Vlasov-Maxwell kinetics: theory, simulations and observations in space plasmas

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Fluid versus kinetic: gains versus losses.

F. Pegoraro

A rather personal and very limited overview:
to be taken simply as a possible starting point for discussions.

Dipartimento di Fisica Università di Pisa pegoraro@df.unipi.it

Which plasma description?

One of the main features of plasma physics, and for that of physics in general, is that it cannot not, and must not, aim at a description of the system under investigation that accounts for all its "accidental" details.

It must first identify the phenomena that are judged to be "essential" to the description of the system and then focus its efforts towards the description of these phenomena.

This is in particular the case for plasma physics since there is no fundamental set of equations, such as (under special conditions) the Vlasov Maxwell[1] system, that can be applied without additional severe simplifications over all the relevant range of time and space scale lengths of the system.

[1] 150 years ago in March James Clerk Maxwell wrote down "Maxwell's" equations

On the other hand this is particularly difficult for plasma physics since, in the absence of even local thermodynamic equilibrium, the response of the system is not "universal" and depends strongly on the details of the system itself. These latter are widely variable and often unknown.

This dichotomy makes the physical description of plasma phenomena hard to formulate, with different degrees of "realism" being required within different approaches and in particular in the interpretation of experimental observations both of laboratory and of space plasmas.

The use of fluid[2] or of kinetic plasma descriptions falls in part within this dichotomy.

[2] Misnomer: the correct word should be *Moment equations.*
The (assumed) conservation of the fluid element is not based on the mean free path being the shortest length and the collision frequency the largest frequency, but on some *ad hoc* closure parameter.

In fact the most appropriate choice of description is not only determined by its strict validity given the specific physical conditions of the system: fluid descriptions would indeed rarely apply to space plasmas.
To a large degree it is determined by an "a priori" choice of what are considered to be the essential features in the phenomena under investigation.
In addition, in most cases, it is also determined by the available numerical resources.

Stepping from a physically fully correct set of equations to a set of model equations whose continued applicability e.g., cannot be a priori guaranteed as the system evolves, clearly brings a loss to our understanding of the system.

However at the same time it often makes it possible to describe the behaviour of the system in a way that can be comprehended, which would not be the case otherwise even if unlimited numerical resources were available and would provide a full solution of the exact equations.

Old-fashioned example: electrostatic drift waves

Drift waves are of basic importance for understanding anomalous particle and energy transport in magnetically confined plasmas. They come in all forms and shapes but, in a low β plasma, they occur in their simplest version as ion acoustic waves modified by the presence of a density gradient perpendicular to the plasma magnetic field. They are oblique modes, $k_\perp \gg k_{||}$, with parallel velocity in the range $v_{thi} < \omega/k_{||} < v_{the}$ and linear dispersion relation

$$\omega^2 - \omega\omega^* - k_{||}^2 c_s^2 = 0, \qquad \omega \sim \omega^* \quad \text{for} \quad k_{||} \to 0, \tag{1}$$

where[3] $\omega^* = k_\perp v^*$ and v^* is the diamagnetic electron velocity related to the diamagnetic current density that maintains total pressure balance.

[3] Imprecise definition, just for the sake of simplicity

In this dispersion relation electrons are $isothermal$ along field lines.

In a kinetic (Vlasov) description, Landau damping makes drift waves resonantly unstable (positive derivative of the electron distribution function along an oblique direction in phase space involving $v_{||}$ and $x_{\perp}$ through ω^*).
Same for ion acoustic waves if the electrons have a parallel drift u_e (with $u_e/c_s > 1$).

Is is easy to see that, if one computes the second order velocity moment of Vlasov equation in order to obtain an energy equation, the Landau damping term implies a violation of the isothermal equation of state.

This was (first) used in the '70s to mimic the effect of Landau damping by considering a finite electron thermal conductivity along field lines[4] (the isothermal equation of state corresponds to infinite conductivity).

[4] B. Coppi: Notes from the Plasma Physics Lectures given to graduate students at MIT around ~ 1974

Clearly collisional conductivity (a la Braginskii) gives the right effect but the wrong dependence of the growth rate on the plasma quantities.

What is the use, now, of this result? Simply pedagogical?

Heuristically it is quite useful in particular when you try to understand the excitation of drift waves with simplified models based on the plasma response to a "virtual" displacement.

Physically :
1) in a collisionless regime it leads to fake dependencies of the growth rate,
2) and it relies on $linear$ orderings and considerations.

Point 1) can be corrected with more sophisticated models that include e.g. features taken from the (kinetic) plasma dispersion function.
A lot of work has been done also recently in this direction, with useful results if I understand correctly, but I am not an expert in the use of these techniques.

Point 2) is in my opinion intrinsically more problematic.

Even at small amplitudes, as far as I see, it makes you loose important "nonlinear" modes such as the so called electron acoustic waves[5] and, at lower frequencies, the analogous "Ion bulk" waves[6].

In addition the mode amplitude does not appear in the closure orderings and as a consequence fluid models cannot be used, or at least become very cumbersome[7], if e.g., trapping phenomena occur.

