# Chapter Four

## Simple models of ensemble-averaged precipitation and surface wind, given the sea surface temperature

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### 4.1 INTRODUCTION

#### 4.1.1 Definition of the problem

In this chapter, we will address the following problem. Consider the sea surface temperature (SST) field as given, and find the quasi-steady component of the tropical surface wind and precipitation field over the tropical oceans. We do not explicitly address precipitation over land.

By "quasi-steady" we mean, in principle, ensemble averaged over all possible realizations associated with that SST, taking deep convection to have a stochastic component. In practice monthly climatologies, or perhaps individual monthly means, will have to be good enough. We are interested in the total fields, not just deviations from the time or zonal means.

We further specify that the problem must be solved without running a general circulation model (GCM). The model must be considerably simpler than that. More specifically, all the models we will discuss here have severely truncated vertical structure, to just one or two vertical modes or layers. Their physical parameterizations are also all greatly reduced in complexity from what would appear in a GCM.

Our goals are to review the various sorts of models of this type that have been constructed, and to assess both the degree to which these models are successful, and to which they are similar to or different from one another. Models constructed on different principles seem to produce qualitatively similar results; we will try to explain this and to ascertain where differences in the results do exist which might be used to test the theoretical ideas underlying the models.

The figures of merit here will be the precipitation and surface wind. The surface wind is important particularly because of its influence on the ocean. Much effort on models of the sort discussed here has been expended in the context of the prediction of the El Niño-Southern Oscillation (ENSO) phenomenon. In such models the precipitation, being proportional to the vertical integral of the convective heating, is of interest primarily through the influence of that heating on the winds. In the present discussion we are also interested in precipitation for its own sake. The questions of to what degree the heating associated with precipitation drives the surface winds, and to what degree convergence of the surface winds drives precipitating convection, will be closely considered. The discussion will focus in particular on what controls the strength and position of intertropical convergence zones (ITCZs).

The limitation to the surface wind means that we do not need to find the free-tropospheric circulation, except to the extent that it may be necessary to do so in order to find the surface circulation. This means that we will avoid questions regarding angular momentum conservation, which are discussed in detail in chapter 5.

### 4.1.2 Observations

We first review the salient features of the observations. Fig. 4.1 shows climatological maps of SST, precipitation, and temperature at 500 hPa for January at tropical latitudes. Fig. 4.2 shows surface wind and divergence (only negative values, i.e. convergence, are shown), from the same season, for the tropical Pacific only. We see that to a crude first approximation, maxima in rainfall and SST coincide, and little or no rain falls over low SST. Temperature gradients at 500 hPa are small compared to those at the surface, so the horizontal structure of the difference in temperature between the sea surface and 500 hPa, which we might take as a crude measure of stability of the atmosphere to deep convection, is governed mainly by the horizontal structure of the SST.

Focusing more closely on the Pacific, we see that in this season, surface convergence is larger in the eastern Pacific ITCZ than in the south Pacific convergence zone, but rainfall is greater in the latter.

### 4.1.3 Outline

Theories for the surface wind and the precipitation are to some degree distinct, although in many cases, as is natural, one theory predicts both. We will consider them separately, although they are obviously related and the relationship is of interest. In both cases we have two broad classes of theories. In broad terms, for both surface wind and precipitation the theories can be categorized into those which view the surface wind (strictly, its convergence) as causing the precipitation, and those which view the precipitation as causing the surface wind. By "causing the precipitation", we really mean here "causing large-scale, large-amplitude horizontal variations in precipitation". In radiative-convective equilibrium (see chapter 3) precipitation will be equal to the local surface evaporation, which itself has some horizontal structure. In practice, however, horizontal variations in precipitation greatly exceed those in evaporation, and are thus balanced in the moisture budget by the variations in the horizontal divergence of the vertically integrated moisture flux due to the large-scale circulation (which when negative is called "moisture convergence" for short). Regarding what causes precipitation, one class of theories tends to focus on moisture convergence, treating it as an external causal factor influencing precipitation. The other class tends to focus on the vertical profiles of temperature and humidity in a column as the important causal factors. The discussion here has considerable projection on a broader debate, occurring over the last decade or so, regarding the parameterization of cumulus convection and the meaning or validity of “conditional instability of the second kind” (CISK).

We will avoid dealing with these issues in their most general form, partly because a number of good reviews address them (Emanuel et al. 1994; Stevens et al. 1996; Smith 1997; Arakawa 2004) and partly because the issues are somewhat different when we focus on the quasi-steady circulation, as opposed to the broader question of convective parameterization in models which aim to simulate variability on a broad range of time scales. Inasmuch as averages such as those shown in figs. 4.1 and 4.2 show much clearer relationships between SST and precipitation (for example) than would be evident in individual daily maps, tropical climate should be easier to understand than tropical weather, and it behooves us to attempt to view the climate problem separately.

For example, convective parameterizations whose closures are based on moisture convergence have problems in both physical justification and behavior which make them unsuited for GCMs. One reason for this is that moisture convergence is not sufficiently external to deep convection to be viewed as an environmental forcing. However, when we are dealing with the quasi-steady circulation only, one may argue that large-scale factors associated with the boundary conditions, such as the SST gradient, induce moisture convergence in a way which may to some extent be viewed as external to the convection, so that a moisture convergence closure may be more defensible. We at least ought to reconsider our views on convective parameterization with an open mind when we are dealing with the quasi-steady problem in isolation. At the same time, inasmuch as there are convective parameterization issues in both GCMs and the simple models we will discuss here, the differences in the nature of the parameterization problems in the two sorts of models — most importantly, the need to simulate transients in one but not the other — mean that we have to be careful in extrapolating any conclusions we may draw from the simple models to the GCM context. This caveat is important, given that much motivation for considering the problems addressed here comes from the problem of understanding tropical biases in GCMs (see chapter 7, this volume).

## 4.2 SURFACE WIND

### 4.2.1 Theory

MATSUNO-WEBSTER-GILL MODELS

A number of models, based on early work by Webster (1972) and Gill (1980), and following still earlier fundamental work on linear equatorial wave theory (Matsuno 1966; Lindzen 1967), assume that the surface winds are driven by heating associated with the condensation of water vapor in deep convection, and that linear dynamics are adequate to understand the surface wind response to this heating. For brevity and consistency with convention, I refer to these as “Gill models” hereafter, while recognizing the contributions of earlier workers. In these models, one assumes a spatial distribution of heating and solves a forced, steady linear wave problem to find the winds. This could be done in three spatial dimensions, but these studies all first assume that the vertical structure is separable and known and has a "first baroclinic mode" form (see Chapter 3), so that only a set of shallow water equations is actually solved. True vertical mode solutions only exist for an artificial rigid lid upper boundary condition. While this prevents some leakage of wave energy to the stratosphere which would otherwise occur, for steady linear flow in the presence of some dissipation the error associated with the rigid lid is not too large (Geisler and Stevens 1982).[1]

Gill's model requires prior knowledge of the heating field, essentially equivalent to knowledge of the precipitation, and thus is not a complete theory for the surface wind, given the SST. A number of models have been constructed which remedy this by developing recipes of varying degrees of complexity for finding the heating, but avoiding convective parameterization in the sense of actual cloud models and the like. Such models include those of Webster (1980), Zebiak (1982, 1986), Weare (1986), Davey and Gill (1987), Seager (1991), Kleeman (1991), and Wang and Li (1993). Some of these, developed for the purpose of seasonal prediction, are anomaly models linearized about a seasonally varying climatology.

