Are We Measuring the Right Things for Climate?

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Abstract If one could exist on climate scales would it make any more sense to measure laboratory-scale quantities to capture climate conditions than it does for us on the laboratory scale to compute wave functions to understand the weather? Clearly the quantum mechanical and the laboratory regime are constructed in terms of different physical variables. Why do we presume, then, that laboratory regime quantities like temperature continue to be the appropriate physical variables to measure in a climate regime? This paper suggests why we may not be measuring the right things and it will broach some alternatives in the context of a reformulation for relevant physics more natural to long timescales: slow time. Specifically it shows that fluctuating velocities can be "thermalized" in suitable averages suggesting that one might imagine climate in terms of a generalization of wind which may include persistent meteorological winds, or none at all. But it also shows that temperature cannot be "thermalized" on long time and space scales, making the notion of local equilibrium and simple generalizations of temperature problematic for climate.

1 Introduction

We measure thermodynamic quantities like temperature, pressure, and humidity for weather—all strictly local and transient properties of a physical system out of global thermodynamic equilibrium. Should we measure the same things for climate? It is taken for granted that these things continue to have meaning for climate. Moreover, the physical climate system is often viewed, to the contrary, as a stable thermodynamic system, only changeable through external influences, even though there is no physical reason to view it in that way. But perhaps there is something

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Fig. 1 Two images of the same Niagara Falls downstream flow. The *left image* is an exposure of 0.4 s, while the *right-hand image* is exposed for 50 s. Note the flow features visible in the *right-hand image* (streamlines, bow waves, standing waves, vortices, etc.) that are not clearly visible or invisible in the *left image*

thermodynamic-like on large enough space and timescales. If climate can actually prove to have such a property, it must emerge from an unstable dynamical system where any direct thermodynamical connections are strictly local. Showing such a thing exists, if it even does, is a most challenging scientific problem.

In terms of thermodynamical quantities, there are few good analogs on the gravity-irrelevant, jiggling, and sticky kinetic-atomic scales, despite some interesting efforts to find thermodynamic-like analogues for those microscopic scales. Such conventional quantities remain tied to the laboratory regime. But studying climate is not unlike atomic physics upside down, where we are the atoms. While it is easy to mistake the appearance of, say, snow or palm trees for climate, these are only indirect manifestations of a grander physics. Trying to imagine that physics from a laboratory-scale perspective is like viruses trying to theorize about what the laboratory they are in looks like. This paper suggests that we may not be measuring the right things for climate, and it will broach some alternatives in the context of a reformulation for relevant physics more natural to long timescales: slow time.

To fix ideas, consider the images of Fig. 1. The left-hand image of Fig. 1 shows the turbulent water of the Niagara River downstream from Niagara Falls as the human eye sees it. The water flow is complex and turbulent as it self-interacts, and interacts with the shore and river bottom, not to mention surface interactions with the air. In contrast the right-hand image of the same scene shows phenomena previously only visible to the most educated eye, if visible at all. Streamlines, bow and standing waves, or downstream vortices are all plain in the right-hand image, which is a 50 s time exposure.

On the 50-s timescale physical phenomena reveal themselves that are invisible to the unaided eye. The reverse is also true. Things are visible to the eye that do not show up on the 50-s timescale. There is an old trick of architectural photographers that eliminates all traffic from an image by the use of long lenses, slow film, and time exposures. Some of what is visible to the human eye is thus made to disappear in the resulting images, not unlike the case of some turbulent water in the Niagara River images.

We have a sense of the appearance and disappearance of different physical phenomena between different physical regimes through the relationship between physics on the atomic and laboratory scales. But in that we also understand the physics of the laboratory scales stands independent of that of the atomic scales too, even though they are physically consistent and compatible (Essex 2011). We can in that sense "ignore" the atomic regime in studying laboratory-scale physics. That is we can make predictions of laboratory-scale phenomena in terms of laboratory-scale variables only, without explicitly referring directly to specific kinetic-scale variables.