[5] *a posteriori* you can recover electron acoustic waves with fluid equations by using two *ad hoc* electron populations, a "cold" one that would correspond to ions in the ion acoustic waves, and a hot one that would correspond to the shielding electrons in the ion acoustic waves.

[6] such modes seem to be of interest for solar wind data, I have no idea whether their drift modification could be of any interest in a laboratory context

[7] See e.g. the semi-fluid models of collisionless shock waves when the "soliton solution" is turned into a "shock solution" by introducing electron trapping and ion reflection

Since we are dealing with nonlinear (essentially) Hamiltonian systems space and time scales are not conserved during the system evolution and are not (or only weakly) bound by dissipative effects.

This means that in practice any closure condition will turn out to be eventually invalidated by the system evolution itself, a self destruction mechanism that makes it extremely hard to make long time analytical and even numerical[8] predictions and that can be the source of serious errors.

[8] independently of consideration of numerical accuracy.
Actually the validity itself of collisionless Vlasov equation can be questioned in the infinite time limit.

Anisotropy and pressure closures

The second order Vlasov moment equation can be written as

$$\frac{\partial}{\partial t}\Pi_{ij} + \frac{\partial}{\partial x_k}Q_{kij} + \frac{\partial}{\partial x_k}(u_k\,\Pi_{ij}) + \frac{\partial u_i}{\partial x_k}\Pi_{kj} + \frac{\partial u_j}{\partial x_k}\Pi_{ik} = \tag{2}$$

$$= (q/m)\,(\varepsilon_{ilm}\Pi_{lj}B_m + \varepsilon_{jlm}B_m\Pi_{il})$$

$$\Pi_{ij} = mn\langle(v_i - u_i)(v_j - u_j)\rangle, \quad Q_{kij} \equiv \langle mn(v_k - u_k)(v_i - u_i)(v_j - u_j)\rangle \tag{3}$$

$$\langle A(\mathbf{x},t)\rangle \;=\; [1/n(\mathbf{x},t)] \int A(\mathbf{x},\mathbf{v},t)\, f(\mathbf{x},\mathbf{v},t)\, d^3v \tag{4}$$

Suppose we take a generalized adiabatic closure[9] $Q_{kij} = 0$ and write Eq.(2) as

$$\partial_t \Pi + \mathcal{H}ydr\, \Pi = \mathcal{M}agn\, \Pi \tag{5}$$

order[10] $\partial_t \sim \mathcal{H}ydr \sim \tau_{hydr}^{-1}$, $\mathcal{M}agn \sim \Omega_c$, with $\Omega_c\, \tau_{hydr} >> 1$, write $\Pi_{i,j} = \Pi^{(0)}_{i,j} + \Pi^{(1)}_{i,j}$ with $||\Pi^{(1)}_{i,j}|| \sim \mathcal{O}[1/(\Omega_c \tau_i)]\, ||\Pi^{(0)}_{i,j}||$ and solve perturbatively

$$0 = \mathcal{M}agn\, \Pi^{(0)}, \qquad \partial_t\, \Pi^{(0)} + \mathcal{H}ydr\, \Pi^{(0)} = \mathcal{M}agn\, \Pi^{(1)}. \tag{6}$$

[9] As mentioned above, better closures can be used for Q_{kij} that mimics Vlasov behaviour: see e.g. the work by the Nice group.

[10] In the absence of a strong magnetic field Eq.(5) would be of little use: if we take $Q_{kij} = 0$ it would correctly predict that an isotropic pressure becomes anisotropic in an hydrodynamical time due to the velocity strain tensor. A related problem, see afterwards in this presentation, is the fact that, in the absence of a magnetic field, a configuration with a shear velocity $u_y(x)$ can correspond to a fluid equilibrium, $(\vec{u} \cdot \nabla)\vec{u} = 0$, but not to a Vlasov equilibrium with non-vanishing temperature (x is not a constant of the motion and there is no canonical momentum to use in Jeans' theorem).

The leading order equation implies that $\Pi^{(0)}$ is diagonal in a local frame aligned along the local magnetic field and has equal eigenvalues in the perpendicular plane. By taking the trace of the first order equation and by projecting along $B_i B_j$ (both operations annihilate $\mathcal{M}agn\,\Pi^{(1)}$) and using the fluid momentum equation to express the derivatives of $B_i B_j$, we recover a double-adiabatic type of closure.

Finally noting that $\mathcal{M}agn$ is proportional to the rotation generator operator acting on symmetric tensors[11] the first order equation can be solved algebraically for $\Pi^{(1)}$ (while its dependence from space and time would require going to next order and using again the annihilators).

We are presently investigating in simple cases whether by setting $Q_{kij} = 0$ the ordering $\Omega_c\,\tau_{hydr} >> 1$ is eventually invalidated by a "mixing" process that creates increasingly shorter spatial scales.

[11] It is proportional to the comutator with the angular momentum operator along $\vec{B}$.

Initial conditions

There is a general feature that is independent of fluid or kinetic models and that is related to the way we model magnetic configurations when we start either an analytical or a numerical investigation.

