Does Laboratory-Scale Physics Obstruct the Development of a Theory for Climate?

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Abstract. Despite best efforts there still is no physical theory for climate based on the physics of the laboratory regime (i.e. fluid mechanics, radiative transfer, classical thermodynamics etc.). This paper builds from previous discussions of how laboratory-regime assumptions may lock our current theoretical efforts into the laboratory regime, and how we might get around this problem. Using ultra-long time photographic exposures (known as solargraphs) for inspiration, it draws into question classical thinking about intensive thermodynamic variables for theoretical climate purposes, while using the fact that physical flows remain well-defined even for climate regimes. These flows are characterized here as "generalized wind." A simple example based on radiative energy and entropy transfer illustrates how these generalized wind fields can partially replace what is lost in moving away from laboratory regime physics. These winds are shown to carry the dynamics in a modified form of radiation-like fluid dynamics that together with radiation might be possible to close in climate regimes.

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1. Introduction

Climate is difficult to visualize because we do not observe it directly. Palm trees or snow, say, only represent indirect effects of climate. We also observe the indirect properties of the atomic or kinetic world (material color, texture etc). We need sophisticated microscopes or other devices to get direct visualizations of individual atoms. Are there devices that can give us a direct visualization of climate? A pinhole camera probably can.

Solargraphs are photographic images made with ultra-long exposure times. The exposure times are so long that they begin to approach the low end of climate timescales. The images are of landscapes taken using pinhole cameras having time exposures of the order of 6 months. While they are artistic photography they are known for depicting the semiannual astronomical movements of the Sun directly (Dodwell, 2008). The Sun appears in them not as a disc but as glowing arches in the sky, showing the Sun's daily traverse (one for each day). The arches are suspended above an eerie landscape that is simultaneously familiar and odd.

There are no clouds and there is no rain. Busy streets are empty. Parking lots on the other hand are full of ghostly images of cars. The movement of cars or other traffic is too quick to show up on the image, while the parking lots are not full of particular cars but of recurrent visits of cars. To show up on the image, things have to be very bright (i.e. the Sun), or persistent. "Persistent" means fixed (such as a building), periodic, or recurrent. The car images in each space of parking lots do not have to be composed of a single car in exactly the same position every day. The persistence emerges from repeated visits of not necessarily identical cars, similarly placed because of the parking rules. The visits are recurrent rather than periodic following the language of dynamical-systems theory.

Over long timescales phenomena show up that would not be seen on short timescales. Similarly things apparent on short timescales are not visible. This is not unlike our experience with the kinetic regime. While atoms are too small to see, atomic movements are also too fast to see, but their collective structures are not. The chemical journey of particular atoms into and out of a large molecule or solid state lattice is not seen, but the overall structure appears to us as analogous to the ghostly parking lot.

Solargraphs are like snapshots of climate. They give a sense of what it would be like to directly perceive the world in a climate regime, where the meteorological world would be like the kinetic regime is to us, at least in terms of time. To make things dynamic, at 24 frames a second, a 10 second video, made from successive 6-month solargraphs, would span 120 years, while a millennium would provide just over 80 seconds of video. Of course these numbers are *ad hoc*, presented just to fix a sense of scale. One could imagine different frame rates and longer exposures to integrate over, say, the annual cycle entirely or even longer. In any case the solar arches would sparkle in the video as different cloud patterns in each frame would turn on and off segments of each arch in successive frames. Over centuries the video would show changes in persistent features too, sometimes suddenly, sometimes not. Some movements might be like the classical brownian jiggling of pollen suspended in water observed through a microscope. To eliminate this, spatial resolution would have to be reduced, analogous to looking at the pollen without a microscope.

The central question that would arise from a hypothetical solargraph movie would be whether there would be any long-time analogue to weather; that is, intrinsic ultra-long-term natural variability. The contemporary modeling picture would lean toward no such intrinsic natural variability. It is presumed in modeling that over sufficiently long times models achieve "climate states" or "climate equilibrium" (e.g. Boer and Yu 2003, Essex et al 2007).

The absence of intrinsic ultra-long-term natural variability in models implies that initial values are not very important for them. That is, averaged behaviour becomes invariant over a substantial range of initial conditions. This is

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the basis of the view held by some modelers that climate is not a mathematical initial value problem like weather, but a mathematical boundary value problem instead. The merit of this mathematical characterization has been argued for and against by various authors (e.g. Pielke 1998, Branstator and Teng 2010). There is general forecasting importance to this question because significant "long-time weather" would imply that external causes are not the sole source of observed "climate" changes. More importantly a model "climate state" sets the *de facto* working definition for "climate" itself.

The phenomenon of model climate "states" is seen through white-noise-like behaviour over timescales of decades and longer. For example Figure 8.13a, on page 624 of Randall *et al.* (2007) depicts not only the extent of attempts to capture long-time internal dynamics in models (in terms of ENSO for this example), but beyond timescales of about 80 months, in that figure, these unforced model spectra become nearly white (i.e. nearly horizontal and flat. Departures from strict whiteness are discussed in Lovejoy *et al.* (2012)). This is the realization of the putative model "climate state."