Can we do this with the 50-s timescale fluid flow from Fig. 1? This is far from clear. Just because we see structure does not mean that there is a stand-alone physics, let alone dynamics for that regime. To see if there is dynamics one could generate a sequence of 50-s time exposures and then run the result as a video. Perhaps the streamlines and standing waves, etc., change and move in the resulting slow-time video, perhaps they do not. But if there is a dynamics of the 50-s regime that stands independent of the laboratory regime, one needs to be able to forecast what happens on the 50-s timescale video without requiring data from the laboratory regime. The resulting theory and its associated variables must be able to ignore the laboratory regime.

The closure problem of fluid mechanics is the famous failure to achieve independence for the physics of turbulent flows from the laboratory regime. Of course the theory, as realized in the Navier–Stokes differential equation, can be integrated to generate integrated variables, which (it was hoped) would stand as the measurables of a putative theory for turbulent flow, independent of the usual laboratory regime. But it failed.

Thus to this day not only can we not always accurately predict the flow in a pipe from first principles but we cannot accurately predict the lowest order statistic either from first principles. It failed because the integration of the equation creates more independent integrals over combinations of variables than original variables in the parent regime. Thus not all values are determined by the integrated equation within the integrated regime. It is always necessary to refer to the parent regime to evaluate them, and thus the integrated equation cannot forecast anything, except in (at best) an empirical manner. The integrated variables are not part of a stand-alone theory, but are subordinate to the laboratory regime. They do not represent the measurables of a stand-alone theory for turbulent flow.

The 50-s regime defined through Fig. 1 does not imply that there is anything special in comparison to, say, a 200-s regime, or a 4-h regime for that matter. All the issues of structure appearing and disappearing can still be in play between them, but none need to represent a regime with a stand-alone physical theory independent of the laboratory regime. The climate problem is simply a version of this problem, but on a much grander scale. But while there is no established on-going discussion of the 50-s regime, there is one for climate. While there are no putative variables for a putative 50-s theory being regularly measured, putative variables for climate have boldly been advanced without proof of their merit.

We do not yet know whether climate is simply a phenomenon subordinate to the meteorological regime (which only differs slightly from the laboratory regime as the parent regime for climate conceptualization), or a physically distinct regime with its own governing equations in terms of variables assembled in an as yet unknown manner from meteorological or kinetic primaries. If the answer to the existence question is yes, it is an open question as to whether what we measure or assemble from meteorological measurements today in the name of studying climate actually represents true climate measurables emerging from a stand-alone theory for climate.

While we cannot answer this question definitively we can use thinking from the beginnings of slow-time theory to look at aspects of this issue, assuming such a stand-alone climate regime exists. We can say that certain variables are not likely to help us with insight into a stand-alone theory for climate. In particular with previous work on the "slow-time Maxwellian" (Essex and Andresen 2015) we will show that local equilibrium will not likely survive in a climate regime, which makes any suppositions about an analog to meteorological local equilibrium problematic, suggesting that climatological measurables will not be simple averages over local thermodynamic states.

First we will address this by discussing how one might envision the thermalization of wind. We will find that the kinetic energy of wind is easily thermalized under particular conditions, making wind something that fits naturally into a climate picture where systematic winds survive averaging and random fluctuations can be envisioned as contributing to a long timescale version of temperature. Second we show that unlike wind, fluctuations in temperature cannot be thermalized, because they typically produce a distribution that does not have a Maxwellian shape.

This suggests that local thermodynamic states normal for meteorology cannot exist for a putative climate regime, and raises the question as to whether averages over local temperature will provide insight into climate.

