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Research

Article submitted to journal

Keywords:
GFD

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Geophysical Fluid Dynamics: Whence, Whither and Why?

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This article discusses the role of Geophysical Fluid Dynamics (GFD) in understanding the natural environment, and in particular the dynamics of atmospheres and oceans on Earth and elsewhere.

GFD, as usually understood, is a branch of the geosciences that deals with fluid dynamics and that, by tradition, seeks to extract the bare essence of a phenomenon, omitting detail where possible. The geosciences in general deal with complex interacting systems and in some ways resemble condensed matter physics or aspects of biology, where we seek explanations of phenomena at a higher level than simply directly calculating the interactions of all the constituent parts. That is, we try to develop theories or make simple models of the behaviour of the system as a whole. However, these days in many geophysical systems of interest, we can *also* obtain information for how the system behaves by almost direct numerical simulation from the governing equations. The numerical model itself then explicitly predicts the emergent phenomena – the Gulf Stream for example – something that is still usually impossible in biology or condensed matter physics. Such simulations, as manifested for example in complicated General Circulation Models, have in some ways been extremely successful and one may reasonably now ask whether understanding a complex geophysical system is necessary for predicting it. In what follows we discuss such issues and the roles that GFD has played in the past and will play in the future.

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1. Preliminaries

What is Geophysical Fluid Dynamics (GFD)? Broadly speaking, it is that branch of fluid dynamics concerned with any and all things geophysical. It thus deals with such fluid phenomena as the Earth's interior, volcanoes, lava flows, ocean circulation, and planetary atmospheres. In this article I will mainly discuss matters associated with atmospheres and oceans, for that is my area of expertise, and I will take GFD to include the fields of dynamical meteorology and oceanography. The moniker 'GFD' has also come to imply a methodology in which one makes the maximum possible simplifications to a problem, perhaps a seemingly very complex problem, seeking to reduce it to some bare essence. It suggests an austere approach, devoid of extraneous detail or superfluous description, so providing the fundamental principles and language for understanding geophysical flows without being overwhelmed by any inessentials that may surround the core problem. In this sense GFD describes a method as well as an object of study.

Although one might think that such a method is, *of course,* entirely appropriate in all scientific areas, in some branches of science there is a tendency to embrace the complexity of reality by using complicated models, to which we add processes whenever possible rather than taking them away. This approach certainly has its place, especially if our concern is in making detailed predictions of the behaviour of a complex system, such as we might if we are engaged in weather forecasts or climate predictions, and climate models have grown enormously in complexity over the past few decades. The GFD approach and the modelling approach have at times diverged, when both approaches (and a range in between) may be needed to gain a proper understanding of a problem. Still, the rift between the two fields is sometimes exaggerated — there are many signs of a realization that we (the scientific community) really do need both.

At the same time as we might be adding complexity, in much of science it is usually taken as a given that we should be endlessly seeking the most fundamental level of understanding, and this search has mostly served us well – provided we have a sensible notion of what 'most fundamental' means for any given problem. In physics, for example, it is taken for granted that we should seek to unify the fundamental forces as much as possible; a unified description of the weak and electromagnetic forces is universally regarded as more satisfying and more foundational than a description of the two separately. There is no discussion but that we should try to go further, and physicists are now looking, apparently without irony, for a single theory of everything.

The simplifications sought by GFD are not quite like that – they are more akin to those sought by biologists, or condensed matter physicists, or anyone dealing with a complex subject that contains *emergent phenomena*. An emergent phenomena is one that emerges from the collective behaviour of the constituents of a system, and is not a property of its individual components — its equivalent atoms or its primitive building blocks; emergence is a manifestation of a group behaviour. Perhaps the most familiar example is temperature, which is a collective property of the molecules of a system and proportional to the mean kinetic energy of molecules in a gas; the phenomenon of phase transitions is another example in physics. Biology itself may be an emergent phenomenon, or at least aspects of it such as life, speciation and evolution.

Now, although there may be a sense in which biology can be reduced to chemistry, chemistry to physics and so on, it is preposterous to seek an explanation for the emergence of biological systems in the laws of physics. One simply cannot understand biology based solely on the laws of physics, as has been widely if not universally accepted for some time (e.g., Anderson, 1972). At each new level of complexity new properties emerge that do not depend on the details of the underlying laws and qualitatively different behaviour takes place; Darwinian principles care nothing for string theory, and an evolutionary system can be built on the computer with no regard to the particular laws of physics.

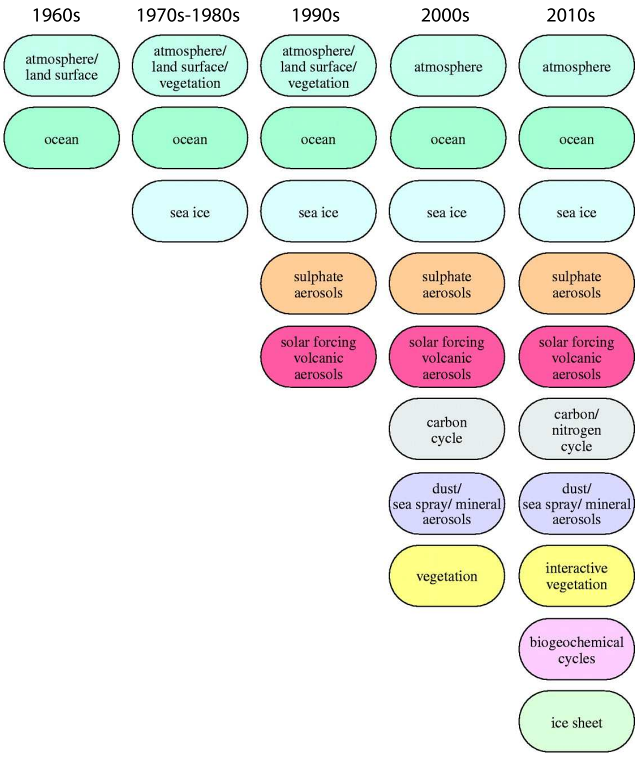

Figure 1. Growth of complexity of climate models over the past few decades. Each component interacts with each other component in models that are over 1 million lines of code.

The construction and function of a gene may be constrained by the laws of chemistry but at a higher level evolutionary principles are not; it would not make sense to seek an explanation of macroscopic evolutionary laws in terms of chemistry or physics anymore than it would make sense to try to understand *Uncle Vanya* using the rules of grammar, even with a dictionary at hand. Indeed, those rules are different in Russian and English, but *Uncle Vanya* transcends that.

Still, there are some emergent properties that we *can* now simulate using the microscopic laws applied to the atoms of the system – the Maxwellian distribution of velocities in a gas for example – but we still would not dream of performing a numerical simulation of the individual molecules to calculate the change in temperature of an ideal gas as it is adiabatically compressed; rather, we seek a direct or macroscopic explanation. In contrast, in some other problems it may be easiest to perform a straightforward computation of their atoms to see how the system evolves — the orbits of planets around their sun, for example. Where does GFD fit in this spectrum?

Regarding GFD

Some of the main goals (and past triumphs) of GFD lie in explaining 'fluid-dynamical emergent phenomena', for example the Gulf Stream in the Atlantic, or a hurricane, for these are not properties of a fluid parcel. However, compared to the complexity of biological systems, or even in some ways phase transitions, these phenomena occupy something of a half-way house: we can seek high-level explanations ('theories') of the phenomena, but we can *also* simulate some of these phenomena quite well using the basic laws of physics, as expressed by the Navier–Stokes equations and associated thermodynamical and radiative equations. These days we do a far better job of describing ocean currents using a numerical simulation than using any theory, analytic or otherwise, that seeks to directly predict them using some more holistic method. Similarly, the climate contains a turbulent fluid (the atmosphere) but our most accurate descriptions of the future climate are made by attempting to simulate the individual eddies over the course of decades and centuries, somewhat akin to following molecules in a simulation of a gas, rather than trying to construct a macroscopic theory of climate. Given all this, is there any need to seek a high-level explanation? In other words, do we still need GFD? The answer, needless to say, is yes, but at the same time GFD needs to continue to evolve and to draw from and give to those large numerical simulations, else it will become irrelevant.