Simply put, by the Wiener-Khinchin theorem a white signal does not correlate with itself so we expect that the long-term signal does not correlate with itself either, obviating long term trends in the absence of external forcing. Alternatively the fluctuation exponent being less than 1 implies that fluctuations converge (Lovejoy *et al.* 2012). Thus the long-term signal does not have organized trends without external forces. Outside of the short timescale wiggles and computation-induced drifts, the long-time physical signal would be flat too. There is thus no natural resonance or "ring" to the model system on those scales. The boundary value problem picture implies that models are physically more like a "hot brick" than a "ringing bell."

There is little observational or theoretical reason for this (Essex 2011) as we shall see below.There is also insufficient data suitable for validating models on such time scales to decide the question empirically, even if we could intelligently distinguish forced from unforced changes. The need to check climate model behaviors against *observational* data and not just climate model control runs is crucial, because all climate models are empirically based. That they are empirical is not a whim, but an intrinsic necessity. It is easy to show that the complexity of the problem is such that even short computational climate runs of a decade, say, would each take longer than the age of the Universe if the appropriate classical physics was fully respected.

Empiricism in this case is thus not just a convenience; it is a necessity. This has a higher cost than many realize, because the small scale structure (i.e. sub-grid-scale) of models does not fully respect physical laws. The only validity for such an approach is the extent that it supports large scale agreement with observations. While this is an effective and well-precedented strategy in the world of engineering, the climate problem is not an engineering problem. This is in part because of the data issue, which can acquire a tautological aspect when models become the main source of data used to check models. Empirical parameterizations in this case mean that the small scale structure is fundamentally "tuneable." There is no unique nonphysical mathematical structure to base parameterization on, and even within a given class of mathematical structures from which the parameterizations are built, specific values for *ad hoc* constants must be selected to optimize the best possible large scale agreement with observations for a given parameterization class. This can work out well in an engineering problem, where experiments can be conducted to set the best values for these constants in all the regimes a design is likely to encounter.

But here the climate problem diverges from the engineering problem. The nature of the climate problem is such that we don't know that the best constants from today or yesterday will be the best values in the future, especially if we aim to capture some large-scale qualitative change. Would the parameterization class itself have to be altered, generalized, or changed completely? It is true that most of the laboratory based parameterizations such as the infrared band models of radiative transfer, say, would remain unchanged, but when it comes to clouds or moist convection, specific hydrology or any number of other complex, climate-based phenomena the same cannot be said, except as a matter of faith. The only way to set these parameterizations, engineering style, is to use the observations of what the system from the future will do, but it is precisely these observations that we don't yet have and are trying to forecast. Thus our best climate models must be more didactic than prognostic. A simple illustration is given in Essex (1991).

Since parameterizations do differ across models, it is still perhaps surprising in this light just how much agreement there is among them. Huybers (2010) discusses one instance of how such agreement can emerge spuriously. But the focus of this paper is the common long-timescale behaviour that climate models share. This tends to bring the focus to timescales of decades to centuries. While some argue that such timescales just represent "low-frequency weather" (Lovejoy and Schertzer 2012) and not actually climate per se, there is no rigorous definition for climate, model "climate states" not withstanding. This timescale could as well be referred to as "high-frequency climate." Moreover we may one day discover that there are many distinct regimes that we currently bundle under the single heading of climate. In any case the telling issue remains as to whether it is possible to deduce a closed system of equations that would be predictive for such high-frequency climate. That was what was inspired by the notion of a solargraph movie in the first place.

On this timescale, there are stochastic-thermodynamic plausibility arguments based on the thermodynamic ocean reservoir timescales that there should not be such low frequency behavior. This supports the boundary value picture for climate. However the atmosphere and oceans are as dynamical as thermodynamical in nature. Complex dynamical systems are notorious for having timescales that can be arbitrarily long, thermodynamical reservoirs not withstanding (Essex et al. 2007). Moreover long numerical integrations of complex nonlinear systems are notoriously unstable. Instability becomes the greatest evil in such calculations, but once it is tamed is the result correct or merely stable? Computational over stabilization produces computationally stable results that are wrong, killing off real dynamics (Corless et al. 1991). There is evidence that computational over stabilization is in fact present in climate models (Valdes 2011). Ensemble averaging provides no way out of this problem (Essex et al. 2007). All or part of the "states" observed in models may simply be computational artifacts, while there is also evidence of ultra-long-term multi-periodic or chaotic dynamics (Tsonis et al. 2007, Wyatt et al. 2012).