2 No Wind

Let's start with a simple example and consider the effect of fluctuations in rest velocity, u, of a small volume of gas, i.e., wind. Without loss of generality we proceed in terms of fluctuations in one space dimension. Then the molecular velocity profile is the Maxwellian,

$$p(v; u, T) = \left(\frac{m}{2kT}\right)^{1/2} \frac{1}{\sqrt{\pi}}\, e^{-\frac{m}{2kT}(v-u)^2}. \tag{1}$$

Imagine that on a large timescale, e.g., the timescale of climate, winds experience reversals and ranges of magnitudes so that we may plausibly assume a normally distributed rest velocity u about $u = 0$ with σ_u being its standard deviation. If over the long timescale there is a prevalent velocity u_0, it is easy to translate this distribution to be around that u_0. Assuming that the central limit theorem holds, this convolution of the v and u distributions is itself a Gaussian,

$$p(v; \theta) = \left(\frac{m}{2k\theta}\right)^{1/2} \frac{1}{\sqrt{\pi}}\, e^{-\frac{m}{2k\theta}v^2} \tag{2}$$

but now with a revised effective temperature, θ,

$$\theta = \frac{\sigma_u^2 m}{k} + T \tag{3}$$

that contains the fluctuations of wind u. Suppose $\sigma_u \sim 5\,\mathrm{m/s}$, then for air at $T = 300\,\mathrm{K}$, $\sigma_u^2 m/k \sim 0.1\,\mathrm{K}$. This change of temperature of 0.1 K is for most practical purposes negligible. However, for other flows than the material wind, e.g., radiation, the ensuing revised effective temperature may be markedly changed. In any event, what is wind on the laboratory (meteorological) scale is still wind on the long timescale. But it has changed what is perceived as temperature.

The new temperature here, θ is an emergent feature of a well-defined underlying (small-scale) mechanism, not just a generalization. It is in all respects a legitimate temperature. As long as u is fluctuating in a Gaussian manner, all of the ideal gas relationships re-emerge, but in the temperature θ instead of T. For example, energy E along one axis is simply, $E = Nk\theta/2$, just as it is in T for the laboratory regime. Coarsening the timescale for fluctuations in u amounts to thermalizing the wind.

3 No Local Temperature

Next we turn to fluctuations in, temperature, over our long timescale as a more relevant quantity for climate predictions. Like before for u, we will assume that fluctuations in T, or some function of T, are normally distributed. This is speculation, but the aim is only to find a plausible slow-time scenario. Meanwhile we will not be working with T but θ defined in Eq. (3), where wind, u, has been thermalized. Actually, for mathematical convenience we will be working in the precision of a distribution rather than its standard deviation. The precision is 1/(standard deviation). For a Maxwellian velocity distribution like Eq. (1) we have that the standard deviation $\sigma_u \propto \sqrt{T}$ while the precision $\psi \propto \sqrt{\beta}$ where $\beta = 1/kT$. However, we will still refer to fluctuations in the precision as "temperature fluctuations." Thus larger precision means a tighter distribution.

Now the Gaussian precision, ψ, is defined by

$$\left(\frac{m}{2k\theta}\right)^{1/2} \frac{1}{\sqrt{\pi}}\, e^{-\frac{m}{2k\theta}v^2} = \frac{\psi}{\sqrt{\pi}}\, e^{-\psi^2 v^2}, \tag{4}$$

where $\psi = 1/(\sqrt{2}\sigma_\theta) = \sqrt{m/(2k\theta)} = \sqrt{m\beta_\theta/2}$ and has units of 1/velocity for the Maxwellian.

Let us now suppose that this precision itself is not constant but is normally distributed in a variable ξ about some reference value ψ_0 such that $\psi = \psi_0 + \xi$. Then Eq. (4) becomes

$$\frac{\psi}{\sqrt{\pi}}\, e^{-\psi^2 v^2} = \frac{\psi_0 + \xi}{\sqrt{\pi}}\, e^{-(\psi_0+\xi)^2 v^2} \equiv p_{v\xi}. \tag{5}$$