There are two reasons why we might seek to understand a phenomenon. The first is that understanding is an end in itself — we gain something by virtue of a better understanding of the natural world. In this sense science resembles the arts and humanities, and scientific understanding lies alongside the pleasure one might find in listening to a Chopin nocturne (or composing a nocturne if one has the talent), in gazing upon a Turner, or understanding the rise and fall of great civilizations. A famous quote by Poincaré comes to mind: 'Mathematics is not important because it enables us to build machines. Machines are important because they give us more time for mathematics.' In our context, GFD gives us an austere understanding of the behaviour of the natural environment, and that understanding *increases* our wonderment at its beauty, to the benefit of all. The beauty of a sunset, or a cloud pattern, and our respect for a hurricane are all increased by our knowledge of them — as is, in a sibling field, our wonder of a spiral galaxy.

The second reason for seeking understanding is more prosaic; it is that by understanding such phenomena we are able to better predict them and thereby bring a practical benefit to society, for example in the form of better weather forecasts or better climate projections. Sometimes such predictions are made through the use of massive computer simulations, solving the equations of motion ab initio and letting the phenomena emerge from the simulation. It might therefore seem as if there is no longer a need (apart from the aesthetic one) to understand those phenomena that emerge from the Navier–Stokes equations, but this is plainly untrue. If we see a hurricane in the tropical mid-Atlantic we know it will usually move westward, and not because a simulation tells us that. We know that global warming is happening, and will continue, not because of the results of complicated models but because of the basic laws of physics. Even if our goals are solely to improve numerical models then knowledge of the fundamentals is required as we will see explicitly in the next few sections. At the same time a purely theoretical-analytic approach alone is insufficient, and decrying the use of large numerical models is tilting at windmills, for it is in the use of such models *in conjunction* with GFD that the future lies. If we pursue GFD without taking account of developments in comprehensive numerical models we will pursue a dry, jejune field that will eventually become irrelevant.

I should also emphasize that GFD is not, or should not be, a purely analytical-theoretical endeavour. Rather, and without seeking a definition, a GFD approach means seeking the most fundamental explanation of a phenomenon, specifically in the geosciences and often of complex phenomena. Using an idealized numerical model with simple equations (but perhaps complex output) certainly fall unders the rubric of GFD and modern GFD relies as much on such simulations as it does on conventional 'paper and pencil' theory.

The rest of this article expands on these notions, discussing some specific examples and reflecting upon where and how GFD might evolve in the future, focussing on atmospheric and oceanic dynamics. In all matters I express my own view and opinions, and some of the historical discussion can also be found in the endnotes of Vallis (2006). In one or two places the description is technical, but readers from other fields may skim these parts without undue loss. We divide the rest of the article up into the past (whence) and the future (whither), with the 'why' permeating all sections. We skate through the early history at speed, for the intent is not to give a history lesson (or a GFD lesson) but a sense of how the field developed and what it is.

2. The Early History

It is hard to say when the subject of GFD began, for that depends on the definition used. One might call Archimedes (287–212 BC) the first GFDer for he rather famously investigated buoyancy as well as showing that a liquid will acquire a spherical form around a gravitationally attracting point. Blithely skipping a couple of millennia, and focussing more specifically on oceans and atmospheres, we come to George Hadley (1685–1768) who put forth a theory for the trade winds in Hadley (1735). He realized that the rotation of the Earth was of key importance, and that, in order for the trade winds to exist there must be a meridional overturning circulation, and that circulation now bears his name – the Hadley Cell. His paper was perhaps not GFD in the modern sense, however, since he did not use the fluid dynamical equations of motion – he could not have done so since the Euler equations did not appear for another 20 years (Euler (1757), English translation in Frisch (2008)) and the Navier–Stokes equations not until 1822. It may have been Laplace who, in about 1776 (English translation is in Laplace (1832)), was the first to use the fluid equations in a GFD context – he wrote down the linear shallow water equations on a sphere, in the rotating frame of reference (and thus with the Coriolis terms) and forced by an external potential. His goal was to understand the tides and he gave some partial solutions, which were greatly extended by Hough as noted below.

Moving forward to the mid and late 19th century, the notion of linearizing the (too complex) Navier–Stokes equations emerged as a way of understanding various geophysical phenomena — Kelvin waves being a notable example. Meteorology itself advanced too; we find in the work of William Ferrel and James Thomson papers on atmospheric circulation with a recognizably modern flavour (Ferrel, 1858, 1859; Thomson, 1892). Using equations of motion Ferrel tried to account for the multi-celled structure of the Earth's circulation, and although his explanations were wrong (he didn't properly understand the role of zonal asymmetries in the flow, and he envisioned a shallow cell existing *beneath* Hadley's equator to pole cell) he did have an essentially correct view of the Coriolis effect and the geostrophic wind. Thompson's Bakerian lecture in 1892 describes his own work along similar lines and also provides a review on the atmospheric circulation as seen at that time, showing a number of figures similar to Fig. 2. Next to one of them he remarks 'It [is] suggestive of the most remarkable features that would probably present themselves in the winds if the surface of the world were all ocean.' Thus did GFD emerge, for it is this style of reasoning – eliminating the continents as a kind of detail for the problem at hand – that really epitomizes the subject. In any given GFD problem one must always face the question, 'What is a detail?'

In the early 20th century GFD as we recognize it today really began to take hold, in the use of the fluid dynamical equations of motion, simplified if needs be, to try to get at the heart of the problem. This was enabled by some relevant advances in fluid dynamics itself, the extension of Kelvin's circulation theorem to rotating and baroclinic flow by Silberstein (1896) and Bjerknes (1898), and Poincaré's results on the effect of rotation on the shallow water equations (Poincaré, 1893). Thus, for example, Hough (1897, 1898) revisited Laplace's tidal problem, and as part of his solution discovered a form of the Rossby wave on the sphere, a wave re-discovered half a century later in a simpler form by Rossby & Collaborators (1939) — and it was Rossby's simpler form that enabled the wave to be properly understood. Hough's papers were perhaps the first to investigate *mathematically* the importance of Earth's rotation on large-scale ocean currents (or at least Hough thought so), and he discussed whether ocean currents could be maintained by evaporation and precipitation, in the absence of continents. A few years later, and following a suggestion by F. Nansen, Ekman (1905) elucidated the nature of the wind-driven boundary layer at the top of the ocean – or indeed in any rotating fluid – and this helped pave the way for the more complete understanding of ocean currents that came a half-century later in the work of Stommel and Munk. Other areas of GFD were having similar advances, and in dynamical meteorology Defant (1921) and Jeffreys (1926) realized that non-axisymmetric aspects of the atmosphere were essential for the meridional

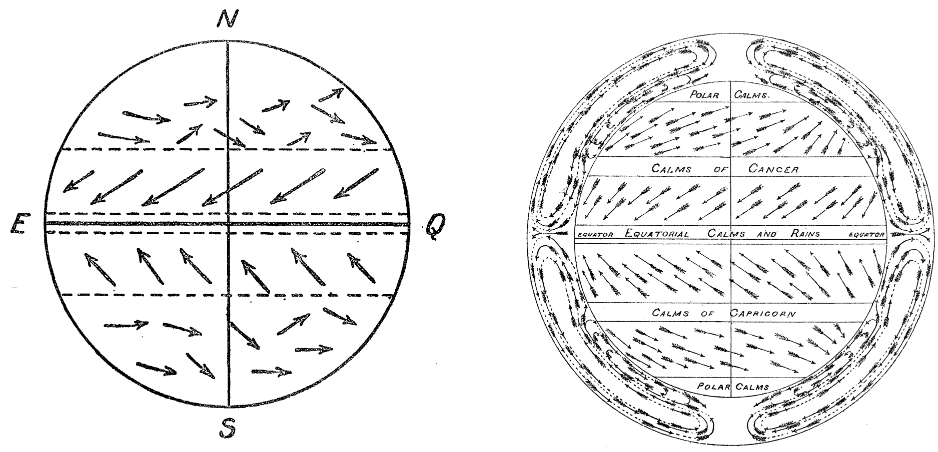

Figure 2. Schematics of the circulation of the atmosphere, taken from the Bakerian lecture of Thomson (1892). The right hand panel shows a pole to equator Hadley Cell, underneath which is a shallow indirect cell, the precursor of the Ferrel Cell, illustrated in fig. 3.