Gill models are subject to a number of criticisms. To give reasonable results, they tend to require damping coefficients (mechanical, thermal or both) considerably larger than can be justified based on physical processes actually occurring in the free troposphere. It may be argued that since the desired output is the surface wind, and the free troposphere is not of interest, such strong damping is justified. This is closer to an engineering than a physical argument. The neglect of the barotropic mode may be a large part of the reason such strong damping is needed on the momentum field. When a barotropic mode is included, as in a true two-layer model or the "quasi-equilibrium tropical circulation model" (QTCM) of Neelin and Zeng (2000), weak surface winds tend to be attained by cancellation of the barotropic and baroclinic modes at the surface [see, e.g., Burns et al. (2006) for a detailed discussion of this in the context of an axisymmetric Hadley circulation]. Accomplishing the same thing by damping a pure single baroclinic mode will certainly lead to large errors in the upper troposphere (where the two modes, if both present, will tend to add constructively rather than cancel), but may perhaps cause other problems even for the surface solution.

Another criticism is that when the three-dimensional wave propagation problem is considered for a heat source which has significant amplitude only above the PBL, as may be claimed for deep convective heating, even small damping prevents waves from reaching the surface, due to the small vertical group velocities of the waves of interest, and thus the long time available for damping to act before the signal reaches the surface (Wu et al. 1999). This implies that the resulting surface winds should be much smaller than the first baroclinic mode structure suggests. However, convective heating does in fact reach the surface via downdrafts (though these cool rather than heat). Also, even if the PBL heating is zero, and the direct wave re-

[1] This error increases with frequency and is a major issue for high-frequency transients.

sponse is totally dissipated before reaching the surface so that significant winds are produced directly by heating only above the PBL, turbulent entrainment of momentum into the PBL can bring the heating-induced wind signal down to the surface (Chiang et al. 2001; Stevens et al. 2002).

### LINDZEN AND NIGAM MODEL

Lindzen and Nigam (1987) introduced an apparently entirely different theory for the surface winds. Strictly, they applied their theory only to the zonally asymmetric component of the wind, but showed that it has some relevance for the zonally symmetric component as well, and we will consider its potential relevance to the total wind here. Rather than viewing the winds as a response to deep convective heating, Lindzen and Nigam posited that the winds could be considered to be driven by baroclinic pressure gradients imposed directly on the PBL by turbulent fluxes, which act to effect an adjustment of the PBL temperature toward the underlying SST. The surface pressure in their model is, with a simple linear relationship, low over warm SST and high over cool SST. The pressure gradient at the top of the PBL, which would in general be added to the local SST-related pressure gradient to produce the surface pressure gradient, and which is presumably determined largely by deep convective heating, is assumed negligible. Rather than solving a forced wave problem, the simplest version of the Lindzen and Nigam model consists of linear, damped, shallow water momentum equations with the pressure field given. This simplest version also assumes that the PBL top is at a fixed height, implying vertical mass fluxes through the PBL top to balance horizontal mass divergence. The Lindzen-Nigam model assumes that deep convection occurs in regions of convergence, venting mass rapidly from the PBL so as to keep its depth from increasing.

To obtain reasonable simulations, Lindzen and Nigam found that they had to go beyond the simplest version of their model, and added a “back-pressure” effect in which the PBL top was allowed to rise slightly, raising the surface pressure, in regions of PBL convergence. This effectively amounts to a linear damping on the perturbation pressure [as in (4.3) below]. The back-pressure effect amounts to an assumption that mass venting by deep convection is not infinitely fast. Emanuel et al. (1994) provided an interpretation of the back-pressure effect in terms of downdrafts.

Conceptually, an attractive feature of the Lindzen-Nigam model is that it provides a very simple recipe for obtaining the surface winds from the SST without finding the precipitation. The precipitation can be diagnosed if desired. The claim implicit in the model is that the surface winds are determined independently of the free-tropospheric convective heating. The PBL momentum budget, constrained only by the SST, determines the mass convergence in the PBL. With a simple assumption about the moisture content of the PBL, such as fixed relative humidity, this in turn determines the low-level moisture convergence. Requiring the steady-state diagnostic moisture budget to be satisfied, given some reasonable parameterization of surface evaporation, then determines the precipitation.

In the Lindzen-Nigam model, the PBL pressure perturbation is proportional to the assumed PBL depth. Lindzen and Nigam assumed this depth to be 300 hPa, rather thick compared to typical observations of trade wind boundary layers. It might be argued that the model aliases the free-tropospheric pressure gradient (which is explicitly ignored) into the PBL (Chiang et al. 2001). Another criticism is that the model ignores the mass of the free troposphere, using the full value of gravity where it should use a reduced gravity for purposes of computing the back pressure, but this is easily addressed by a straightforward modification of the model (Battisti et al. 1999).

Another criticism of the Lindzen-Nigam model, as well as other models with equal drag coefficients for zonal and meridional momentum, is that it ought to include the stress on the PBL due to entrainment of momentum from the free troposphere (Chiang and Zebiak 2000; Stevens et al. 2002; McGauley et al. 2004). This entrainment acts differently than does Rayleigh drag with a constant coefficient. This also can be dealt with by straightforward modification of the model, most simply by allowing different drag coefficients for $u$ and $v$

### THE RELATIONSHIP BETWEEN THE LINDZEN-NIGAM AND GILL MODELS

The mechanisms involved in these two arguments, those of Lindzen and Nigam and those based on Matsuno-Webster-Gill type models, are entirely different. In one view the surface wind is determined by convective heating, while in the other convective heating is irrelevant to the surface wind. Nonetheless, taking the coarse-grained, qualitative perspective from which one typically evaluates simple theoretical models, the two views have many important similarities as well, both in their formal structure and in the predictions to which they lead. Because both assume fixed vertical structure, they can both be reduced to a set of linear, damped, steady, thermally forced shallow-water equations (Neelin 1989). Let's write these on an equatorial $\beta$ plane, in somewhat general form:

$$-\beta y v = -\phi_x - \epsilon_u u, \tag{4.1}$$

$$\beta y u = -\phi_y - \epsilon_v v, \tag{4.2}$$

$$\Phi \nabla \cdot \mathbf{u} = Q - \epsilon_p \phi, \tag{4.3}$$

where $\Phi$ and $\phi$ are the mean and perturbation geopotential, $Q$ represents a fixed component of the heating (mainly convective heating, but possibly including a radiative component) and other notation is standard. $Q$ here has units of geopotential per time, and is related to the heating in standard units (e.g., degrees per day) by hydrostatic balance. What vertical structure the prognostic variables are assumed to have varies from model to model. In Lindzen-Nigam, $\phi$ can be thought of as geopotential on a pressure surface near the surface of the earth. It is taken proportional to minus a PBL temperature, which in turn is assumed to be simply related to the SST. In Gill, a deep layer is represented and $\phi$ is proportional to a deep tropospheric temperature. I have assumed Rayleigh-type damping in all three equations, but allowed the three to have different rates, $\epsilon_u, \epsilon_v, \epsilon_p$. One concrete difference between our two views lie in how these rates are chosen. A second, perhaps more important difference lies in how $Q$, the convective heating is determined. Yet there as well, the difference in practice may not be entirely obvious.

### 4.2.2 Surface wind - evidence

A number of recent studies have shed new light on the evidence for one or the other of the two views described above regarding the question of how the surface winds are determined. We are particularly interested in narrow ITCZ regions, where SST gradients are high and we expect Lindzen-Nigam arguments to be most applicable. We do this despite that the flow in such regions projects substantially on the zonal mean, to which Lindzen and Nigam themselves did not claim their arguments were primarily relevant. Much of the debate here can be boiled down to the question of how much of the pressure gradient at the surface is imprinted from above by the free troposphere vs. the hydrostatic result of temperature gradients in the PBL. For the zonal mean zonal wind, this argument is irrelevant because there is no pressure gradient in the zonal mean. Thus this debate applies to the meridional wind (both in the zonal mean and deviations from it) and to the departures from the zonal mean zonal wind.