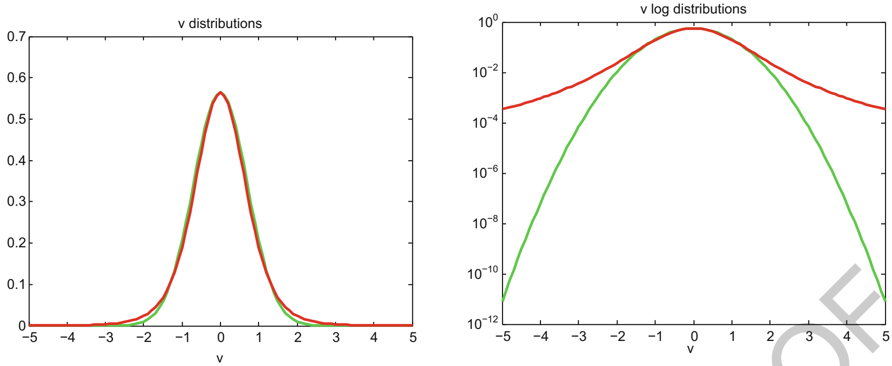

Fig. 2 The velocity distribution $p(v; w, \psi_0)$ of Eq. (7) for $w = 2.5$ and center precision $\psi_0 = 1$ (*red*). A pure Gaussian thermal distribution is shown in *green* for comparison. The *left frame* is a normal linear plot, the *right frame* a semilog plot where the agreement between the slow-time distribution (*red*) and a thermal distribution (*green*) for small velocities but large discrepancy at large velocities is even more evident

Since $\psi = \sqrt{m/2k\theta} > 0$ for finite θ, $\xi \in (-\psi_0, \infty)$ so that the normal distribution ought to be truncated. However, in typical statistical applications infinite domains are commonly used instead of semi-infinite ones. For example, the convention of spectroscopy is to integrate over spectral lines for frequencies, $\nu \in (-\infty, \infty)$, even though negative frequency makes little physical sense. In this case the inadmissible values contribute little to relevant integrals as well (Essex and Andresen 2015).

Taking this position we allow $\xi \in (-\infty, \infty)$ instead. The corresponding probability distribution function in ξ is

$$p_\xi = \frac{w}{\sqrt{\pi}}\, e^{-w^2\xi^2}, \tag{6}$$

where w is the Gaussian precision for this ξ distribution with units of velocity. We will see that the resulting structure is such that w appears naturally in the expressions as a velocity, aiding interpretation of molecular velocity v regimes:

$$p(v; w, \psi_0) = \int_{-\infty}^{\infty} p_{v\xi} p_\xi d\xi = \frac{w^3\psi_0}{\sqrt{\pi}(v^2 + w^2)^{3/2}} \exp\left(-\frac{w^2\psi_0^2 v^2}{v^2 + w^2}\right). \tag{7}$$

This equation is the temperature counterpart of Eq. (2) for the wind average.

Two distinctive features emerge: This probability distribution function has polynomial (heavy) tails and a Gaussian core. The shift between these is controlled by the remarkable argument of the exponential, $-w^2\psi_0^2 v^2/(v^2 + w^2)$. Notice that Eq. (7) is almost symmetrical in v and w. For small velocities, when $v \ll w$, it becomes the classical Gaussian form $\exp(-\psi_0^2 v^2)$ since the denominator in the pre-factor, $(v^2 + w^2)^{3/2}$, behaves like a constant. For large velocities, $v \gg w$ the argument of the exponential approaches a constant leaving an asymptotic behavior of $\sim v^{-3}$. Figure 2 illustrates this mixed behavior.

Thus near the center of the probability distribution function it behaves like a Maxwellian with temperature θ while far from the core the simple notion of temperature is not sustainable. This Maxwellian is invalid for $|v| > |w|$, thus θ has no usable role in the sense of thermodynamics in that moments of the integral will not produce the traditional simple functions in terms of θ.