Leaving purely didactic climate models aside, there are three distinct conventional approaches to addressing the theoretical climate problem from first principles: time averaging of dynamical equations, scalar field averaging, and ensemble averaging in modeling (Essex 2011). The first of these runs up against the classical closure problem, because in its most rudimentary form it ultimately is just time averaging of the Navier Stokes equations in the manner of Reynolds. If this didn't work out well for turbulence, why would it work better for a more difficult problem? The second approach, scalar field averaging, has more than a closure problem. It aims to extract local physical meaning from trends in integrals over the field values of local intensive thermodynamic variables. This has little coherent physical basis (Essex 2011). Averaging takes on its physical meaning in terms of physically based equations that do not need to refer explicitly to unaveraged quantities (i.e. closed) (Essex 2011). Finally ensemble averaging, which aims to combat the vagaries of sensitivity to initial conditions, relies on the ensemble representing an invariant probability density induced by the dynamics. But one might not exist in practice, and in any case any long-term invariant properties are dependent on the computational scheme (Essex et al. 2007), bringing us back to the prospect of computational over stabilization.

The change in time and space scale from the laboratory regime to the climate regime is no smaller than the change in time and space scale from the kinetic regime to the laboratory regime. That means that a meteorological approach to climate using computer models is not unlike attempting direct kinetic theory calculations on computers to understand laboratory scales. Neither can be done exactly, and only the latter can be directly tested, let alone treated theoretically. Similarly, simply coarsening the 19th and early 20th century theories of radiative transfer, fluid mechanics, and thermodynamics has proven problematic. Even Saltzman, who was one of the historical proponents of time averaging, eventually capitulated in envisaging a super GCM (i.e. General Circulation Model) (Saltzman 2002, Essex 2011) as the only way forward. If current empirically based GCMs are indeed over stabilized, we are left only with *ad hoc* definitions for climate, and the question of whether an actual climate regime defined by physical equations that can ignore the laboratory regime even exists in nature.

So is there any way forward? This paper suggests some possibilities. Current first-principles climate theory works upward in scales from the laboratory regime. But the physical theories of the laboratory regime can themselves be derived from averaging over the kinetic regime. Are there some aspects of that averaging that lock the resulting equations into the laboratory regime making them fundamentally unsuitable for a climate regime? If there are, then any effort to find a climate theory from averaging those equations might prove futile.

This paper begins with this question, and, as such, approaches climate theory in a non-classical manner. That is averaging starts with the kinetic regime instead of the laboratory one. Some obstacles from laboratory scale physics are identified in section 2. But identifying them leads to thoughts about how to get around them. That is the topic of sections 3 and 4. Solargraphs help to anchor theoretical reasoning. Questions are raised about the applicability of the usual meteorological quantities to a climate regime, while a physically based generalization of the notion of wind emerges. These notions lead to strangely linear structures for fluids more reminiscent of radiation, which fit easily with similar equations for radiation.

2. Locked Into the Laboratory Regime

To observe very fast events in the laboratory regime we need to employ high speed cameras, producing movies we know as slow motion. They allow us to visualize what is too fast to see. The empty busy streets of the solargraphic images, would need a climate version of slow motion to observe laboratory regime traffic for a climate observer. This suggests a maximum non-persistent observable speed for physical processes in a climate regime, u_{max}. Faster (meteorological) processes are simply part of the underlying noise, which must become observationally invisible in order for an observer to see new structure. In photography this would be like arguing that over exposure destroys an image, so the underlying noise can be thought of as analogous to the processes that are too small and too fast at the kinetic scales.

We know that at very small scales (nanoscales for example) the physics in play is different in practice from that of our normal experience. On nanoscales things are fundamentally "sticky" and thermodynamical principles such as the second law of thermodynamics do not hold except on the average because everything is jiggling (e.g. Smalley 2001). We should also expect that the key physics will be different from that of our normal everyday experience when we scale up to a climate regime. This section focusses on some obvious differences, and how not taking them into account can trap thinking into the laboratory regime.

At the kinetic level atoms or molecules move and collide with each other. But we are not capable of following the dynamics of every atom and molecule. Various important approaches to molecular dynamics do attempt this with computers, always limited by approximation or numbers. But the scales of the problem of concern in the climate problem are far too big for this, so there is a different approach. As we coarsen space and time, we are gradually able to adopt a continuum description of the physics. The continuum description is valid for kinetic systems even where classical fluid mechanics does not hold. The continuous function of interest is the mean occupation number,

$$n(\mathbf{r}, \mathbf{p}, t)\, d^3r\, d^3p = \tilde{n}(\mathbf{r}, \mathbf{v}, t)\, d^3r\, d^3v \qquad (1)$$

which is just the number of atoms or molecules (ignoring internal structure) in an element of phase space or alternatively configuration-velocity space on the right side (i.e. $\mathbf{p} = \mathbf{v}/m$), where $\mathbf{v}$ is velocity and $\mathbf{r}$ is the position vector.

For atoms or structureless molecules of mass m, the mass density ρ and the vector mass flux density, $\mathbf{G}(\mathbf{r}, t)$ are given by

$$\rho = \int n(\mathbf{r}, \mathbf{p}, t)\, m\, d^3p\,; \quad \mathbf{G} = \int n(\mathbf{r}, \mathbf{p}, t)\, m\, \mathbf{v}\, d^3p\,, \qquad (2)$$

respectively. The mechanical rest frame velocity of the mass flow is $\mathbf{u}_m = \mathbf{G}/\rho$.