A key assumption of the Lindzen-Nigam model is that the pressure gradient at the top of the PBL is small enough to be unimportant compared to the component of the pressure gradient imposed directly on the PBL from below by the SST. Observations from the EPIC experiment have been used to test this central assumption of the Lindzen-Nigam model directly (McGauley et al. 2004, Raymond et al. 2004). These observations show that when the time-averaged fields are considered, this assumption holds fairly well for the meridional component of the pressure gradient. The picture is quite different for the synoptic to intraseasonal time scale transients, which have a large component of their pressure gradients imposed from the free troposphere. Most precipitation appears to be associated with these transients, so the relationship of the steady Lindzen-Nigam component of the meridional wind (which presumably accounts for most of the convergence) to the steady component of the rainfall is unclear.

Bacmeister et al. (2002) did a similar analysis for the zonal component of the pressure gradient in both a GCM and the NCEP/NCAR Reanalysis data set. In both cases they found that, for the most part, the free-tropospheric component of the pressure gradient dominated the SST-controlled component. They did not examine the meridional component of the pressure gradient.

Chiang et al. (2001) addressed this issue using a dry linear GCM. They incorporated a PBL of Lindzen-Nigam type, but also a deep heating whose spatial structure was derived from observations. They varied a number of parameters, including PBL depth and damping coefficients, in a systematic way to determine which set gave the best fit of the resulting surface winds to observations. They found that, for the best fit parameters, the free-tropospheric pressure gradient (associated with the deep heating) tended to dominate the zonal component while the SST-controlled component tended to dominate the meridional component.

The agreement among these studies seems to justify a fairly high degree of confidence in the conclusion that the Lindzen-Nigam picture explains much of the meridional wind, particularly in regions of large meridional SST gradient, while free tropospheric pressure gradients produced by deep convective heating explain most of the zonal wind's departures from its zonal mean.

## 4.3 PRECIPITATION

### 4.3.1 Theory

In one view, PBL momentum dynamics drives deep convection by controlling moisture convergence in the PBL. The Lindzen-Nigam model is an example of this view, but there are other examples, which allow factors excluded from the Lindzen-Nigam model to influence the PBL momentum budget. Such factors are free-tropospheric pressure gradients and nonlinearity. In the other view, deep convection is thermodynamically controlled. By this we mean that precipitation is determined by the local profiles of temperature and humidity, the momentum budget is essentially irrelevant to precipitation, and moisture convergence, while diagnostically closely related to precipitation, is not an external factor which can be viewed as causing precipitation. Again, there are multiple ways, differing in detail, in which this view can be expressed in an explicit theory.

As in the case of the surface winds, these two classes of ideas appear totally different. One requires only local information, while horizontal structure is essential to the other. One assumes that precipitation can be found independently of the momentum budget, while the momentum budget of the PBL is essential to the other. I will first describe some of the ideas which fall into these classes in more detail, then consider some evidence which may help us distinguish between them. In the process we will examine whether there are any points of connection between these two apparently very different views.

#### *4.3.1.1 PBL momentum control*

The notion of control of precipitation by the momentum budget in the context of the quasi-steady circulation has roots in an early body of literature [well reviewed by Waliser and Somerville (1994), and Gu and Zhang (2001)], which addresses what sets the latitude of the ITCZ. While in general the ITCZ lies close to an SST maximum, rendering some form of direct control of precipitation by local SST perhaps the most obvious explanation (Pike 1971), a number of studies proposed mechanisms which could place the ITCZ off the equator even if the SST were uniform or had a maximum on the equator. Some of these studies invoked transients (Holton et al. 1971; Lindzen 1974) while others did not (Charney 1971). All of these studies, as well as Waliser and Somerville (1994), make what are essentially CISK arguments — to the effect that, for one reason or another, heating at some particular finite latitude is particularly effective at inducing PBL convergence, which then provides moisture to “fuel” the deep convection which produces the heating. Since these ideas assume a given heating, and then ask how this heating can best induce PBL convergence, they may be classified as momentum control ideas, in that they take the primary bottleneck controlling the occurrence or intensity of deep convection to lie in the PBL momentum budget.

In the Lindzen-Nigam view, described above, the SST induces PBL pressure gradients directly, and these drive the PBL flow by linear dynamics. At places where this flow converges, convection occurs to vent mass from the PBL. Several other theories invoke PBL momentum dynamics as an agent influencing deep convection, but emphasize other aspects of PBL momentum dynamics.

Tomas and Webster (1997) and Tomas et al. (1999) took a longitudinally-independent perspective (appropriate to long, thin, east-west oriented ITCZs) focused on the meridional flow in regions of large cross-equatorial pressure gradient. They took the pressure gradient as given, whether induced directly by the Lindzen-Nigam mechanism, or whether imposed at the top of the PBL. [To the extent that the latter (free-tropospheric) component is important, one expects it to be strongly influenced by the location and intensity of deep convection. Since the theory aims to predict the distribution of convection, the theory is to some extent diagnostic, rather than fully predictive. Tomas and Webster (1997) argued that the pre-existing pressure gradient was induced on "larger scales" by land-sea contrasts or SST gradients, which might be taken to imply that the theory aims to predict perturbations to the ITCZ intensity and location that are in some sense "small scale".] If this pressure gradient is strong enough, the cross-equatorial flow that it drives in the PBL is able to displace the zero absolute vorticity contour significantly off the equator. By a mechanism which involves the meridional advection of relative vorticity, and is thus nonlinear, and which has some common elements with symmetric instability (though the flow itself is not linearly unstable in the presence of substantial friction, as appropriate for the PBL), this leads to strong convergence at a location on the high-SST side of the equator, but, in general, equatorward of the SST maximum, assuming an SST distribution like that commonly observed in the eastern Pacific. The authors argued that this would both strengthen the ITCZ precipitation, and shift it equatorward of the location where it would otherwise be.

Pauluis (2004) derived another idea from an analysis of axisymmetric GCM simulations. This idea applies to situations in which the cross-equatorial SST gradients are small, as opposed to large in the case of Tomas and Webster (1997) and Tomas et al. (1999). In Pauluis' simulations, the SST has a single positive peak north of the equator, and is uniform elsewhere, with the SST gradient on the equator being close to zero. In this case, if the PBL is also sufficiently shallow (remember that the pressure gradient induced by the Lindzen-Nigam method increases with PBL depth, for fixed SST gradient) the pressure gradient force is unable to balance friction for any significant meridional velocity on the equator. Further poleward, on both sides of the equator, southerly flow (towards the SST maximum) does occur, with the drag being balanced by the Coriolis force on the zonal flow. Since the flow in the southern hemisphere is towards the equator but then stops at the equator, convergence occurs at or just south of the equator; the flow must ascend and cross the equator above the PBL, where there is little friction and so less pressure gradient is required. This implies a secondary maximum in precipitation (the primary maximum being over the SST maximum), and Pauluis argued that this could be a mechanism for the formation of a double ITCZ.

While these various views differ in the PBL momentum balances they invoke in order to generate convergence, they all wind up generating convergence one way or another through the momentum balance, and then assume that this convergence produces convection. In Lindzen and Nigam (1987), nothing about convection need be known in order to find the PBL convergence, so that the theory truly predicts precipitation. In the others, to one degree or another knowledge of the occurrence and intensity of deep convection must be assumed to find the PBL convergence, but the ability to produce PBL convergence, given the constraint imposed by the PBL momentum budget, is nonetheless considered the limiting factor controlling the convection. The overall direction of causality is thus the same in each case, as far as the present argument is concerned.