This is quite different from the result of letting the velocity u fluctuate, where the result was another Gaussian probability distribution function, but with a revised temperature, θ. The u fluctuations were naturally incorporated into the microscopic ones. This does not happen with the fluctuations in ξ since the microscopic quantity temperature or precision also appears in the normalization factor multiplying the exponential in Eq. (1). Thus knowledge of short time quantities is needed for calculation of the longtime average of temperature. In other words, temperature cannot be part of a self-contained set of variables at long times.

4 Other Winds

The preceding makes two key points clear:

1. For finite w temperature cannot be thermalized like wind. Thus local equilibrium and all that it implies is tied to the laboratory regime, and not a property of large space and timescales.
2. Properties like wind can be formally thermalized as above, and mechanical pressure (distinct from thermodynamic pressure) continue to have meaning. Persistent winds on long timescales can be captured in the preceding by not assuming wind fluctuations are centered on zero.

Local equilibrium is tied entirely to the practical existence of intensive thermodynamic variables (Essex and Andresen 2013). Local conditions must then be characterized in a different manner in a putative climate regime. Unlike intensities, extensive thermodynamic variables can exist in such a regime. Thus we can still speak, for example, of energy and numbers of molecules. We can still imagine boundaries that such properties traverse, therefore fluxes still make sense. Vector flux densities divided by the corresponding volume densities of any extensive thermodynamic quantity of that slow regime will thus induce a local vector velocity field. This provides a way to distinguish between fixed conditions and evolution. When all vector velocity fields become identical, all processes stop. There is a rest frame in which there are no flows. The need for local equilibrium is thus circumvented. The various vector velocity fields are referred to as *generalized winds* (Essex 2013). Furthermore, all flows are put onto a common scale: velocity. A departure in velocities from each other is a measure of the vigor of processes.

5 Conclusion

This paper has contemplated the perspective of an observer who would regard the laboratory regime as jiggly and microscopic, much as we see the kinetic or nanoscales. We aimed to get beyond pure speculation by focusing on how the Maxwellian distribution might be seen by such a slow-time observer. The window of observation for this observer would be bounded by events that are too close in time to distinguish from his point of view (fast time), which would include our regime. We would regard the putative observer as experiencing slow time. Hence the resulting distribution is described as the slow-time Maxwellian.

The technique was to form compound distributions by fluctuating the wind, u, and temperature, T. Temperature and velocity emerge with a conjugate quality, which occurs explicitly in the case of thermalizing of wind. But it also appears in a more subtle manner in the precision picture of the Gaussian distribution because fluctuating precision led to a normal distribution with its own precision (i.e., the precision of the precision). The latter has units of velocity, and this velocity, w, plays a decisive role in the structure and behavior of the resulting compounded densities. It acts like a reference velocity separating regimes. It divides Gaussian-like structure from polynomial, heavy-tail structure.

An unusual hybrid of Gaussians with heavy tails emerges in this paper as a key feature. Heavy tails clearly can be expected to be a feature of the slow-time regime. This has some consequences. First, the notion of local equilibrium ceases to be strictly valid. There is no straightforward temperature, as there is in the Maxwellian case. There could be other qualities that might play such a role in the slow-time regime, but they would not be temperature strictly speaking. If w is large enough, the core would still behave Maxwellian, which would permit a limited return to temperature as long as the core of the probability distribution function is of importance. Second, the wings of the distribution need to be considered from a physical standpoint to avoid divergent moment integrals.

The slow-time observer is left with a rather different behavior for the ideal gas. There are heavy tails and a nearly Gaussian core, becoming more Gaussian with increasing w. But as the tails are heavy, we observe divergent second moments. Does this mean that energy becomes infinite? Not if there are only a finite number of particles and finite energy in the underlying system to begin with. The composition of probability distribution functions changes nothing in this regard.

The fundamental finding of this study is that while wind persists in slow time (the climate perspective), temperature does not. Hence any conclusions based on an extrapolation of short laboratory time measurements of temperature are ill founded: We are *not* measuring the right things.

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