphi$.

Continued

Since there are dN_1 molecules similar to M_1 and dN_2 similar to M_2 in unit of volume, the whole number of encounters of the given kind between the two systems will be

$Vbdbd\phi \delta tdN_1 dN_2.$

Now let Q be any property of the molecule M_1 , such as its velocity in a given direction, the square or cube of that velocity or any other property of the molecule which is altered in a known manner by an encounter of the given kind, so that Q becomes Q' after the encounter, then during the time δt a certain number of the molecules of the first kind have Q changed to Q', while the remainder retain the original value of Q, so that

 $\partial \mathbf{Q} d\mathbf{N}_1 = (\mathbf{Q}' - \mathbf{Q}) \nabla b db d\phi \delta t d\mathbf{N}_1 d\mathbf{N}_2,$

or

$$\frac{\delta Q d N_1}{\delta t} = (Q' - Q) V b d b d \varphi d N_1 d N_2. \qquad (3)$$

Here $\frac{\delta Q dN_1}{\delta t}$ refers to the alteration in the sum of the values of Q for the dN_1 molecules, due to their encounters of the given kind with the dN_2 molecules of the second sort. In order to determine the value of $\frac{\delta QN_1}{\delta t}$, the rate of alteration of Q among all the molecules of the first kind, we must perform the following integrations:—

End

1st, with respect to φ from $\varphi = 0$ to $\varphi = 2\pi$.

2nd, with respect to b from b=0 to $b=\infty$. These operations will give the results of the encounters of every kind between the dN_1 and dN_2 molecules.

3rd, with respect to dN_2 , or $f_2(\xi_2\eta_2\zeta_2)d\xi_2d\eta_2d\zeta_2$.

4th, with respect to dN_1 , or $f_1(\xi_1\eta_1\zeta_1)d\xi_1d\eta_1d\zeta_1$.

These operations require in general a knowledge of the forms of f_1 and f_2 .

The Key Move

- The fundamental principle used in the derivation is an Assumption about the Number of Collisions or, in German a *Stosszahlansatz*. The assumption is just this: If the target regions occupy a proportion P of the total volume, then (about) the same proportion P of "attacking" particles will, at that time, happen to find themselves in the target region, and hence will undergo that type of collision in the stated period of time.
- That is, the Stosszahlansatz is just the claim that the location of the target areas is *uncorrelated* to the location of the attacking particles. The sense of being *uncorrelated* is perfectly clear.

A Theorem

• Given the Stosszahlansatz, Maxwell's result is (almost) a proper theorem (issues about the epsilonics). So the relentless, time-asymmetric march toward the Maxwell distribution will in fact occur as long as the Stosszahlansatz holds. What the reversibility objection shows, then, is that in the time-reversed scenario the Stosszahlansatz does not, in fact hold. What the recurrence objection shows is that eventually, if we wait long enough, the Stosszahlansatz will not hold.

Boltzmann 1872

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Before the collision x, x+dx = x', x'+dx' (A) After the collision $\xi, \xi+d\xi$

The kinetic energy of one molecule is in the column labelled a, and that of the other in the column labelled b. We now ask, how many collisions take place in time τ in unit volume such that the kinete energies of the colliding molecules lie between the limits (A)? We shall denote this number by dn. The determination of dn can only be accomplished properly by considering the relative velocity of the two molecules. Since this calculation, although tedious, is not

Boltzmann 1872

the two molecules. Since this calculation, although tedious, is not at all difficult, and has no special interest, and the result is see simple that one might almost say it is obvious, I will simply state the result. It is the following: dn is, first, proportional to the time τ ; the longer this time τ is, the more collisions of the specified kind will occur, as long as τ is very small, so that the state of the gas during τ does not change noticeably. Second, dn is proportional to the quantity f(x, t)dx; this is indeed the number of molecules in unit volume whose kinetic energy lies between x and x + dx; the more such molecules there are in unit volume, the more often will they collide. Third, dn is proportional to f(x', t)dx'; for whatever holds for one of the colliding molecules will of course hold for the other one. The product of these three quantities must be multiplied by a certain proportionality factor, which is easily seen to be an infinitesimal like $d\xi$. This factor will in general depend on the nature of the collision, and hence on x, x' and ξ . To express these properties we shall write the proportionality factor as $d\xi \cdot \psi(x, x', \xi)$ so that we have therefore

 $dn = f(x, t)dx f(x', t)dx' d\xi \psi(x, x', \xi)$

The Explanatory Status of Lack of Correlation

• If we think that Maxwell and Boltzmann have actually managed to *explain* both what the equilibrium distribution *is* and *why* it is the equilibrium distribution (i.e. the distribution towards which all actual isolated boxes of gas in the history of the universe approaches), then the *Stosszahlansatz* must have a particular sort of explanatory force, so that deriving a phenomenon from it counts as *explaining* the phenomenon. What could such a status be?

A Priorism

• One might argue that a priori, before "opening our eyes", we should expect positions not to be correlated. And one might then ask by what right we could form such an expectation? (Cue David.)

Pure Empiricism

• We could say that the only rational expectations we can have must come from experience. If in our experience, such particles often *are* uncorrelated then that can become a Humean law, and hence we can appeal to it to explain. Or in any case, if what counts as "explaining" a phenomenon is nothing more than noticing that "it usually happens", and that can only be determined empirically.

Default Explanatory Status

• There is another possible position, somewhat *a priori* but distinct from the *a priori* view. It says: you should have no expectations at all about what you will see when you open your eyes. But *if* you happen to find positions like this uncorrelated, then, absent other considerations, that *requires no further explanation*, and if you happen to find correlations, that *does* require further explanation.

Example

Suppose you are flipping a coin in Santa Cruz and the 0 Pope is flipping a coin at the Vatican. You both flip many times. Maybe both coins are fair, and the flips are about half heads half tails, maybe one or both are biased, etc. But suppose we ask: are the outcomes of these different flips correlated or uncorrelated. That is a perfectly well-defined question, which amounts to: would conditionalizing on the results of the Pope's flip change the frequency of results of my flips at all? If not, they are uncorrelated, is so they are correlated.

Claim

- I claim that our attitudes in the two cases are quite different. If the outcomes are uncorrelated then absent further considerations (e.g. an attempt to rig things so they were correlated) that requires no further explanation. Once we explain the frequency of each coin individually, the lack of correlation between them requires no further explanation. But if the flips were correlated, then that would demand a further explanation. (See Bell's theorem).
- This is a true psychological fact. I claim it is also proper methodology.