#### *4.3.1.2 Thermodynamic control*

There are at least two entirely distinct sorts of ideas by which thermodynamic factors can be hypothesized to control convection.

##### STABILITY

The first set of ideas relies on parcel stability considerations of one form or another. In one form of this view, precipitation will occur where positive convective available potential energy (CAPE) exists, or would exist if precipitation were not occurring (since the occurrence of deep convection will tend to squelch CAPE). To evaluate the properties of the hypothetical nonprecipitating state, we can (for example) use a well-understood one-dimensional model of the nonprecipitating trade wind boundary layer (e.g., Betts and Ridgway 1989). The requirement of positive CAPE can be replaced by one that CAPE be above some finite threshold. We can also stipulate further that the rain rate will be some monotonic function of the CAPE in the nominal nonprecipitating state.

CAPE increases as the moist static energy (or moist entropy) of the near-surface air increases, and as the free-tropospheric temperature decreases. The warmer and moister the air would be in the nominal nonprecipitating state, *or* the colder the free-tropospheric sounding, the greater the CAPE, and thus the greater the rain rate we expect to actually occur. Free-tropospheric temperature is constrained to be close to horizontally uniform in the deep tropics, by dynamical adjustment under strong (dry) stratification and small Coriolis parameter. We assume it is uniform enough that we can neglect its variations relative to those of the PBL moist static energy in computing CAPE [the “weak temperature gradient” (WTG) approximation; e.g., Sobel and Bretherton (2000); Sobel et al. (2001)]. The vertical stratification can be assumed to be moist adiabatic (see chapter 3 of this volume). The rain rate is thus a function only of the PBL moist static energy in the hypothetical nonprecipitating equilibrium. We further assume that the PBL moist static energy in this state is a function only of the SST. This is fairly reasonable assumption for qualitative purposes. Thus, the rain rate depends only on the SST in this model.

Since deep convection consumes CAPE rapidly, it may make more sense to relate precipitation to the rate of CAPE production (as opposed to CAPE itself), which suggests a focus on surface fluxes. This in turn suggests that surface wind speed as well as SST should be an important factor, as is true in hurricanes, and as has been argued for intraseasonal variability (Emanuel 1987; Neelin et al. 1987). Here we must be careful not to generalize from the uncoupled to the coupled problem, since high wind speed will also reduce SST on seasonal time scales, and surface fluxes are limited on these time scales by surface radiation and ocean heat transport regardless of wind speed (e.g., Waliser and Graham 1993; Sobel 2003; Seager et al. 2003).

We can make this argument more sophisticated in a number of additional ways. One is to include some dependence on free-tropospheric relative humidity, which appears in observations to exert some control over deep convection (e.g., Sherwood 1999; Parsons et al. 2000), presumably through its effect on entrainment and downdrafts. Free-tropospheric moisture does not enter standard — non-entraining, pseudoadiabatic — definitions of CAPE (e.g., Emanuel 1994; Curry and Webster 1999)[2], and so is considered a separate influence according to standard categorizations. Another is to relate precipitation to other indices of parcel stability, instead of or in addition to CAPE, such as convective inhibition (CIN), as argued by Mapes (2000) in the context of transient variability, or other measures of disequilibrium between PBL and free-tropospheric parcels, as in "boundary layer quasi-equilibrium" (Raymond 1995; Raymond et al. 2003). All these measures can generally be expected to vary in the same way with SST; high SST regions tend in general to have high tropospheric moisture, low CIN, etc. Because of this, and because none of these ideas requires any information about the PBL momentum budget, for our purpose here it is not important to distinguish between them.

These thermodynamic ideas are manifest in a number of models for the steady circulation which use so-called quasi-equilibrium convective closures (e.g., Seager 1991; Kleeman 1991). The control of precipitation by SST according to these ideas is perhaps most transparently seen in Sobel and Bretherton (2000) who use single column models (with essentially CAPE-based convective closures) to predict precipitation, with the free-tropospheric temperature profile held fixed (WTG) and SST the only input parameter which is allowed to vary from one point to the next.

### Conserved variable budgets

An entirely distinct view, originating with Neelin and Held (1987) invokes the moist static energy budget, and ignores CAPE or any other measure of parcel buoyancy entirely. I give an exposition of this view here, drawing directly on the treatment by Neelin (1997). We stick with moist static energy although the same arguments can be phrased in terms of moist entropy or equivalent potential temperature with no important changes. In pressure coordinates, the equation for the moist static energy, $h = c_pT + L_vq + gz$, with $c_p$ the heat capacity at constant pressure, $T$ temperature, $L_v$ latent heat of vaporization, $q$ specfic humidity, $g$ gravitational acceleration, and $z$ geometric height, is

$$\partial_t h + \mathbf{u} \cdot \nabla h + \omega \partial_p h = -R \tag{4.4}$$

where $-R$ is the radiative cooling ($R$ assumed positive), and we have not yet performed any averaging in time or space. Now, taking a time average over some

[2] Free-tropospheric moisture does enter the non-entraining CAPE through its influence on the virtual temperature of the free troposphere, but that effect is both small in magnitude and of opposite sign to the effect of the same moisture on the likelihood and intensity of deep convection via entrainment and dowdraft effects.

interval long enough that the average tendency becomes negligible, and then vertically integrating from the surface ($p = p_s$) to a nominal tropopause ($p = p_t$) at which the vertical velocity $\omega$ and all turbulent fluxes are assumed to vanish, leads to

$$< \overline{\mathbf{u} \cdot \nabla h} > + < \overline{\omega \partial_p h} > = F_{net} \tag{4.5}$$

where

$$F_{net} = \overline{E} + \overline{H} - < \overline{R} > \tag{4.6}$$

is the total moist static energy input to the atmospheric column, $E$ is the surface latent heat flux and $H$ is the surface sensible heat flux, the overbar represents the time average and the angle brackets represent a vertical average:

$$< X > = (p_s - p_t)^{-1} \int_{p_t}^{p_s} X dp.$$

Now let us neglect horizontal advection. This is a major assumption, not necessarily justified. Horizontal temperature advection can be generally assumed small in the deep tropics, but horizontal moisture advection cannot, and this implies that significant horizontal advection of moist static energy can occur. Nonetheless, we make this assumption here for the sake of argument, and return to it later. We then also neglect transients, assuming

$$< \overline{\omega \partial_p h} > \approx < \overline{\omega} \partial_p \overline{h} > .$$

This may not necessarily be justified either, and needs to be reconsidered later together with the neglect of horizontal advection. We now make one additional key assumption. We assume $\overline{\omega}$ to have separable horizontal and vertical dependence:

$$\overline{\omega}(x, y, p) = \Omega(p) \hat{\omega}(x, y), \tag{4.7}$$

with the dimensionless function $\Omega(p)$ known. We assume that $\Omega$ is of order unity and positive; assuming the vertical velocity to have a deep, single-signed vertical structure, as normally associated with the first baroclinic mode, $\hat{\omega}$ can be thought of as the value of $\overline{\omega}$ at a midtropospheric level, say, 500 $hPa$. The theory can be generalized to allow $\Omega(p)$ to be a function of horizontal position, as long as it is a slowly varying function compared to $\hat{\omega}$. Then, we can take $\hat{\omega}$ out of the integral to obtain

$$-\hat{\omega} M = -\hat{\omega} < -\Omega \partial_p \overline{h} > = F_{net}. \tag{4.8}$$

We now have a predictive theory for the horizontal structure of the vertical velocity, if we know three quantities: the surface fluxes (of which the latent heat flux is by far the dominant contributor over the tropical oceans; sensible heat flux can be neglected there to a good approximation), the radiative cooling, and the gross moist stability, $M$:

$$M \equiv - < \Omega \partial_p \overline{h} > . \tag{4.9}$$

$M$ is not directly related to any measure of local stability at a point (e.g., the buoyancy frequency), nor to any measure of column stability derived from buoyancy considerations, such as CAPE. [Note that here $M$ has the units of $\partial_p h$, that is,

$J\ kg^{-1}\ hPa^{-1}$. This is different than the convention used, for example, by Neelin and Zeng (2000), though the difference is only multiplication by a dimensional constant.]