The commonly adopted approach is to start from an equilibrium configuration (fluid or Vlasov) with an *integrable magnetic field* i.e., from a magnetic configuration for which magnetic surfaces exist[12] and add small perturbations.

In other words our initial conditions on the magnetic field are in the neighbourhood of an integrable configuration.

[12] They always exist locally, but we are interested in global (i.e. large scale) configurations.
$\vec{B} \cdot \nabla \psi = 0$ has no global solution in general (aside for a zero-measure set of configurations).

This poses strong constraints on the initial physical quantities: most of them are constrained to be "Flux functions".
Example: Ferraro's corotation theorem[13] $\Omega = \Omega(\psi)$.

Actually this starting point is not so much a choice but is almost a necessity if we want to use our concepts of instabilities etc., and start from a time independent configuration.
However this corresponds, I fear, to an "old fashioned approach" where in the back of our mind in order to determine an initial plasma state we imagine that the system can be thought of as closed[14] etc. and such an approach misses phenomena of selforganization where e.g., transport adjusts itself to external forcing in determining the global plasma structure (see e.g. *temperature profile consistency* in tokamaks).

[13] This is a rather meaningful, although approximate, constraint because of the *externally imposed* planet magnetic field.

[14] For example in laboratory toroidal plasmas temperature is generally measured not to be a flux function.

The important question would be: *is the evolution of near integrable configurations generic?* in the sense that the system would loose trace of these very special initial conditions due, e.g., to the onset of instabilities ?

In a number of cases, in particular in the laboratory, near integrable conditions make sense but not in space in general.

Would it be possible to start from a more general (not integrable) time independent initial con figuration (or weakly time dependent) and still give a meaning to specific instabilities? (for example what does it mean in such configuration a reconnection instability?).

Must we only resort to statistical analyses or can we still follow and understand single dynamical events as we do when we start from exact equilibria?

The situation is even worse in the case of kinetic simulations because of the in principle rather arbitrary, but in practice very limited, choice of initial self consistent distribution functions (difficulties when applying Jeans' theorem to selfconsistent configurations in finding selfconsistent constants of the motion).

For example, in the galactic dynamical context people resort to "filling orbits" in a rather unintuitive procedure in order to produce the required density.

Look also at the fact that only one force-free Vlasov equilibrium was known until recently (with spatially rotating fields) and that the recent beautiful extension[15] of the Harris solution to force free configurations leads to somewhat unphysical, double bumped distribution functions (possibly two-stream unstable).

[15] M.G. Harrison, T. Neukirch, One-dimensional Vlasov-Maxwell equilibrium for the force-free Harris sheet, *Phys Rev Lett.*, **102**, 135003 (2009)

Could we take fluid equilibria to initiate kinetic simulations? i.e. distribution functions such that force balance is macroscopically satisfied[16] but that are not not stationary solutions of Vlasov's equation?

Apparently such initial conditions in kinetic simulations do not seem to behave wildly (actually I am not certain why) and they are frequently adopted in kinetic simulations.

The situations is possibly at bit better with reduced kinetic descriptions, such as gyro-kinetic or better drift-kinetic equations, but I have no direct experience in such type of approaches.

[16] Note that in general there may be no Vlasov equilibria that correspond to a fluid equilibrium.

Locally Hybrid approach

As mentioned before, the violation of the Vlasov closures in a number of cases seem to occur locally in physical space (or more precisely there are regions where such violations cannot be hidden in the model).

Is it possible to run fluid codes and to open them up only in selected regions by using kinetic equations (Vlasov or some reduced kinetic description) only locally?

Which kind of boundary conditions do we impose on the kinetic simulations ?
(Shifted) Maxwellian distributions that have the correct fluid parameters ?

It is far from obvious to me that this is justified and more properly that it is generic in the sense that it does not constraint the kinetic simulations too tightly.

And then? No non-trivial answer in mind

The problem is not that much, or not only, a question of codes, memory and computational power, it it also a question of scientific semantics.

A one to one representation of the system is technically impossible[17] at the moment and cognitively not very useful (unless maybe we are looking for yes or no type answers such as in a fusion experiment: is the plasma igniting or not ?).

The question of which model description should be adopted for a given problem is better answered in a heuristic way i.e, which is the approach that in a given phase of research is capable to providing relevant results?

[17] It is partly different in relativistic laser plasma simulations where time scales of interest are short and the system seems to be less complex e.g., in general for the time of interest ordered (fluid) particle energy is much larger than disordered energy etc.

The questions we ask must be gauged to the specific state of development, and deeper questions that can be physically justified could be essentially inopportune at a certain stage of research.

The devil is in not overdoing with the results obtained and it is absolutely essential to be aware that they are incomplete and that in particular they cannot be extended blindly beyond their limited range of validity.

Personally for example I feel uneasy with the $ultraconfident$ use that is sometimes made of MHD results in contexts where in my opinion their degree of predictability is far too low to make them credible, unless the use of these equations is agreed upon as a first provisional exploration with a far more diversified approach eventually in mind.