$\mathbf{u}_m$ is familiar to meteorologists as the solution of the Navier-Stokes equation,

$$\rho \frac{\partial\, \mathbf{u}_m}{\partial t} + \rho\, \mathbf{u}_m \cdot \nabla\, \mathbf{u}_m = -\nabla \cdot \mathbf{P} + \rho\, \mathbf{g}. \qquad (3)$$

$\mathbf{P}$ is the pressure tensor, and $\mathbf{g}$ is the acceleration vector of gravity. $\mathbf{u}_m(\mathbf{r}, t)$ is a vector field that we interpret as wind. (3) can be derived directly from moment integrals over the time derivative of the mean occupation number (e.g. Duderstadt and Martin 1979), which will be discussed more extensively in Section 4.

In terms of wind, it is envisaged that a point observer riding at $\mathbf{u}_m(\mathbf{r}, t)$ would experience calm conditions, with local well-defined thermodynamical variables such as temperature and pressure. Of course observed pressure is the manifestation of large numbers of atomic or molecular collisions with a macroscopic measuring device. In that sense wind is itself a kind of persistently structured anisotropic pressure. Pressure interpreted in this kinetic manner is thus reference frame dependent, while the classical meteorological pressure is tied to the mechanical rest frame. If the frame is moving with respect to an observer in the climate regime at a speed above u_{max}, then the observer would not "see" the frame that the pressure is tied to, making the quantity rather abstract to an observer in the climate regime.

Likewise local temperature, T, is similarly tied to the mechanical rest frame of the laboratory regime, emerging through an approximate local Maxwellian distribution of velocities in which T appears as a parameter of the probability density function (PDF),

$$\tilde{n} = \tilde{n}_L \approx \tilde{n}_M = N(\mathbf{r},t)\left(\frac{m}{2\pi k_B T}\right)^{3/2} \exp\left(-\frac{m|\mathbf{v}-\mathbf{u}_m|^2}{2k_B T}\right). \quad (4)$$

The subscript L denotes the laboratory regime, and the subscript M denotes the Maxwell Boltzmann distribution. The trivially different $\tilde{n}$ form of the mean occupation number was used, from (1), because a representation in terms of $\mathbf{v}$ is the more common form. k_B is the Boltzmann constant, while $N(\mathbf{r}, t)$ is the number per unit volume of configuration space.

n_L is not coarsened enough for a climate regime. In establishing the continuum approximation for a climate regime, we must choose a bigger volume in configuration space and larger intervals of time to coarsen over. So $n(\mathbf{r}, \mathbf{p}, t)$ would not normally be given by (4), but it could be approximated by a folding of approximate Maxwellians over space and time — one n_M for each occurrence of $\mathbf{u}_m$ in the space-time volume coarsened over. This would broaden the distribution $\tilde{n}$ considerably in velocity space leading to a new $\tilde{n} = \tilde{n}_C$ — similarly for momentum space. Over long enough times (climate) this would involve flow reversals. Flow reversals are physically meaningful in terms of mass flow — laboratory scale wind (i.e. meteorological wind) changes direction. It follows that the sub field of the laboratory regime to be coarsened over defines mass flows in local space-time $\{\mathbf{G}^L(\mathbf{r}, t)\}$ that would map, by coarsening, to a single net mass flow $\mathbf{G}^C(\mathbf{r}, t)$ in the climate regime. Thus we anticipate a smaller averaged mass flow than in the laboratory regime, $|\langle \mathbf{G}^C(\mathbf{r}, t)\rangle| < |\langle \mathbf{G}^L(\mathbf{r}, t)\rangle|$. It follows that the mechanical rest frame velocity, $\mathbf{u}_m^C$, in the climate regime will differ from the corresponding velocity in the laboratory regime according to $|\langle \mathbf{u}_m^C(\mathbf{r}, t)\rangle| < |\langle \mathbf{u}_m^L(\mathbf{r}, t)\rangle|$. Moreover for very long times $|\langle \mathbf{u}_m^C(\mathbf{r}, t)\rangle| \ll |\langle \mathbf{u}_m^L(\mathbf{r}, t)\rangle|$, which agrees with the u_{max} picture inspired by solargraphs.

The resulting n_C would be connected to a folding of the probability density function(s) (PDF's) induced by (3) with that of n_L. That PDF could be non-parametric and quite exotic. There is a wide array of families of PDF's induced by different processes, from Lévy distributions to PDF's based on generalizations of generalized hypergeometric functions. These arise in actual physical applications, such as anomalous diffusion processes (e.g. Hoffmann et al. 1998, Prehl et al. 2010). Chaotic systems even have fractal attractors, and thus PDF's can even be fractal functions. If by some unlikely chance these PDF's result from the statistical independence necessary for the central limit theorem to hold, we would come back to a Maxwellian/Gaussian form. In the unlikely event that this form is not a transient, it would still be a composition of many n_L's, so the parameter that we might look to for temperature would not yield the meteorological temperature. Instead it would be some other value suited to the particular climate regime for as long as that form of PDF persists.