To illustrate the dependence of $M$ and thus $\hat{\omega}$ on the vertical structure $\Omega(p)$, consider a simple example, illustrated by fig. 4.3. The figure shows a typical tropical moist static energy profile from a deep convective region [in this case a mean over slightly less than two months at Kwajalein, Marshall Islands, during the KWAJEX field experiment (Sobel et al. 2004; Yuter et al. 2005)]. Imagine a two-dimensional control volume with horizontal flow into it localized as a delta function in the vertical at a pressure $p_{in}$, and a similarly localized outflow at $p_{out}$. The horizontal divergence is thus

$$\nabla \cdot \mathbf{u} = C[\delta(p - p_{out}) - \delta(p - p_{in})],$$

where $C$ is a constant with dimensions of $hPa\ s^{-1}$. The vertical velocity is thus constant, call it $\omega_0$, for $p_{out} < p < p_{in}$, and zero elsewhere, and the vertical structure function ($\Omega_0(p)$, say) is reasonably chosen to be unity for $p_{out} < p < p_{in}$, and zero elsewhere. Using (4.9), the gross moist stability is

$$M = \frac{h_{out} - h_{in}}{\Delta p}, \tag{4.10}$$

the moist static energy difference between the outflow and inflow levels, divided by the pressure depth of the troposphere, $\Delta p$. We will have $M > 0$ as long as $p_{out}$ is low enough (the altitude of outflow high enough) that $h_{out} > h_{in}$. From the figure it is evident that we can change the magnitude and even the sign of $M$ quite easily by making only modest changes in $p_{in}$ or $p_{out}$. For fixed $p_{in}$ below the $h$ minimum (around 600 hPa in this profile), and $p_{out}$ above the minimum, $M$ increases as $p_{out}$ decreases. In reality, of course, horizontal divergence is continuous in the vertical, and the integral in (4.8) is not so transparently evaluated, but it remains generally true that deeper ascent generally leads to larger $M$, and shallower ascent to smaller or even negative $M$.

Knowing $\hat{\omega}$, we can find the precipitation, by applying the same steps and approximations which led to (4.5) to the equation for dry static energy, $s = c_pT + gz$, yielding

$$< \overline{\omega \partial_p s} >= -\hat{\omega} M_s = \overline{P} - < \overline{R} >, \tag{4.11}$$

where we have defined the gross dry stability,

$$M_s = - < \Omega \partial_p \overline{s} >,$$

and $P$ is the precipitation expressed in energy units. Eliminating $\hat{\omega}$ between (4.8) and (4.11) yields

$$\overline{P} = \frac{M_s}{M} F_{net} + < \overline{R} > . \tag{4.12}$$

The second term on the right-hand side of (4.12) accounts for the precipitation which would occur in radiative-convective equilibrium, while the first represents variations associated with the large-scale circulation. Keep in mind that while surface fluxes contribute importantly to $F_{net}$, this first term does *not* represent rainfall balanced directly by surface evaporation [that is accounted for by the second term, though it isn't immediately obvious, because (4.12) is most directly a heat budget rather than a moisture budget], but rather that component of rainfall which is balanced by large-scale moisture convergence. Through the decomposition of the energy transport into a mass transport and a gross moist stability, the net vertical energy input into the atmosphere, $F_{net}$, which includes surface fluxes, appears as a forcing which drives the circulation which accomplishes that convergence.

It is worth pausing for a moment to examine the logical structure of this argument, because the result appears somewhat miraculous: we have obtained a theory for the time-mean precipitation without either using any convective parameterization or considering the horizontal structure of any field. Up to (4.7), we had done nothing but construct a budget, do some averaging, and neglect certain terms. While the neglect of these terms is debatable, we can at least find some situations in which it is valid (for example, at local maxima and minima in $h$, horizontal advection must vanish). Even the assumption of separability and known vertical structure, (4.7), does not at first glance seem all that restrictive; consider that a model with fixed vertical structure can still have a broad range of convective parameterizations (e.g., Wang and Li 1993).

There are several problems, however. The first, as mentioned above, is the neglect of horizontal advection. Analysis of reanalysis data sets (L. Back and C. Bretherton, pers. comm.) yields the result that in many convective zones, horizontal advection is competitive with or even dominates vertical advection in the vertical average, making neglect of the former a poor approximation. Even if we ignore this, there are more subtle difficulties related to our assumptions of what is known and what must be predicted. (4.12) predicts $\overline{P}$ if we know $F_{net}$ and $M_s/M$. While the surface fluxes can ultimately not be taken as an external forcing, being part of the solution themselves, it is nonetheless reasonable to consider the problem with fixed-flux boundary conditions as a first step. Since departures from radiative-convective equilibrium are generally large, so the spatial structure of precipitation is accounted for more by variations in moisture convergence than those in surface evaporation, the latter of which are considerably smaller and do not even have the same spatial structure as those in precipitation (see fig. 4.4, and compare to precipitation shown in fig. 4.1) thus taking the surface fluxes as given does not make the problem trivial. Obtaining $< \overline{R} >$ is a job for a radiative transfer model, which can be considered more or less independent of the rest of the problem. Radiative cooling depends strongly on cloudiness and humidity, but these are closely related to precipitation, so a simple parameterization by which $< \overline{R} >$ is made a function of $\overline{P}$, linear in the simplest case:

$$< \overline{R} >= R_0 - r\overline{P},$$

with $r$ a positive dimensionless constant (expressing $\overline{P}$ and $< \overline{R} >$ in the same units, say $W\ m^{-2}$), apparently in the range $0.1 - 0.2$ (e.g., Su and Neelin 2002, Bretherton and Sobel 2002, Lin et al. 2004, Perez et al. 2005), captures the essence of the physics without adding any fundamental complexity to the problem.

The quantity $M_s/M$ is the main difficulty. It is not at all clear that this quantity can be considered known or fixed. Neelin and Held (1987) hypothesized that spatial variations in $M$ exerted a primary control on spatial variations in $\hat{\omega}$ (and thus implicitly $\overline{P}$), but, consistent with the assumption of known vertical structure (in their case, a two-layer model), took those $M$ variations to be determined by variations in $\partial h/\partial p$. However, $M$ is very sensitive to changes in $\Omega$, because $\partial h/\partial p$ changes sign in the troposphere while $\omega$ often does not (or at least does not do in a way that resembles $\partial h/\partial p$), so there is generally a lot of cancellation in the integral in (4.9). In the toy case above, for example, we can plausibly choose $p_{in}$ and $p_{out}$ such that $h_{in} = h_{out}$, so $M = 0$; then increasing or decreasing $p_{out}$ by a tiny amount leads to $M$ of opposite sign. Since $\partial s/\partial p$ does not change sign in the troposphere, $M_s$ is much less sensitive, so the ratio $M_s/M$ retains the sensitivity.