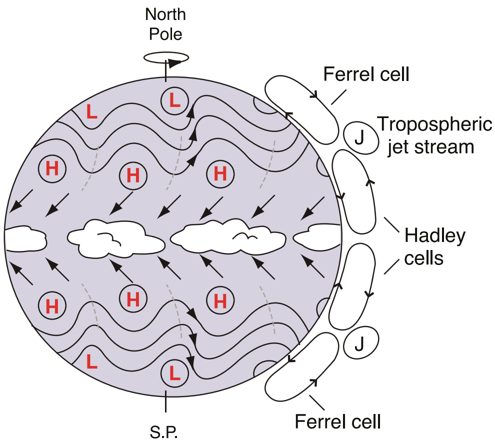

Figure 3. Schematic of the modern view of the zonally-averaged atmospheric circulation, taken from Wallace & Hobbs (2006). The main differences from fig. 2 are the explicit recognition of the importance of zonal irregularities in the flow and the different nature of the Ferrel Cell.

transport of heat and angular momentum – Jeffreys noted that 'no general circulation of the atmosphere without cyclones is dynamically possible when friction is taken into account.'

It is interesting that this period – call it the pre-modern era – is also marked by the first descriptions of possible *numerical* approaches to solving the fluid equations, by Abbe (1901); Bjerknes (1904) and Richardson (1922). Their methodologies were prescient but at the time impractical, and Richardson's actual attempt was wildly unsuccessful, but it led to the revolution of numerical modelling in the second half of the 20th century.

3. The Modern Era

The modern era of GFD began around the middle of the 20th century, when Benny Goodman was in his prime and Miles Davis was emerging, but well before the Beatles' first LP. It began in atmospheric dynamics in the work of Rossby, Charney, Eady and others, and in oceanography with Stommel, Munk and others. By *modern* I mean work that has a direct influence on the work done today – it is no more than one or two intellectual generations from current research. In what follows I won't give a comprehensive or historical view of this work. Rather, I will chose a few specific examples to show how this work has both led to a better understanding of our natural world (which might be regarded as its aesthetic justification) and has led (or not) to practical benefits, sometimes through the development (or not) of better numerical models. The examples chosen are those I am familiar with and sometimes have been involved in, but many others would serve the same purpose and may be equally or more important. I have a large-scale bias, and a few of the more egregious omissions are frontal theory (indeed all of mesoscale meteorology), convection, gravity waves, tides, hydraulics, and El Niño.

(a) Quasi-geostrophic theory and baroclinic instability

One of the first triumphs of modern GFD was the discovery of baroclinic instability and the theoretical framework that enabled it, namely quasi-geostrophic (QG) theory. Charney (1948) may be credited for the first systematic development of quasi-geostrophy, although the term 'quasi-geostrophic' seems to have been introduced by Durst & Sutcliffe (1938) and the concept used in Sutcliffe's development theory of baroclinic systems Sutcliffe (1939, 1947). The full Navier-Stokes equations are too complicated for most meteorological or oceanographic purposes and Charney himself comments in his 1948 paper, 'This extreme generality whereby the equations of motion apply to the entire spectrum of possible motions — to sound waves as well as cyclone waves – constitutes a serious defect of the equations from the meteorological point of view.' Quasi-geostrophy is a particular solution to this problem, and a remarkable achievement, for it reduces the complexity of the Navier–Stokes equations (3 velocity equations, a thermodynamic equation, a mass continuity equation, an equation of state) to a single prognostic equation for one dependent variable, the potential vorticity, with the other variables (velocity, temperature etc.) obtained diagnostically from it – a process known as 'potential vorticity inversion'.

Thus, for a Boussinesq system, the evolution of the entire system is given by

$$\frac{\mathrm{D}q}{\mathrm{D}t} = F, \qquad q = \nabla^2\psi + f + f_0^2 \frac{\partial}{\partial z}\left(\frac{1}{N^2}\frac{\partial \psi}{\partial z}\right), \tag{3.1}$$

along with boundary and initial conditions. Here, q is the quasi-geostrophic potential vorticity, F represents forcing and dissipation terms, ψ is the streamfunction for the horizontal flow, f is the Coriolis parameter, f_0 a constant representative value of f, and N is the buoyancy frequency. Temperature is related to the vertical derivative of streamfunction and velocity to the horizontal derivative, and hydrostatic and geostrophic balances are built-in. This equation is now standard fodder in textbooks such as Pedlosky (1987) and Vallis (2006). The beauty of this equation lies in its elimination of all extraneous phenomena that may exist in the original ('primitive') equations, thereby making the flow comprehensible. It is a line drawing of a rich and detailed landscape.

The development of quasi-geostrophic theory was absolutely crucial to the development of numerical weather forecasts. We noted earlier that L.F. Richardson's attempt in 1922 failed rather miserably, and one reason for this would have been the presence of high-frequency waves in his equations. The development of quasi-geostrophic theory enabled the development of numerical models in the 1950s that filtered sound and gravity waves thereby avoiding the need for complicated initialization procedures and allowing a much longer timestep (Lynch, 2006). The era of using quasi-geostrophic models as forecast tools passed fairly quickly, and use of the primitive equations (essentially the Navier–Stokes equations with hydrostatic balance and one or two other mild approximations) began in the 1960s, but the early importance of quasi-geostrophy can hardly be overstated, for without quasi- geostrophy numerical weather forecasts simply might never have got off the ground. Contrary to its reputation as a difficult subject, *GFD makes things easier*.

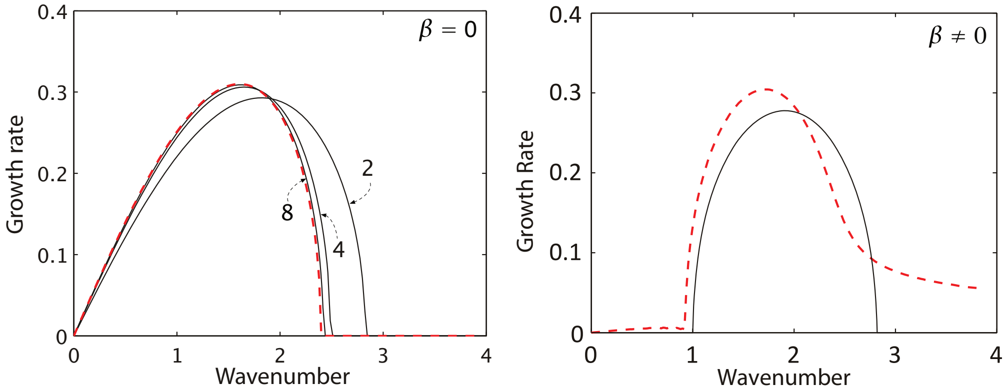

Figure 4. Left: Baroclinic growth rate in nondimensional units with 2, 4 and 8 vertical levels (solid lines, as labelled), and a continuous caculation (red dashed line). The two-level result is the analytic result of (3.4) and other layer resutls are numerical. The continuous result is that of the Eady model. Right, a similar calculation with $\beta \neq 0$, with the two-level calculation (solid line) and a continuous stratification (dashed red line, calculated numerically).