Thus the usual measured thermodynamic variables of meteorology may well be unsuitable in climate regimes. An observer in a hypothetical climate regime would measure different things with different instruments than we are used to, much like a pinhole camera differs from a normal camera. Perhaps other forms of highly "desensitized" instrumentation like pinhole cameras needs to be considered. Seen in this light, physically interpreting trends in integrals over meteorological scalar fields seems particularly problematic.

So what sort of variables could we use? What drives thermodynamic processes, if one does not have the usual gradients in intensive thermodynamic variables in the mechanical rest frame? Even out of local thermodynamic equilibrium extensive thermodynamic variables such as internal energy, and entropy are well defined. They may be physically compared by generalizing the notion of wind.

3. Generalized Winds

Even without classical laboratory regime thermodynamic quantities, dynamics still has meaning. Mass and extensive thermodynamic properties such as internal energy and entropy are still well-defined and moved in the ocean and atmosphere in a climate regime. Classical wind is given by $\mathbf{u}_m$ from (3). In regimes of longer timescales and larger space scales there would still be mechanical wind, but it would be non-classical and it would decline with larger space-time scales. Wyatt et al. (2012) propose observable 80 year timescales over a hemisphere, which suggests characteristic speeds of the order of $10^{-1}\,\mathrm{cm\,s^{-1}}$ possible for the low end of climate regimes. On still longer timescales we expect that $|\langle \mathbf{u}_m\rangle|$ will decline to arbitrarily small values with respect to the Earth frame. The Earth frame would become a universal frame out of practicality, as the mechanical rest frame no longer has any special thermodynamical significance. But in the coarsened limiting case, all classical wind becomes inaccessible to an observer. The system could then be viewed as having no mechanical rest frame at all. In that sense the limiting case is not unlike radiation in radiative transfer, where the Earth frame is selected because there is no mechanical rest frame at all for relativistic reasons.

Even though there are no mechanical winds for radiation, there are still meaningful generalizations of wind for radiation, even under steady conditions. For example, the internal energy vector flux divided by the internal energy density, $\mathbf{u}_e$, or the entropy vector flux divided by the entropy density, $\mathbf{u}_s$, etc. are each velocity fields in their own right. As rest frames, they are just as valid as $\mathbf{G}/\rho$ is. To illustrate this, return to the point observer riding with the mechanical rest frame of a classical fluid at $\mathbf{u}(\mathbf{r}, t)$. There will be no mechanical wind, but there will generally speaking be other generalized winds such as energy wind $\mathbf{u}_e(\mathbf{r}, t)$ or entropy wind $\mathbf{u}_s(\mathbf{r}, t)$. That is the vector fields for each of these different types of generalized wind will be different from each other, and the mechanical rest frame observer would observe time varying currents of these quantities running through the mechanical rest frame.

In this generalized wind picture, thermodynamic equilibrium, the absence of entropy production, only occurs when all types of generalized wind align to yield a unique rest frame for all flows. Agreement between velocity fields indicating thermodynamic equilibrium is a concept due to the thermodynamicist Stanislaw Sieniutycz (1996), arising from his work on relativistic covariant formulations of fluid mechanics. It is particularly suited to this picture because we do not trust the laboratory frame intensive thermodynamical variables for the reasons described above, and the mechanical $\mathbf{u}_m$ has reduced magnitude and importance. But since differing thermodynamical intensive variables are the traditional marker for non-equilibrium, something else must fulfill this role.

To give an example of these generalized winds in practice, consider a case from radiative transfer. Fundamentally photons are much simpler than atoms or molecules. While space vehicles have been envisaged that have sails designed to be pushed by radiation pressure, we cannot think in terms of mechanical wind rest frames for radiation. But we certainly can speak of energy and entropy velocity fields, which constitute generalized winds even if that may seem strange for a radiative transfer problem, not to mention strange from the standpoint of classical fluid mechanics. Radiative entropy transfer was probably introduced to the atmosphere/meteorology literature first in Essex (1984b). Perhaps the definitive modern comprehensive physical treatment can be found in Essex and Kennedy (1999). Direct treatment of entropy radiation in terms of classical radiative transfer can be found in Holden and Essex (1997) and Essex (1984a). These references support the following arguments.