Yu et al. (1998) computed $M$ using $\partial_p h$ computed from a large-scale meteorological analysis and from tropical soundings. They did not use meteorological analyses directly to estimate $\Omega$, but took the latter to have the structure associated with a first baroclinic mode, an approximately half-sine type structure between $p_s$ and $p_t$, but implemented a similarity theory, allowing $p_t$ to vary depending on the depth of convection diagnosed from CAPE. Using this together with observed $\partial h/\partial p$ from observations, they found that variations in the depth of convection tended to be comparable, but of opposite sign to those due to variations in $\partial h/\partial p$, so that $M$ tended to remain roughly constant. However, to the extent that vertical profiles of vertical velocity obtained from assimilation data sets are any guide, the assumption of a first baroclinic mode, even a stretchable one with varying $p_t$, is too restrictive for the purpose of this calculation. When typical variations in both $h$ and $\omega$ from such data sets are considered, one comes to the conclusion that variations in $\omega$ generally dominate variations in the gross moist stability.

In a model with parameterized (or resolved) convection, the vertical structure of the vertical velocity will be strongly influenced by the convection, since in the deep tropics we expect WTG balance to hold,

$$\omega \frac{\partial s}{\partial p} \approx Q, \tag{4.13}$$

where $Q$ is the total heating, and the dry stability $\partial s/\partial p$ varies little. Thus the sensitivity of $M$ to the vertical structure of $\Omega$ hides a dependence of this theory on convective parameterization, which determines the vertical structure as well as the magnitude of $Q$.

Finally, perhaps the most important weakness of the moist static energy budget argument is that it does not by itself predict the occurrence of significant deep convection or large-scale ascent (the two being inseparable on time scales of a day or greater), only their intensities at those locations where they are already known, by other means, to be occurring. This is clear from the study of Yu et al. (1998), for example, who only plot $M$ in regions of significant precipitation.

Looking at (4.8), and assuming $M$ to be positive, it may be tempting to think that we can associate mean ascent with positive net mean forcing on the column moist static energy budget, $F_{net} > 0$, corresponding in steady state to net export of moist static energy from the column. This is not correct, because many tropical and subtropical regions of mean descent also export moist static energy. This is apparent from the analysis of Trenberth et al. (2001), and is also illustrated here in fig. 4.5. The figure shows a climatology for September of precipitation and $F_{net}$ from the second NCEP Reanalysis (one may be legitimately concerned about estimating $F_{net}$ from assimilation data sets in which the energy fluxes are entirely model products; all we can say is that we have made similar figures for several such data sets as well as general circulation models and the features of interest here are remarkably consistent across all of these). While there is arguably some correspondence between $F_{net}$ and precipitation, particularly over land, we see clearly that $F_{net}$ over the southern tropical Atlantic is as large as in the ITCZ to the north, but precipitation in the south is less than $2\ mm\ d^{-1}$. The associated convective heating is well below the radiative cooling in this region, so this is a region of descent. The positive $F_{net}$ implies moist static energy export nonetheless. Some of this export may be due to transients, but it is entirely possible that a large fraction of it is explainable within the confines of the argument above, that is, by a local argument in which steady vertical advection of moist static energy is the key factor.

In descent regions, the free troposphere tends to be dry, so the minimum in the moist static energy profile tends to be relatively pronounced. The planetary boundary layer, on the other hand, still has high relative humidity, and particularly if the SST is not too low, may still have fairly large moist static energy. All we need in order to have net moist static energy export via descent, according to the argument above, is an $\Omega$ profile which is not too top-heavy. Fig. 4.7 illustrates this, using the same sounding as in fig. 4.3 but with choices of $p_{in}$ and $p_{out}$ such that $M < 0$, though we should not really call this “gross moist instability” as we would if we had $M < 0$ in an ascent region, because in this case we are still exporting energy, just doing it through descent rather than ascent.

To the extent that the time-mean vertical velocity profile in descent regions can in fact configure itself in such a way as to accomplish moist static energy export, $F_{net}$ does not determine the sign of the large-scale vertical velocity even in a steady, horizontally uniform atmosphere. What does? Presumably we must fall back on arguments having to do with CAPE, or some other similar measure of convective stability (and thus ultimately relatable to the SST) or to PBL momentum considerations. While such considerations may not contradict the moist static energy budget arguments, they are outside the scope of the latter, and the need for these extra considerations demonstrates the incompleteness of the moist static energy budget arguments.

### 4.3.2 Precipitation - evidence

As described above, we have two classes of theories for the precipitation. The mechanisms involved in the two classes are almost entirely unrelated. One focuses on local thermodynamics, the other on boundary layer momentum dynamics. Each class contains multiple specific theories which are at least somewhat distinct. In the case of thermodynamic control in particular, the ideas based on stability and on conserved variable budgets are quite different. There are many possible ways one could go about testing these ideas. Many of them would be particular to specific theories rather than to the whole class to which the theory belongs. I am most interested in distinguishing between the two classes of ideas.

In this light, we first consider recent observational evidence from the East Pacific Investigation of Climate (EPIC) field experiment (Raymond et al. 2004) which contradicts the notion that PBL convergence necessarily implies deep convection. This notion is essential to all PBL momentum control ideas in their strongest forms. Weaker forms, in which PBL momentum control modulates deep convection to some degree, changing the distribution of precipitation quantitatively from what it would otherwise be based on thermodynamics alone, are still tenable in the face of this evidence.

The EPIC studies mentioned above in the context of the surface wind (Zhang et al. 2004; McGauley et al. 2004; Raymond et al. 2004) find that when the SST-induced pressure gradient is strong, and produces convergence, there need not be deep ascent nor rain associated with that convergence. Rather, there can be shallow ascent and divergence just above the boundary layer, with a shallow return flow there rather than a deep Hadley circulation. This is just what we should expect if convection were thermodynamically controlled. If the thermodynamic conditions are not right for deep convection, but the SST gradient drives low-level convergence nonetheless, the only way to satisfy mass conservation (without the deep ascent that would necessarily come with deep convection) is to have a shallow circulation of this type. The observations confirm this theoretical expectation. Presumably this situation also occurs in the second, southern ITCZ, during the large fraction of the year when it exists in surface convergence but not precipitation (Liu and Xie 2002). Again, this evidence from EPIC does not prove that PBL convergence has no influence on deep convection whatsoever, only that it is not sufficient to induce deep convection if the thermodynamic conditions are not right. It is still possible that under some thermodynamic conditions, dynamically-induced PBL convergence can either induce deep convection, or increase its intensity.

More broadly, we would like to determine whether PBL momentum control or thermodynamic control ideas are more correct by using each one to produce a simulation of the precipitation, and comparing to observations. This turns out not to give an unambiguous result. Simulations using simple models based on both theories, as well as combinations of the two (e.g. Wang and Li 1993) have been done, and all reproduce the observations at comparable levels of accuracy — with significant errors, as one expects from simple models, but with some fidelity to the main gross features of the climatological precipitation distribution (or in some studies, the deviations from the time or zonal mean).

A very simple model problem gives some idea of why the two sets of ideas do not lead to dramatically different predictions. Consider an ocean slab-symmetric in one horizontal direction (call it $y$, so the SST varies only in $x$) with no planetary rotation. In that case (4.1) becomes

$$p_x + \epsilon_u u = 0,$$

which, solving for $u$ and taking minus the partial derivative with respect to $x$ to find the convergence gives

$$-u_x = \frac{p_{xx}}{\epsilon_u}.$$

Assuming rainfall to be proportional to convergence, and $p$ to be proportional to minus the SST, both being the case in the Lindzen-Nigam model, we find that the rainfall is proportional to minus the curvature of the SST,

$$P \sim -\frac{\partial^2}{\partial x^2}(SST). \tag{4.14}$$

The extended Gill model described above, on the other hand, suggests that the rainfall will be related to the local value of the SST, relative to some background value, of which the tropical mean is presumably approximately representative. If the SST is high compared to the tropical mean, we expect significant precipitation, but not otherwise. For small perturbations, we may assume the relation to be linear above a threshold,

$$P \sim \mathcal{H}(SST - SST_{min})(SST - SST_{min}), \tag{4.15}$$

where $\mathcal{H}$ is the Heaviside function and $SST_{min}$, the minimum SST at which convection can occur, is a function of the mean tropical climate, for example presumably related to the mean SST over the tropics (e.g., Sobel et al. 2002).