The development of QG theory also allowed Charney (1947) and Eady (1949) to independently develop the theory of baroclinic instability – which, put simply, is a mathematical theory of weather development and is thus one of the most important scientific theories developed in the 20th century. The theory was further simplified by Phillips (1954), who derived the two-layer equations that, when linearized about a constant shear, may be written as

$$\left(\frac{\partial}{\partial t} + U\frac{\partial}{\partial x}\right)\left[\nabla^2\psi_1' + \frac{k_d^2}{2}(\psi_2' - \psi_1')\right] + \frac{\partial \psi_1'}{\partial x}(\beta + k_d^2 U) = 0, \tag{3.2}$$

$$\left(\frac{\partial}{\partial t} - U\frac{\partial}{\partial x}\right)\left[\nabla^2\psi_2' + \frac{k_d^2}{2}(\psi_1' - \psi_2')\right] + \frac{\partial \psi_2'}{\partial x}(\beta - k_d^2 U) = 0, \tag{3.3}$$

where U is the sheared mean zonal flow, k_d is the inverse of the radius of deformation and β is the latitudinal variation of the Coriolis term. In Earth's atmosphere $U \sim 10\,\mathrm{m\,s^{-1}}$ and $k_d \sim 1/1000\,\mathrm{km}^{-1}$, whereas in the ocean $U \sim 0.1\,\mathrm{m\,s^{-1}}$ and $k_d \sim 1/100\text{–}1/10\,\mathrm{km}^{-1}$. The simplicity of these equations allows one to analytically obtain a dispersion relation and/or growth rate for perturbations, and if (for illustrative purposes) we set β to zero we find

$$c = U\left(\frac{K^2 - k_d^2}{K^2 + k_d^2}\right)^{1/2} \qquad \text{or} \qquad \sigma = Uk\left(\frac{k_d^2 - K^2}{K^2 + k_d^2}\right)^{1/2}. \tag{3.4}$$

where c is the wave speed, σ is the growth rate and $K^2 = k_x^2 + k_y^2$ is the total wavenumber, squared. For a complex problem this is a remarkably simple result, illustrated in Fig. 4, along with the corresponding result for a more complete model when $\beta \neq 0$ and the stratification is continuous, and the results are quite similar. There are two important findings that transcend the simplifications of the two-level model, namely

(i) The horizontal scale of instability is similar to, or a little larger than, the Rossby radius of deformation, $L_d = 2\pi/k_d \sim NH/f$, which is a characteristic scale in GFD where rotation and stratification are both important, given a height H. More elaborate calculations bring up constant factors, and the presence of beta and continuous stratification (as in the Charney problem) further complicate the matter, but nonetheless this is a transcendent result.

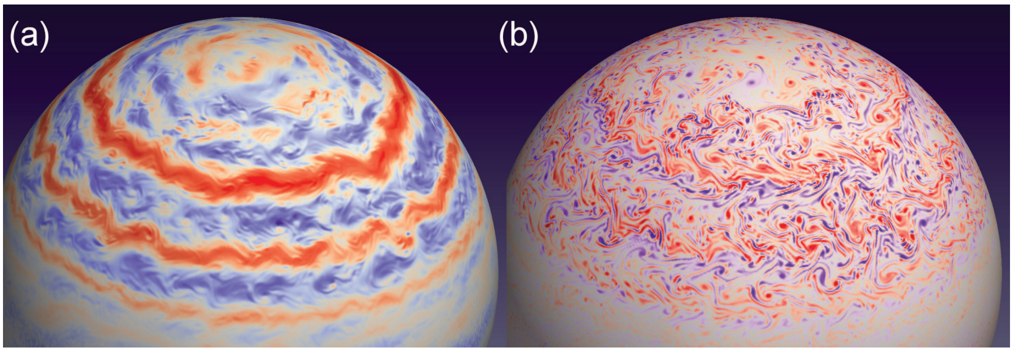

Figure 5. A modern numerical simulation of a rapidly rotating terrestrial planet. The left panel shows zonal velocity and the right panel relative vorticity.

(ii) The maximum growth rate is approximately $\sigma \sim Uk_d$, or Uf/NH. In Earth's atmosphere this is measured in days, and in the ocean weeks. On Mars it is weeks-to-months and the baroclinic waves are much steadier.

The result already tells us something important for numerical models: the grid size needs to be sufficiently small to resolve the instability. This is easily done for the atmosphere, but is still *not* done routinely for the ocean where the deformation radius is down to 10 km at high latitudes. The inability to resolve baroclinic eddies in the ocean suggests that we should parameterize their effects, which is easier said than done. However, imperfect as it is, the parameterization of Gent & McWilliams (1990) led to marked improvements in models of the ocean circulation, to the degree that arbitrary 'flux corrections' could be eliminated. (Flux corrections are empirical fluxes between atmosphere and ocean that were added to coupled models in order that they not drift too far from reality.) The lesson here is two-fold: first, theory is necessary to make numerical models perform better. Second, there are many cases where we cannot and should not expect theory to substitute for numerical models, for as model resolution increases with faster computers we can expect to drop the Gent–McWilliams parameterization; even its redoubtable inventors would admit that a computer model can do a better job than their theory given sufficient resolution. Baroclinic instability in the atmosphere is well resolved by modern GCMs and to predict the atmosphere with anything less than a full-fledged numerical model, with equations close to the full Navier–Stokes equations, would be foolish.

A numerical modeller of Earth's atmosphere may, these days, have little knowledge of the early work of Charney or Eady, but this work played a significant role in the early development of the numerical modelling. Even today, a forecaster wishing to get a sense of the vertical velocity in a developing cyclone may look to the omega equation, which is the the quasi-geostrophic way of diagnosing vertical velocity.

(b) Jets and surface westerlies

Both a triumph and an ongoing problem in GFD is to obtain a better understanding of zonal jets. These jets are manifested in the surface westerlies on Earth and in the magnificent grandeur of the jets on Jupiter. The ideas that we base today's theories on took form in the 1950s with a search for the cause of the surface wind pattern on Earth – easterlies (westward winds) in the tropics and westerlies in midlatitudes, and sometimes weak polar easterlies, depending on season. There is no single paper that can be pointed to as a breakthrough here – Rossby (1949); Starr (1948) and Eady (1950) all realized the importance of large-scale eddy motions, and although Eady realized that the large-scale eddies were the result of baroclinic instability (and not 'just turbulence') even he could not properly crystallize its surface-wind and jet-producing essence. Kuo (1951) addressed the problem in a rather different way by considering the maintenance of zonal flows by the mechanism of vorticity transfer in a state with a meridional background gradient, but the situation remained opaque. As late as 1967, Lorenz noted that the cause of the poleward eddy momentum transport across mid-latitudes, and hence the cause of the surface eastward winds, was not at that time properly explained.

A relatively simple explanation in terms of the momentum transport due to Rossby waves emerged shortly after, in papers by Dickinson (1969) and Thompson (1971, 1980), although ironically Thompson's initial motivation was oceanographic. The essence of the explanation begins with the dispersion relation for a barotropic Rossby wave

$$\omega = ck = \overline{u}k - \frac{\beta k}{k^2 + l^2} \equiv \omega_R, \tag{3.5}$$

implying a meridional group velocity,

$$c_g^y = \frac{\partial \omega}{\partial l} = \frac{2\beta kl}{(k^2 + l^2)^2}. \tag{3.6}$$

Now, the velocity variations associated with the Rossby waves are

$$u' = -\mathrm{Re}\, C\, \mathrm{i} l \mathrm{e}^{\mathrm{i}(kx+ly-\omega t)}, \qquad v' = \mathrm{Re}\, C\, \mathrm{i} k \mathrm{e}^{\mathrm{i}(kx+ly-\omega t)}, \tag{3.7}$$

where C is a constant that sets the amplitude, so that the associated momentum flux is

$$\overline{u'v'} = -\frac{1}{2}C^2 kl. \tag{3.8}$$

This is of *opposite sign* to the group velocity. Thus, a source of Rossby waves is associated with a *convergence* of eddy momentum flux, so that a Rossby wave source in mid-latitudes associated with (for example) baroclinic instability will give rise to surface eastward flow.

GCMs were, of course, *already* simulating the surface wind pattern reasonably well at that time, so one cannot say that the theory led to better simulations, and the nonlinear transfer of momentum by Rossby waves continues to be better simulated than theorized about for the purposes of the general circulation. But the theory does provide an underpinning for the simulation, and also provides needed guidance as we go out of the comfort zone of GCMs, into multiple jet regimes or superrotation on other planets, or possibly a different wind regime in a future or past climate on Earth.