For radiation, the energy and entropy velocity fields are given by

$$\mathbf{u}_e(\mathbf{r},t) = \frac{\mathbf{F}}{u_r}\,; \quad \mathbf{u}_s(\mathbf{r},t) = \frac{\mathbf{H}}{s_r} \tag{5}$$

respectively, where

$$\mathbf{F} = \int I_\nu\,\hat{\mathbf{n}}\,d\Omega\,d\nu; \quad u_r = \frac{1}{c}\int I_\nu\,d\Omega\,d\nu; \tag{6}$$
$$\mathbf{H} = \int J_\nu\,\hat{\mathbf{n}}\,d\Omega d\nu; \quad s_r = \frac{1}{c}\int J_\nu\,d\Omega\,d\nu.$$

$\hat{\mathbf{n}}$ is the unit direction vector of a beam, along which energy and entropy flows. I_ν is the specific intensity (or radiance) for energy, while J_ν is the specific intensity (or radiance) for entropy. These are moment integrals for photons just like equations (2) are for molecules. This is more obvious with the relationship

$$I_\nu = \frac{2h\nu^3}{c^2}n^r, \tag{7}$$

where n^r is the mean occupation number for photons. The relationship for entropy is more complex, but well defined.

Assuming steady conditions, local thermodynamic equilibrium, frequency independent absorption (gray) and plane parallel geometry we may integrate over all ν (denoted by dropped ν subscripts),

If we assume all upward flow is independent of $\hat{\mathbf{n}}$ then $I = I^+$, and similarly for downward flow $I = I^-$, we have the conditions for the classical two-stream gray atmosphere. Similar definitions for entropy allow the definitions (6) to be simplified to,

$$\mathbf{F} = F\,\hat{\mathbf{k}} = \pi(I^+ - I^-)\,\hat{\mathbf{k}}; \quad u = \frac{2\pi}{c}(I^+ + I^-); \tag{8}$$
$$\mathbf{H} = H\,\hat{\mathbf{k}} = \pi(J^+ - J^-)\,\hat{\mathbf{k}}; \quad s = \frac{2\pi}{c}(J^+ + J^-).$$

$\hat{\mathbf{k}}$ is the unit vector in direction of increasing altitude, z.

Thus

$$\mathbf{u}_e = \frac{c}{2}\frac{I^+ - I^-}{I^+ + I^-}\;\;; \quad \mathbf{u}_s = \frac{c}{2}\frac{J^+ - J^-}{J^+ + J^-} \tag{9}$$

In radiative equilibrium $\nabla\cdot\mathbf{F} = dF/dz = 0$ and $\nabla\cdot\mathbf{H} = dH/dz > 0$. The latter is positive because it equals the entropy production rate. Thus F is constant and H is an increasing function of z. Using $I^+ = F/\pi + I^-$ and $J^+ = H/\pi + J^-$ from (8) we find

$$|\mathbf{u}_e| = \frac{c}{2}\frac{1}{1+r_e}\;\;; \quad |\mathbf{u}_s| = \frac{c}{2}\frac{1}{1+r_s}, \tag{10}$$

where $r_e \equiv 2\pi I^-/F$ and $r_s \equiv 2\pi J^-/H$. $r_e \neq r_s$ for finite z. But because F is a positive constant, H is positive and increases with z, and downward intensities decrease with z (They represent less downward emitting material with increasing z), both r_e and r_s are decreasing functions of z. Moreover because of the boundary condition that downward intensities must vanish at the top of the atmosphere we find that $|\mathbf{u}_e| \neq |\mathbf{u}_s|$, both are increasing functions of z, and

$$\lim_{z\to\infty}\mathbf{u}_e = \lim_{z\to\infty}\mathbf{u}_s = \frac{c}{2}\,\hat{\mathbf{k}}. \tag{11}$$

This is the equilibrium limit because $\lim_{z\to\infty}\nabla\cdot\mathbf{H} = 0$. Note that the factor of 2 reflects the plane parallel geometry.

This simple example illustrates a number of properties of generalizations of wind:

1. Non-mechanical velocities are not subject to mechanical forces, so the acceleration of $\mathbf{u}_e$ and $\mathbf{u}_e$ is not problematic.

2. They are nonetheless the velocities an observer must move at to prevent "wind" currents of entropy or energy from passing through the observer's frame.

3. There is no frame that freezes all currents at once in the absence of thermodynamic equilibrium.

4. A unique rest frame occurs only where entropy production stops—at the top of the atmosphere in this example.

These generalized winds have implications for how we might think of fluids, using radiation as inspiration, when the mechanical rest frame is not the central physical question as it is for (3).

4. A Modified Radiation-Fluid Mechanics For Climate Regimes

What might fluid mechanics look like for a climate regime considering the issues raised above, when proceeding from kinetic theory? For this section we will assume that all densities and fluxes are appropriate to a climate regime, dropping superscripts and subscripts unless specifically indicating otherwise. We will use the following list of assumptions based on the properties discussed above:

1. $|\mathbf{u}_m|$ tends toward being small or 0 in the long-time limit. It will be much smaller than in the laboratory regime. By not requiring it to be strictly 0, mechanical winds such as trade winds for example may be expected. But even such mechanical winds do not regain their laboratory regime importance because there is no local equilibrium.

2. The Earth is the standard reference frame. $\mathbf{u}_m$ loses its special status among other velocity field rest frames because it is no longer locked to $\tilde{n}_M$ of (4) that links fluids to intensive thermodynamic quantities. It is only one of many types of generalized wind in that regime.