Comparing (4.14) and (4.15), we see that even at the very crude level of argument used to obtain these relations, the fundamental difference between the two models is clear, with one involving the local value of SST and the other involving the spatial structure. Nonetheless, it is also apparent why it is not so simple to use observations to determine which is correct. If we assume that spatial and temporal variations in SST have characteristic magnitude and spatial scale that are known, then the two models will not disagree greatly since both predict rainfall to maximize at SST maxima. The actual predicted SST-rainfall relationships will, of course, depend quantitatively on the parameters in the models, which have been ignored in the arguments here, but if at all possible it is desirable to resolve theoretical arguments without having to debate too closely what these parameters should be.

At the same time, this example does suggest one situation which may be used to distinguish between the two views. That is a local maximum in SST, even a very sharp one, which is not a global maximum. In that case, the SST curvature can still be as large, so the PBL convergence and thus rainfall according to a Lindzen-Nigam type view will be large, while the thermodynamic control view will predict less or zero precipitation, depending on just how large or small the local SST is compared to values elsewhere in the tropics.

The southern ITCZ in the eastern Pacific ocean, which has significant precipitation only in northern spring in reality, but year-round in many GCMs, may be viewed as just such a case. Although it does not contain a true local maximum in SST, it is a local maximum in the meridional direction, in which gradients are sharpest. This is enough to produce precipitation by the LN mechanism. At the same time, during most of the year the local SST is lower than that in the northeast Pacific, or the western Pacific or much of the rest of the tropical oceans, so by thermodynamic control arguments we expect little precipitation. As an example, see fig. 4.6

The same arguments, to a lesser degree, may also apply to the northeast Pacific ITCZ. The SST there is high enough to induce convection by thermodynamic control arguments (assuming the absence of very strong dry air advection or some other anomalous suppressing effect) but, most of the time, not quite as high as in the western Pacific warm pool. The meridional SST gradients are as sharp as anywhere in the tropics however, so we might expect both models to predict precipitation, but boundary layer momentum control arguments, all else equal (in some admittedly imprecise sense), to predict greater precipitation.

Simulations with simple models appear to bear this out. Models based on Gill formulations, with no explicit boundary layer, tend not to have a spurious southern ITCZ. At the same time, these models tend to underestimate the precipitation in the northern ITCZ. This difficulty with a Gill model was discussed by Seager (1991). Fig. 4.8 shows results from the Quasi-equilibrium tropical circulation model (Neelin and Zeng 2000; Zeng et al. 2000; chapter 6, this volume). This model is more complex in several respects than other models discussed in this chapter, but it lacks a boundary layer (its vertical structure consists of two modes, barotropic and first baroclinic). The figure shows essentially zero precipitation in the northeast Pacific ITCZ in January, where the Xie-Arkin data set shows a fairly strong, narrow ITCZ in that season, as shown in the top panel of Fig. 4.1.

Models with a boundary layer can do much better in this regard, though sensitivity to the convective parameterization is large. Fig. 4.9, from Wang and Li (1993), shows results from their model, which contains a boundary layer as well as a first baroclinic mode. The first scheme, called "linear heating" is a moisture convergence-based scheme, similar to those of Kuo (1974), and to those used in simple models by Webster (1972), Zebiak (1986), etc., but is truly linear in that negative precipitation is allowed. The second is essentially the same scheme, except that negative precipitation is not allowed to occur. The resulting precipitation field is not exactly the same as would be obtained from setting all negatives in the first panel to zero, because the heating has feedbacks with the rest of the model dynamics. The third panel uses a similar scheme, but one in which precipitation is not allowed to occur unless SST is greater than a threshold value. In the fourth panel, precipitation does not occur for SST below the threshold, is linearly proportional to SST above that threshold, as in (4.15), and then has no SST dependence above a second, higher threshold; in addition to the SST dependence the precipitation retains the moisture convergence dependence of the preceding schemes. The schemes used in the third and fourth panels, through their explicit SST dependences, bring the model closer to thermodynamic control ideas. We can see that in these two panels the southern ITCZ in the eastern Pacific is nonexistent, but the northern one is also weak or nonexistent. The two upper panels, using models closer to momentum control ideas, both have strong northern ITCZ. In the linear scheme, this is accompanied by negative precipitation south of the equator. The conditional heating scheme is perhaps the best, giving a strong northern ITCZ accompanied by a modest but clear southern ITCZ, for a double ITCZ bias comparable to what one might find in a GCM.

## 4.4 DISCUSSION

### 4.4.1 Some open questions

We are left with the impression that boundary layer momentum dynamics does play some role in producing the sharp, intense northeast Pacific ITCZ which is observed, as concluded by Wang and Li (1993). At the same time, too much emphasis on PBL momentum control, or too little thermodynamic control, can lead to a double ITCZ bias.

In this light, the double ITCZ bias in GCMs, discussed in more detail in chapter 7, seems particularly paradoxical. Apparently, the GCMs are responding too strongly to the boundary layer momentum forcing, and producing deep convection in the southern hemisphere, where instead they should produce a shallow circulation of the sort found at certain times in the north during EPIC (McGauley et al. 2004; Raymond et al. 2004). Why should the GCMs do this, when nearly all modern convection schemes used today are essentially CAPE adjustment schemes of one sort or another, which is to say they are constructed on thermodynamic control principles? Few if any current models actually build in an explicit tendency towards deep convection where there is low-level convergence (in the way that older models using, for example, the Kuo scheme and its relatives did). Based on the simple models discussed above, one might expect that many GCMs would have no southern ITCZ, but might rather err on the side of too weak a northern ITCZ. The former is clearly not true, and the latter does not seem to be systematically true either. As discussed in section 4.1, we cannot directly apply conclusions from the simple model context to the GCMs, but the question here suggests that it may be fruitful to focus attention precisely on the different roles of the convective parameterizations in the two sorts of models. The need to simulate transients in GCMs but not the simple models is the most obvious difference, and indeed some recent work suggests that transients may be playing a large role in the double ITCZ bias in at least one GCM (Bacmeister et al. 2006).

This leads to another, closely related, and more fundamental question: how does PBL convergence induced by momentum forcing actually influence convection? Simply stating, as some models built on these principles do, that deep convection must occur so that the mass (and moisture) can be vented from the boundary layer is not satisfactory, because the mass and moisture can also be vented by a shallow circulation with little or no precipitation. Additionally, at least the most basic tenet of thermodynamic control thinking must be satisfied: the atmosphere must be in some appropriate sense unstable in order for deep convection to occur. Our understanding should include a grasp of how the PBL convergence influences the stability of the atmosphere so as to make deep convection more probable.

Several mechanisms exist by which PBL convergence can destabilize the atmosphere:

- An influence on CAPE. If the ascent induced by the PBL convergence reaches a significant height, it can have a significant influence on CAPE, because the ascent will adiabatically cool the troposphere. However, a shallow circulation will have little influence on CAPE, as most CAPE resides in the difference between the moist static energy of a near-surface parcel and the saturation moist static energy of the environment at relatively high levels (McBride and Frank 1999).