Our understanding of multiple jets – possibly such as those seen on Jupiter – came through a different route, that of geostrophic turbulence and wave-wave interaction (Newell, 1969; Rhines, 1975; Vallis & Maltrud, 1993) with many variations. The basic idea is that nonlinear interactions will, because of the beta-effect, preferentially produce zonal motion at large scale. The concept is extraordinarily robust, as might be suggested from the vorticity equation on a beta-plane, namely

$$\text{Time derivative and nonlinear terms} + \beta v = \text{forcing and dissipation}. \tag{3.9}$$

If β is large then the meridional velocity, v must be small, because otherwise the equation cannot balance. Depending on the parameter regime, the zonal flow is produced by wave-wave interactions or wave–mean-flow interactions of various forms (e.g., Manfroi & Young, 1999; Tobias & Marston, 2013). Explications in physical space as a potential vorticity staircase are also very revealing, and may touch the truth better than spectral arguments (Dritschel & McIntyre, 2008; Marcus, 1993). These various theoretical notions form the basis for our theories of the production of jets not only on Earth but on giant planets, in accretion disks, and possibly stellar interiors, well beyond the capabilities of explicit detailed numerical simulations. Figure 5 shows a high resolution (T512) primitive-equation simulation of a rapidly rotating planet (thanks to Junyi Chai). The numerical planet is terrestrial, with a well defined surface, but has other parameters similar to Jupiter. It is a beautiful simulation, full

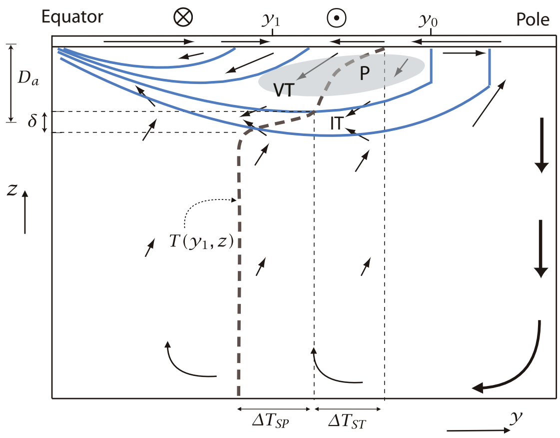

Figure 6. Schematic of the overturning circulation and thermocline in a single basin in a single hemisphere.

of jets and eddies. But it is nowhere close to the resolution needed to resolve the question as to the nature of Jupiter's jets, especially in the vertical, especially given the contrasting paradigms for jet maintenance that require very different types of numerical model to properly simulate them (Vasavada & Showman, 2005).

(c) Ocean circulation

Let's turn our attention to the ocean, and consider two related problems, the thermocline and the meridional overturning circulation.

The thermocline

The thermocline is that region of the upper ocean over which temperature varies quite rapidly from its high surface values to its low abyssal values. It is about 1 km in depth and over much of the ocean it coincides with the pycnocline, the region of high density variation. If we take it as a given that we wish to understand the ocean's structure – after all the ocean covers 2/3 of the globe – then we must understand the thermocline, and this is a GFD problem.

Many of our ideas about the thermocline stem from two papers published in 1950. Welander (1959) posited an adiabatic model, based on the planetary-geostrophic equations, with no diffusion terms, whereas Robinson & Stommel (1959) proposed a model that is intrinsically diffusive. Over the years both models had their adherents, and both models were developed theoretically and somewhat independently (Luyten *et al.*, 1983; Salmon, 1990; Veronis, 1969), with some bridges between the two (Colin de Verdière, 1989; Welander, 1971). Building on all this work, Samelson & Vallis (1997) proposed a model with an adiabatic and advectively dominated upper regime (essentially the ventilated thermocline of Luyten *et al.* 1983) and an unavoidably diffusive base (essentially the internal boundary layer of Salmon 1990), and two points of view became one.

In its most basic form, the theory of the thermocline proceeds by way of deriving the so-called M-equation, which is a single partial differential equation that encapsulates the motion on very large scales in rotating, stratified systems. The planetary-geostrophic equations of motion are

$$-fv = -\frac{\partial \phi}{\partial x}, \qquad fu = -\frac{\partial \phi}{\partial y}, \qquad b = \frac{\partial \phi}{\partial z}, \tag{3.10}$$

$$\nabla \cdot \boldsymbol{v} = 0, \qquad \frac{\partial b}{\partial t} + \boldsymbol{v} \cdot \nabla b = \kappa \nabla^2 b. \tag{3.11}$$

In these equations f is the Coriolis parameter, $\boldsymbol{v}$ is the three-dimensional velocity, ϕ is the pressure divided by the density, b is the buoyancy and κ is a diffusion coefficient. The vertical direction, z, has much smaller scales than the horizontal (x and y) so that we can approximate $\kappa\nabla^2 b$ by $\partial^2 b/\partial z^2$. If we now define a variable M such that $\phi = M_z$, the thermodynamic equation, (3.11b), becomes

$$\frac{\partial M_{zz}}{\partial t} + \frac{1}{f}J(M_z, M_{zz}) + \frac{\beta}{f^2}M_x M_{zzz} = \kappa M_{zzzz}, \tag{3.12}$$

where J is the horizontal Jacobian and the subscripts on M denote derivatives and we take $f = \beta y$. The other variables are obtained using

$$u = -\frac{M_{zy}}{f}, \quad v = \frac{M_{zx}}{f}, \quad b = M_{zz}, \quad w = \frac{\beta}{f^2}M_x. \tag{3.13}$$

Equation (3.12) is amenable to attack by various means — asymptotics, similarity solutions and simple numerical solutions among them (Salmon, 1990; Hood & Williams, 1996; Samelson, 1999). We can simplify this model further if we seek solutions of the form $M(x,z) = (x-1)M(z)$ and then, in the steady case, (3.12) reduces to the one-dimensional problem

$$\frac{\beta}{f^2}WW_{zzz} = \kappa W_{zzzz}. \tag{3.14}$$

Although this equation still nonlinear and of high order, is at least approachable – it looks a little like Burger's equation, and the solution has shock-like features. If we were to add an advective motion *above* the internal boundary layer then the entire upper ocean would become stratified, even at low diffusivity, and a schematic of this is given in fig. 6. The point is that, algebra aside, with a series of rational simplifications we can build a picture of the structure of the thermocline – a picture that both makes testable predictions and gives us some understanding.

So who cares? Where is the practical benefit of a better understanding of the thermocline, or, put another way, why should a numerical modeller or an observer care about a somewhat esoteric theory? At least one answer is that we now have a model of the upper ocean that can produce a stratification that does not solely rely on a large diapycnal eddy diffusivity. The magnitude of that diffusivity is a matter for observation (and a different theory), but there is no need for heroic observational measures to find large amounts of mixing. If observations show that mixing is small in the thermocline then modellers must use numerical methods that can adiabatically subduct water from the mixed layer and that do not introduce spurious mixing.