3. Long term flows are stratified in the sense $\mathbf{u}_m\cdot\mathbf{g} \approx 0$, where $\mathbf{g}$ is the downward vector acceleration due to gravity. This assumption is not theoretical requirement. Clearly persistent flows can have vertical components in principle, but for simple expository purposes we assume that for suitable space coarsening, vertical flows cancel out over long enough times. Thus there is no fundamental limitation to hydrostatic requirements or exclusion of three dimensional flow.

4. n is approximately an even function about the origin in momentum space.

All or some of these assumptions are subject to later review, particularly in terms of refining the approximations indicated. But for the purposes of this paper they will generate a kind of fluid dynamics that is different from (3).

The total time derivative $n(\mathbf{r},\mathbf{p},t)$ is

$$n_t\ + v_i n_{x_i}\ + m g_i n_{p_i}\ = C. \tag{12}$$

where subscripts i and j denote indices and other subscripts denote partial derivatives, and the summation convention is in effect. C is a rate of collisional addition and removal of particles from the phase space neighborhood. It is normally presumed to have properties that cause moment integrals over it to vanish (Duderstadt and Martin 1979).

Multiplying (12) by m and integrating over momentum space, using (2), as well as considering reasonable limits at infinity, yields,

$$\frac{\partial\rho}{\partial t} + \nabla\cdot\mathbf{G} = 0 \tag{13}$$

This is just the continuity equation, but n is presumed to be suitable for a climate regime in this case, so despite appearances, it is not physically the same, even if it belongs to the same mathematical family.

Multiplying (12) by $\mathbf{p}$, then integrating over momentum space we find

$$\frac{\partial \mathbf{G}}{\partial t} + \nabla \cdot \mathbf{\Pi} = \rho\, \mathbf{g} \tag{14}$$

where

$$\mathbf{\Pi} = [\Pi_{ij}] = \left[\frac{1}{m}\int p_i\, p_j\, n\, d^3p\right]. \tag{15}$$

Note that $\mathbf{\Pi} \neq \mathbf{P}$ from (3), as the reference frame is set according to assumption 2 above. $\mathbf{P}$ is given by

$$P_{ij} \equiv \frac{1}{m}\int (p_i - mu_i)\,(p_j - mu_j)\, n\, d^3p = \int (v_i v_j - v_i u_j - u_i v_j + u_i u_j)\, m\, n\, d^3p$$
$$= \Pi_{ij} - \rho u_i u_j. \tag{16}$$

where u_i is the ith component of $\mathbf{u}$. The last term on the right side is the cause of the famous nonlinearity on the left side of (3). If one uses (16) in (14) the equation becomes once again centered on a mechanical reference frame and the nonlinearity arising from traditional kinetic theory re-emerges. Thus the climate momentum equation (14) is linear. We interpret $\mathbf{\Pi}$ as the momentum flux densities, one for each momentum component, leading to a flow velocity for each component.

Finally, using assumptions 3 and 4 above, multiplying (12) by $|\mathbf{p}|^2 = p^2$ the balance equation for internal kinetic energy results,

$$\frac{\partial \mathcal{E}}{\partial t} + \nabla \cdot \mathbf{K} = 0 \tag{17}$$

$$\mathbf{K} = \frac{1}{2m^2}\int \mathbf{p}\, p^2\, n\, d^3p \quad \mathcal{E} = \frac{1}{2m}\int p^2\, n\, d^3p. \tag{18}$$

In the climate regime, the three equations (13, 14, 17) would replace classical fluid mechanics equations of continuity, momentum and internal energy. Similar equations were dismissed previously for the laboratory regime in deference to (3) (Duderstadt and Martin 1979, Essex 2011).

The parallel to radiation has an advantage over the laboratory regime. In the laboratory regime radiation and fluid mechanics are mathematically as well as physically distinct and thus often considered as separate problems. The laboratory regime fluid equations are indeed closed on their own, but in this modified regime the equations for radiation follow naturally to complete the picture. n^r, the suitably coarsened mean occupation number for photons (from (7)), can be differentiated, similarly to (12), while noting acceleration is zero,

$$n^r_t + v_i n^r_{x_i} = B, \tag{19}$$

where B is the interaction term with matter and $v_i = c\mu_i$ (μ_i are direction cosines). This becomes the equation of transfer using (7). With (8) we find,

$$\frac{\partial u_r}{\partial t} + \nabla \cdot \mathbf{F} = \epsilon, \tag{20}$$

where ϵ is the local rate energy is going into the radiation field. $-\epsilon$ would need to appear on the right side of (17) in the event this energy exchange from matter to radiation were nonzero.