(d) The meridional overturning circulation

The quasi-horizontal circulation of the ocean – the gyres and western boundary currents – obtained its conceptual model half a century ago in the work of Stommel (1948) and Munk (1950), but the meridional overturning circulation (MOC) had to wait more than another half-century for its own, and still our ideas are evolving. The MOC was first called the 'thermohaline' circulation, in the belief that it was largely driven by surface gradients of temperature and salinity, and the theory of the MOC was closely related to the diffusive theories (or internal boundary layer theories) of the thermocline. The balance in the thermodynamic equation is then, approximately, the advective-diffusive relation

$$w\frac{\partial T}{\partial z} = \kappa\frac{\partial^2 T}{\partial z^2}, \tag{3.15}$$

which is closely related to (3.14). In order to produce a deep circulation of the magnitude observed this balance requires a diffusivity of about $1 \times 10^{-4}\,\mathrm{m^2\,s^{-1}}$ (Munk, 1966), which is about an order of magnitude larger than that commonly observed in the thermocline (Ledwell *et al.*, 1993), and although values of κ in the abyss and in boundary layers may be larger that alone cannot support an MOC as observed. The conundrum was overcome when, over the years, it became realized that the deep water that sank in the North Atlantic

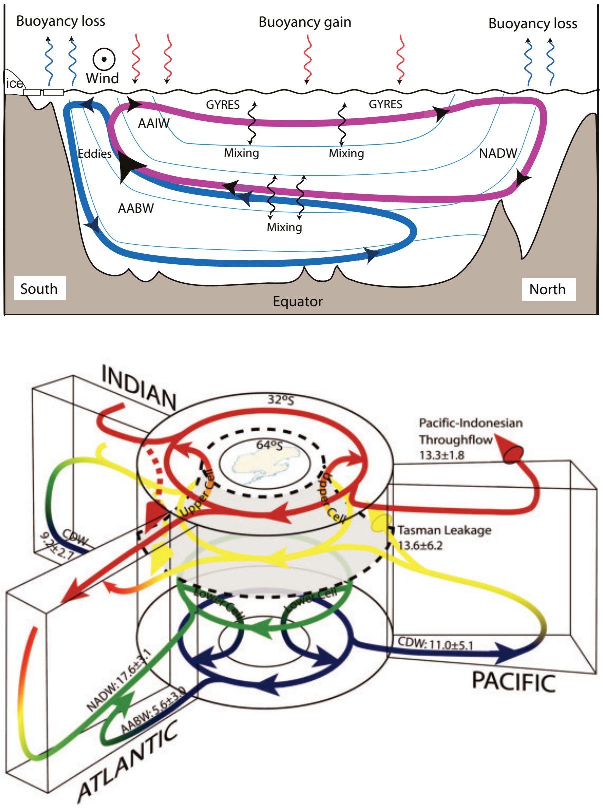

Figure 7. Two views of the meridional overturning circulation, not to scale. The first panel shows the MOC in a single basin resembling the Atlantic, and which is now reasonably well understood with a combination of idealized and comprehensive models and observations. GFD has yet to fully come to grips with the more complex, multi-basin reality, at bottom (courtesy of K. Speer), although it may yet be a minor extension of the single-basin picture.

upwelled in the *Southern Ocean*, and not uniformly in the subtropics and that this could take place with little vertical mixing (Toggweiler & Samuels, 1998; Webb & Suginohara, 2001). This observation led to a picture of a wind-driven, near-adiabatic, pole-to-pole circulation as crystallized using idealized models (Vallis, 2000; Wolfe & Cessi, 2011) and now simulated with full GCMs. Rather interestingly, theoretical models based on the equations of motion (e.g., Nikurashin & Vallis, 2011, 2012; Samelson, 2004) really only properly emerged *after* the first numerical models and observations, albeit with some simple conceptual-model precursors, (e.g., Gnanadesikan, 1999), and it was the use of idealized numerical models that led to the development of the theory. So we cannot claim that analytic theory preceded the numerical models; still, the use of idealized numerical models is part of GFD, and the numerical models of Vallis (2000) and (Wolfe & Cessi, 2011) are close to the being the simplest possible ones that capture the phenomenon, even if their output is complex.

Regardless of the history, theory and understanding can still help improve complex models and guide observations. One example is the realization that an overly diffusive model may give an incorrect picture of the circulation, so that use of either an isopycnal model or carefully-designed advection schemes in conjunction with high vertical resolution in a height-coordinate model is needed (Ilıcak *et al.*, 2012) All told, this is a story of how numerical models, observations and simpler conceptual models (or 'theory') can work in unison to give a fairly complete understanding, although in this case it took a rather long time.

4. Whither? Do we still need GFD if we have comprehensive numerical models?

(a) Generalities

Do we *still* need understanding of the natural environment if we have comprehensive numerical models that simulate the emergent phenomena for us? It is a reasonable question, for certainly some branches of science come to an end – they die of natural causes (Horgan, 1996). However, the truth of the matter is that the presence of complicated numerical models of complex phenomena only *increases* the need for understanding at a more basic level, with such understanding coming from both analytic manipulations and, ever more, from simplified numerical models. GFD has a substantial role to play in this activity for many years to come, but it must continue to evolve or it will wither and there will be no whither.

Two types of contributions of GFD are immediately evident.

(i) Improving the performance of comprehensive numerical models via parameterizations of processes not treated properly by the models, such as subgridscale processes.
(ii) Understanding the behaviour of a system as a whole.

Let us focus on the practical benefits of these activities rather than on understanding as an end it itself (which some readers may take as a given and some as irrelevant).

The subgridscale

The subgridscale has, with justification, provided nourishment for basic fluid dynamics for many years and this will continue. Large-eddy simulations rely on having some understanding of turbulence, and the aforementioned Gent–McWilliams parameterization is a notable GFD example. As we enter an age of mesoscale-eddy resolving ocean simulations it may lose its importance, but the parameterization of the submesoscale (e.g., Fox-Kemper *et al.*, 2008) will come to the fore, and so on. In the atmosphere convection occurs on the kilometre scale and it is impossible to properly resolve it in global climate models, and will be for many years to come – although with heroic efforts we can now resolve the atmospheric mesoscale (Fig. 8). The unresolved breaking of gravity waves is a third example, important in both atmosphere and ocean: in the former the breaking both provides a drag on the flow and produces the QBO, and in the latter it provides a finite diffusivity that partially drives the overturning circulation.

These problems, or their successors, will be with us for years to come (Fox-Kemper *et al.* (2014) provide a quantification) and GFD, and the style it brings, is the field that addresses them. Developing a parameterization sometimes seems to be regarded as dirty work, whereas *it should be regarded as a high calling*, for it is nothing more or less than developing a theory for a phenomena, and expressing that theory in a very practical form.

Still subgridscale improvements alone are unlikely to be enough, and in any case will be hard to achieve, since at any given model resolution it will be the interaction of the subgridscale with the resolved scale that limits the model performance, and we are unlikely to be able to improve the model without some understanding of that – in Fig. 8 we see small convective clusters organized by the large-scale cyclone for example. Doubling or quadrupling resolution is never guaranteed to reveal problems that might involve the feedback of the small scale on the large, a feedback that might act differently in a different climate or planetary regime. Thus, in conjunction with the inclusion of ever more detail, a more holistic approach is needed as we now discuss.

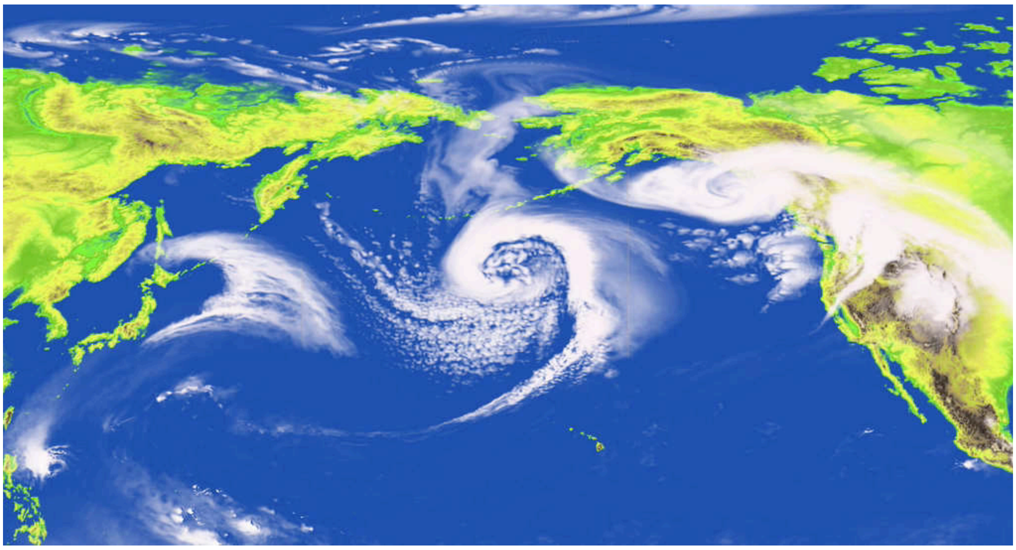

Figure 8. A snapshot of a global numerical simulation at about 12 km resolution, beautifully resolving a mid-latitude cyclone system in the Pacific. From Orlanski (2008).

The system as a whole

The world is a complicated place – far too complicated, as Von Neumann noted, 'to allow anything but approximations' – so how do we understand it? The meaning of 'understanding' is open to interpretation – in some branches of physics it seems to be taken simply to mean the ability to predict the results of an experiment or observation. But for a complex system we might mean the ability to simultaneously grasp the behaviour of the system at multiple levels of granularity. Thus, for example, to fully understand the climate system requires an understanding of a simplified version system as a whole (say, a simple energy-balance model), through an intermediate model to a complicated GCM– this is the renowned 'hierarchy of models' perhaps first adumbrated by (Schneider & Dickinson, 1974) and further discussed by Colin de Verdière (2009); Held (2005); Hoskins (1983) and others and which is, perhaps, more commonly discussed than implemented. Adding complexity to a climate model is no guarantee of improvement, since we may simply be adding unconstrained feedbacks that cause different models to diverge further. Indeed, today's climate models have not succeeded well in narrowing the possible range of the global and regional warming we will experience in the years to come.

The understanding that GFD and simpler models provides is of key practical importance, for suppose we put a lot of effort into producing a large numerical model, and then that numerical model produces the wrong answer, or it produces an answer that differs from another model. What then? We should try to improve the subgridscale representation, but in a complex system like the atmosphere with many feedbacks this is extremely difficult, and virtually impossible unless we have some level of intuitive understanding of the system as a whole, as discussed by Held (2005). If a GCM does not produce a good QBO, we expect to be able to fix it by increasing the resolution and lowering the diffusivity, and by ensuring that tropical convection produces gravity waves of the correct magnitude. But we only know this because we have an understanding of the nature of the QBO, and we cannot apply the fix if we don't understand tropical convection and gravity waves. And so it goes with other poorly-simulated phenomena — the Inter-Tropical Convergence Zone, or biases in the Southern Ocean sea-surface temperature. The fix will ultimately come in the form of an improved parameterization in a new piece of code, but that code cannot be written without some prior understanding. Many of these problems will involve the interaction of GFD with other aspects of the climate system, and GFD must embrace that. This will be a practical challenge as well as scientific challenge, but modern computational tools like scripting and/or object-oriented languages (e.g., Python, Julia) do make model building at different levels of complexity a viable task. Let us be a little more specific about ripe scientific problems in which GFDers, or at least dynamicists, might productively engage. There are a host of interesting pure fluids problems remaining, but in what follows I have emphasized impure problems.

(b) Interaction with physics and chemistry

For historical reasons, in the atmospheric science aspects associated with fluid mechanics have become known as 'dynamics' and aspects associated with such things as moisture, radiation, and even convection (a fluid dynamical problem) are known as 'physics'. The demarcation may be that the latter are not treated by the dynamical core of a model that solves the fluid equations. In any case, ripe problems for the atmospheric sciences lie in the interaction of GFD with these physics problems, and in the ocean comparable problems lie in the interaction of dynamics with biogeochemistry, such as the carbon and nitrogen cycles, and with plankton, as well as the fiendish problems associated with the idiosyncrasies of the seawater equation of state.

Moisture and radiation

Almost the entire general circulation of the atmosphere is affected by the presence of moisture, and needless to say life on Earth depends on water. From a dynamical perspective, water vapour is a decidedly non-passive tracer, releasing significant energy into the atmosphere upon condensation. The condensation itself is a rapid process, occurring much faster that a typical advective timescale, even for convective scale flow, and the criterion for condensation – that the level of water vapour exceed the saturated value – is essentially an exponential function of temperature, as determined by the Clausius–Clapeyron equation. Evidently, nonlinearities and feedback abound, as well as a mathematical stiffness, and understanding moist effects is a challenge both for numerical modellers and theoretical dynamicists. It is hardly original to say that folding moisture into a GFD framework is a challenging and important problem, for much work has already taken place – see Emanuel *et al.* (1987), Emanuel (1994), Lapeyre & Held (2004), Pauluis *et al.* (2008), Lambaerts *et al.* (2011) and Pauluis *et al.* (2010) for miscellaneous examples and reviews – but it is a true statement nevertheless.

A particular example lies in the area of convectively-coupled equatorial waves and the Madden–Julian oscillation (MJO) Kiladis *et al.* (2009). The MJO is an eastward travelling pattern in the tropics, moving at a few meters per second and thus much more slowly than a Kelvin wave with an equivalent depth appropriate to a tropical atmosphere, and it is sometimes called the 30–60 day oscillation, because it seems to recur on that timescale, but it certainly should not be regarded as a linear wave with such a period. It is the phenomenon that is most akin to mid-latitude weather systems in the languid tropics, and so obviously rather important to understand and simulate. Progress has been rather slow, and we are still unable to give a crisp, blackboard-style explanation; nor are we able to unambiguously simulate it with a simple numerical model, and although minimal models exist (Majda & Stechmann, 2009) our understanding of it is not at the same level as it is of dry baroclinic instability. Our ability to simulate it has not moved much more quickly; some cloud resolving models do seem able to capture the phenomenon (Miura *et al.*, 2007), but the progress of GCMs has been slow (Kim *et al.*, 2009), perhaps just outpacing our theoretical ideas. The success of a cloud-resolving model can be regarded as an existence proof that models will get there in the end, but without a better understanding of the phenomena our progress will continue to be slow and GCMs will improve only incrementally. It is a difficult problem, but it is the *kind* of problem that GFD is well placed to attack (in addition to pure fluid problems) and that is obviously relevant to climate. The interaction of fluid dynamics with radiation is another such problem — it is this interaction that gives us the height of the tropopause on Earth and other planets (Vallis *et al.*, 2015) – but let us move on to oceanography.

Ocean Dynamics, paleoclimate, and biogeochemistry

There are many, many, important remaining problems in oceanography of a purely fluid dynamical nature. The meridional overturning circulation is one, for although we now have various conceptual and theoretical models of the MOC in a single basin, we do not have a comparable model, derived from a rational simplification of the equations of motion, in multiple basins. This is rather a basic gap in our field, and such a model would be a considerable advance. Our understanding of the Southern Ocean is also at a slightly cruder level than our understanding of the basins, for one of the foundational dynamical balances – *Sverdrup balance* – cannot apply over large swaths of the Southern Ocean because of the lack of meridional boundaries. Mesoscale eddies then play a leading order role, with all the attendant difficulties of geostrophic turbulence and it may be hard to make qualitative leaps beyond what is already known.

Another class of problem that is open to GFDers lies in the interaction of the circulation with ocean chemistry and biology. This interaction has not been neglected – see the books by Williams & Follows (2011) and Mann & Lazier (2013) – but its importance may be growing. The inherent complexity of the subject naturally suggests the use of complicated numerical models, but a GFD-like approach can also have value, just as it does with climate modelling. The carbon cycle is one such area – the uptake and sequestration of carbon by the ocean is an extraordinarily important problem, for it will in part determine the level of global warming that we suffer in the decades and centuries ahead, and it is a key aspect of the glacial cycles that Earth goes through on the 100,000 year timescale (Archer, 2010; Watson *et al.*, 2015). The carbon cycle is complex, as is the ocean circulation, yet neither are so complex that a more holistic approach is not tenable. At the simpler end of the scale, carbon models have been studied, to good effect, using box models of the ocean circulation but there is a very large jump to full GCMs (Toggweiler *et al.*, 2003). Box models are usefully transparent but most have virtually no proper fluid dynamics, and problems such as the subtle differences between heat uptake and carbon dioxide uptake will require a much better treatment of the physical oceanography. That problem is particularly important in the long term, for it determines how the temperature of the planet will evolve as carbon is added to the atmosphere and whether the temperature will continue to increase, or will fall, when carbon emissions cease. Full GCMs with a carbon cycle ('Earth System Models') are part of that picture, but are less able to help isolate mechanisms, and models and arguments such as that of Burke *et al.* (2015), Watson *et al.* (2015) or Goodwin *et al.* (2015), that at least try incorporate the fluid dynamics in a simple and rational way, can have an important role to play and their growing presence is a hopeful sign.