For radiation entropy (6) produces,

$$\frac{\partial s_r}{\partial t} + \nabla \cdot \mathbf{H} = \sigma_r, \tag{21}$$

where σ_r is the local radiation entropy production rate. A similar equation would hold for the entropy production in the material component of the radiation-fluid medium (Essex 1987) which would also be subjected to coarsening, yielding a complete entropy balance equation for both matter and radiation

$$\frac{\partial s}{\partial t} + \nabla \cdot \mathbf{Y} = \sigma, \tag{22}$$

where s is the entropy density of matter and radiation, $\mathbf{Y}$ is the flux of entropy, and σ is the full entropy production rate.

Unlike the classical equations, all of these equations (13, 14, 17, 20, 21, 22) are linear. However we do not know what n is, unlike the case of the laboratory regime. Neither do we know n_r, not to mention know how we should handle clouds or scattering or rain etc. in connection with radiation. Solargraphs show no clouds except in the solar arches. Rain also does not appear in the solargraphs either, although it surely falls in the landscapes imaged.

Another drawback of this system of equations is that it is not closed. Investigating the prospect of closing them will surely have many complications. But radiation equations in the laboratory regime are not closed anyway and there may well be simplifications from coarsening. Moreover prolonged attempts to close time integrals over classical fluid mechanics remain unsuccessful too. Each of the matter equations induces generalized winds, generating five distinct velocity fields plus one for entropy. For radiation there are two, bringing the total to eight. The deviations between these vector fields would be the focus for this approach to climate fluids. Closing these equations ideally would be done with matter and radiation together as one. The similar structures as well as simplifications from coarsening may make this possible.

5. Conclusions and Discussion

There is no physical theory of climate, even though there has been plenty of famous theoretical work done on the subject (Essex 2011). This is understandable given the enormity and complexity of the problem. But this paper suggests that there is a complication beyond that explanation. The classical physics of the laboratory regime (classical thermodynamics, fluid mechanics etc) with which we explain the everyday world of our experience is rooted in the laboratory regime. A case has been made in this paper, through the direct evidence of solargraphs in the first instance, that an observer who could perceive climate directly would see a world that would would differ from our everyday world. There are no clouds or rain. In avoiding “over exposure,” things we would expect to see in the laboratory regime must be invisible to an observer in the climate regime. On the other hand, as the solargraphs show, an observer in a climate regime can also see things clearly invisible to us.

Conditions such as local thermodynamic equilibrium, which we rely on in meteorological applications, become problematic. No Maxwellian-like distribution can be expected in a climate regime. The usual thermodynamic intensive variables, so central to our daily lives, cease to have meaning without that. The mechanical rest frame producing the vector flow field that defines classical wind loses its importance without it too. Moreover it is argued here that this field will decrease in magnitude toward zero in the long-time limit.

The importance of the classical mechanical rest frame is connection to local equilibrium and its thermodynamic consequences rests on the Maxwell Boltzmann distribution for classical particles. This raises the prospect that primitive laboratory scale equations lock us into the laboratory regime no matter how they are averaged. Not only is, for example, Navier Stokes order missing something over very long times (Essex and Davison, 1998), but the physical properties that a climate regime observer would measure would likely be different than what we observe in the laboratory regime, requiring different sorts of instruments measuring different physical properties.

A suggestion for such properties is the generalization of the classical notion of wind. Many distinct velocity field rest frames co-exist in a dynamical continuum simultaneously. Each determines a rest frame that stops a flow for an observer riding at the velocity in question. But no frame exists that stops all flows except in thermodynamic equilibrium. This helps replace the thermodynamical intensive variables. A proof of concept was made in a simple radiative transfer model. It was shown that radiative energy and entropy velocities differed throughout except at the top of atmosphere where radiative entropy production stopped (Sieniutycz 1996).

Radiation is a good example because overlooking fluid rest frames gives the kinetic description a character not unlike radiative transfer. In the case of radiation this is because a rest frame does not exist physically, while for the fluid it is because the mechanical rest frame approaches a single standard rest frame (i.e. the Earth) over sufficiently long time scales and has no other special role. Linear fluid equations have been deduced here for a climate regime. They are similar to equations produced to illustrate the importance of the mechanical rest frame in the laboratory regime in the absence of collisions or body forces (Duderstadt and Martin 1979). However in this case the problem is turned around. The mechanical rest frame is not so important, body forces play a role and simple assumptions enumerated in section 4 allow slightly modified equations to hold nonetheless.

This work remains speculative because the equations are not closed even if they are charmingly linear. But the resulting fluid mechanical equations, (13, 14, 17), harmonize well with radiation and entropy equations,(20, 21, 22), raising the prospect of closing the entire system and not just the fluid equations alone. (3) has been studied for more than a century with the unfulfilled aim of closing the fluid equations in some average, empirical closure schemes notwithstanding. Radiative transfer is not normally even considered from a closure perspective. On the other hand attempts to close the system presented here with matter and radiation together has no such history. Perhaps some broad structural constraints can be shown to apply to n and n_r for climate, which can be filled in with direct observation. Maybe closing the equations will destroy the linearity, or maybe no physically meaningful closure exists at all in nature. These possibilities all remain open.

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