Paleoclimate is a particular area where the interaction of GFD (ocean dynamics in particular) with the rest of the climate system is key. Observations are sparse and come largely by proxy, and are insufficient to constrain the many semi-empirical parameters of ocean general circulation models. The result is that different models often produce different answers when forced with the same boundary conditions. Theory is not a substitute – it alone cannot decide how the ocean might operate with a different configuration of continents, or when totally covered by ice in a snowball Earth. Rather, theory and idealized numerical simulations can provide rigourous and understandable results in certain configurations, and/or span a parameter space that is unattainable to a comprehensive model. As with simulations of today's climate, simulations with comprehensive models may ultimately provide the most accurate descriptions of the paleo world, but the absence of a large set of observations with which to tune a model will make that task well-nigh impossible if it is regarded purely as a modelling problem.

(c) Planetary atmospheres

For our last example we consider GFD on other planets, in our solar system and beyond. The discovery of planets beyond our solar system – over 3000 so far and counting – is surely one of the most exciting scientific developments to have occurred in the last 20 years, and it now seems almost certain that there are millions, and probably billions, of planets in our galaxy alone. These exoplanets are a shot of adrenaline for GFD, for they bring a host of new problems to the table and, since they lie beyond the solar system, they are within the remit of astronomers and astrophysicists and so bring a new cadre of scientists to the field.

Exoplanets have atmospheres (and some probably have oceans) that come in a variety of shapes and sizes and there can be no single theory of their circulation. There is no planetary equivalent of astronomy's main sequence of stars that shows the relation between stellar luminosity and effective temperature and on which most stars in the Universe lie. Planetary atmospheres differ from each other in their mass and composition, their emitting temperature, the size and rotation rate of the host planet, whether they are terrestrial planets or gas giants or something else, and in a host of other parameters. Some of these atmospheres will be as multifaceted as Earth's but in quite different ways, and the combination of complexity and variety provides a considerable challenge. The complexity of any given atmosphere suggests that complicated numerical models are a natural tool, but this is inappropriate as a general methodology for two reasons. First, it is simply impossible as a practical matter to build a GCM for each planetary atmosphere and ocean. Second, and more fundamentally, the amount of data we have about these planets diminishes rapidly with distance from Earth. We have orders of magnitude less data about Venus and Mars than we do for Earth, and we have orders of magnitude less data for Gliese 581d (a recently discovered, potentially habitable, exoplanet) than we do for Venus and Mars. Any Earth-like GCM that is applied to an exoplanet will be vastly unconstrained, and the danger is that we will be able to get almost any answer we wish for a specific planet. The difficulty lies in reining in our ideas and models – exo-planets are such fertile ground that anything will grow on them, weeds as well as roses. Thus, we have to use simpler models and by doing so we must engage fully in GFD. However, the information we obtain from other planets comes entirely electromagnetically and so GFD is forced to interact with 'physics', as discussed above.

Understanding and classifying the range of circulation types is a place for GFD to start (e.g., Kaspi & Showman, 2015; Read, 2011). This immediately gives rise to a number of poorly understood dynamical phenomenon, for example superrotation, which essentially is the prograde motion of an atmosphere at the equator. This motion implies that angular momentum has a local maximum in the interior of the fluid, which in turn implies that the motion is eddying. Rossby waves are the obvious way to generate an upgradient transfer, and in a slowly rotating regime (such as Titan) they seem to be excited in conjunction with Kelvin waves, with a Doppler shift allowing phase locking, but by no means is the mechanism transparent (Mitchell & Vallis, 2010; Potter *et al.*, 2014). Detailed GCMs still have trouble reproducing the zonal wind on Venus (Lebonnois *et al.*, 2013), and the mechanism of superrotation on the gas giants almost certainly differs again – (e.g., Lian & Showman, 2010; Schneider & Liu, 2009), even if the jets are shallow. More generally the mechanism of the jet formation at all latitudes on Jupiter remains rather mysterious, with various thoughts about the mechanism of coupling of the weather layer to the gaseous interior. The original Jovian dichotomy was that there is either a shallow weather layer, rather like on a terrestrial planet with beta-plane turbulence giving rise to jets (Williams, 1978), or a deep Taylor columns of which the jets are surface manifestations (Busse, 1976). These remain useful end-members, but other variations are possible in which a weather layer is to a greater or lesser extent coupled to deeper jets, giving rise to surface vortices and jets that can equilibrate with little or no bottom friction and that are quite zonally uniform (e.g., Dowling & Ingersoll, 1989; Thomson & McIntyre, 2016), whereas the baroclinic jets of fig. 5 wander a little in the longitudinal direction.

Finally, understanding the possible role of circulation in determining habitability is a GFD problem of a grand nature. The habitable zone is often taken to be the zone around a star in which a planet can support liquid water, and if we know the atmospheric composition we can get an answer just using a a one-dimensional radiative-convective model – generally with complicated radiation and simple dynamics. But we know from our experience on Earth how circulation modifies temperatures, so using a three-dimensional dynamical model with simple radiation is a complementary approach, and both will be needed if we are to gain an understanding of three-dimensional calculations with full radiation, and the problem itself.

5. A final note on education

We can't prescribe how the field will evolve, but we can educate the next generation to be prepared for whatever comes, and perhaps influence what they bring to it. Numerical models are now such a part of the field that there is not so much a danger that students will not be exposed to them, rather that they will not be exposed to the basic ideas that will enable them to understand them, or infer whether the models are behaving physically. Thus, the very basic concepts in GFD are as important, perhaps more so, than they ever were, but can sometimes seem irrelevant; relatedly, there is a danger that analytic skills, if not lost, become divorced from modelling skills. Scientists will always have personal preferences and differing expertise, but combining analytic ideas with simple numerical models can be a very powerful tool in both research and education, and modern tools can be used to enable this at an early stage in the classroom. A numerical model transparently coded in 100 lines and run on a laptop can then play a similar role to that of a rotating tank in illustrating phenomena and explaining what equations mean, and the rift between theory, models and phenomena *then never opens.*

Although conventional books will remain important for years to come, the next textbook or monograph in GFD, or really in any similar field, could to great effect be written using a Jupyter Notebook (formerly IPython Notebook), or similar, which can combine numerical models with conventional text and equations (e.g., LaTeX markup), figures, and even symbolic manipulation in a single document, enabling interactive exploration of both analytical and numerical GFD concepts. Such an effort would be a major undertaking so a collaborative effort may be needed, perhaps like the development of open source software, and the end product would hopefully be free like both beer and speech. Research papers are a separate challenge, but a first step might be to allow supplementary material to be in the form of such a Notebook. There are obvious difficulties with such a program, but if one specifies that the text and figures be self-contained the material can at the least be read as a conventional article or book. Another difficulty is that software standards evolve faster than hardcopy printing methods: it was 500 years after the Gutenberg press before digital typography arrived. Thus, a Jupyter Notebook might be unreadable in 20 years and Python might be passé, but a 'real' book will live on. This problem simply might not matter (immortality is not the goal) but if it does it might be wise not to couple the text and code too tightly. In any case, these are all simply obstacles to discuss and overcome rather than insurmountable barriers.

I hope that it is by now clear that GFD has played an enormous role in the development of our understanding of the natural world. With the emergence of complicated models that role is more important than ever; it may hidden, like the foundations of a building, but without that foundation the edifice will come tumbling down. I will continue to do GFD because it is interesting, important and fundamental. Other peoples' motivation may differ but whatever the future holds GFD, and the approach it brings, has (or should have) an expansive role to play.

Acknowledgements

The author declares no competing interests. There is no data associated with this paper. The work was funded by the Royal Society (Wolfson Foundation), NERC, NSF, and the Newton Fund. I am very grateful to two reviewers for their careful readings and thoughtful comments.

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