- An influence on convective inhibition (CIN). This is more likely in the case of a shallow circulation, as only ascent at the top of the PBL is needed to induce cooling there, reducing the negative buoyancy of near-surface parcels which have ascended to that level and making it easier for them to break through to their levels of free convection.

- An influence on free-tropospheric moisture. Shallow ascent will advectively moisten the lower free troposphere, reducing the negative contribution to buoyancy which entrainment imparts to updrafts, and reducing the tendency of downdrafts to stabilize the sounding.

At least the latter two of these mechanisms are quite plausible, but it would be very valuable, not least from the point of view of convective parameterization and model development, to determine quantitatively which is more important, or whether some other factor, not named above, is responsible for the enhancement or strengthening of deep convection in the ITCZ by PBL momentum-driven convergence.

### 4.4.2 Moist static energy argument

Despite having offered a critique earlier of moist static energy arguments, we can offer a hypothesis here in terms of such arguments. We may be motivated to do this in order to provide an alternative perspective on the questions above, or perhaps just because analyzing a budget of the most conserved variable in the system is such a fundamental principle in physics that we should not abandon it easily. Some aspects of this hypothesis are testable.

This hypothesis is relevant to regions where the SST is low enough that the persistent absence of deep convection is possible, but not so low that deep convection is nearly impossible, as is the case in the eastern Pacific cold tongue for example. In such regions, in the absence of PBL momentum-driven convergence, deep convection will be weak or absent. In its absence, the troposphere will cool radiatively and this will induce subsidence. Assuming the subsidence profile is not too top-heavy, it is possible for the steady divergent circulation to export moist static energy. We assume that in our regions of interest the total column moist static energy forcing (surface fluxes minus radiative cooling) is positive, and to make the argument simple, we assume that it takes a constant value, whether deep convection is occurring or not. Our descending circulation configures itself so as to export moist static energy at the rate necessary to balance the forcing.

Up to this point our argument has been thermodynamic, considering only the local SST value, relative to some large-scale tropical mean, except that by assuming the absence of momentum-driven convergence, we implicitly assumed that the horizontal structure of the SST field was not such as to generate such convergence. Now consider the case in which such convergence does occur, via Lindzen-Nigam dynamics, for example because our location is a local SST maximum (though not a global one). The environment being stable to deep convection, this convergence will generate a shallow circulation, with ascent only near PBL top, and a divergent return flow shortly above, with the vertical velocity becoming downward again well below (say) the freezing level. Taken on its own, this shallow circulation has negative gross moist stability, because $\partial h/\partial p$ is positive ($h$ decreasing with height) over most or all of the shallow region of ascent (even if the ascent extends somewhat past the moist static energy minimum, we can still have $M < 0$ as long as it does not go too far past that point). We can view the resulting import of moist static energy as an additional column forcing, on top of $F_{net}$, in equations (4.5) or (4.12). We can also say that the means of exporting moist static energy by descent has been cut off by the imposed near-surface circulation, since the configuration of $\omega$ and $\partial h/\partial p$ over the whole column that allowed net export is no longer dynamically possible. The result is that deep convection must occur in order to export moist static energy at the required rate. This argument is made quantitative by the model of Sobel and Neelin (2006), which couples a free tropospheric model with vertical structure similar to that of Neelin and Zeng (2000) with an explicit PBL in which Lindzen-Nigam effects (among other mechanisms) can operate.

## 4.5 CONCLUSIONS

There are two classes of theories for the quasi-steady component of tropical precipitation and surface wind, given the SST. In one, the SST determines the winds directly, and the convergence of the winds at low levels determines the location and intensity of precipitation. In the other, precipitation is determined locally by the SST and related thermodynamic factors, and the surface winds are then induced by the heating associated with the precipitation.

Available evidence suggests that deep convective heating is largely responsible for determining the surface zonal wind, but that particularly in regions of strong meridional SST gradients, those gradients to a significant extent induce the meridional wind directly, essentially as in the model of Lindzen and Nigam (1987). While the picture is less clear for precipitation, there is also some evidence to suggest that in these same regions, the narrow ITCZs, the convergence associated with these meridional winds, induced through the PBL momentum budget independently of deep convection, plays a role in modulating the precipitation, though thermodynamic factors must play a significant role as well.

We have argued that the interplay of PBL momentum dynamics and free-tropospheric thermodynamics may possibly be understood by considering the moist static energy transport by the flow deduced from the PBL momentum budget forced directly by SST. When the latter is divergent, and the SST not too high, we expect descent, and this can still be associated with positive $F_{net}$ and moist static energy export. When it is convergent, we can consider the moist static energy transport directly associated with this PBL-induced flow as another external forcing on the moist static energy budget of the deep troposphere, which will add constructively to $F_{net}$. The descending export mode is forbidden by the PBL momentum budget, so deep ascent is the only means for the necessary moist static energy export to be accomplished, and this implies precipitation.

Formulating a simple model for the quasi-steady component of the precipitation and surface wind over the tropical oceans is obviously a more limited task than formulating a GCM, which must simulate a much broader range of phenomena. Finding the best idealized model and understanding its properties would not necessarily lead immediately to improved GCM simulations of tropical climate. Nonetheless, if we do not understand, in a way that we can express through the formulation of a simple model in which cause and effect are clear, the basic factors controlling the location, spatial extent, and intensity of the tropical precipitation zones, our efforts to improve their simulation in GCMs can amount to little more than groping in the dark. Further effort with models at the simpler end of the hierarchy seems warranted at this moment.

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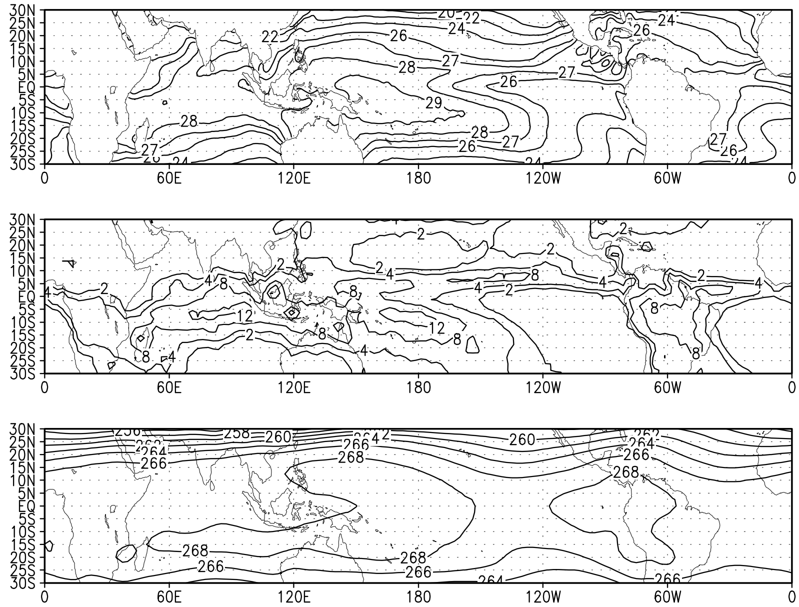


Figure 4.1 Top: climatological SST ($^{\circ}C$, contour interval $2^{\circ}C$ below $26^{\circ}C$, $1^{\circ}C$ above) from the Reynolds and Smith (1994) data set. Middle: precipitation ($mm\ d^{-1}$, contour interval $2\ mm\ d^{-1}$ below $4\ mm\ d^{-1}$, $4\ mm\ d^{-1}$ above), from the CMAP data set (Xie and Arkin 1997). Bottom: 500 hPa temperature ($^{\circ}K$, contour interval $2^{\circ}K$) for January, from the NCEP/NCAR Reanalysis (Kalnay et al. 1996). Figure adapted from Sobel (2002).

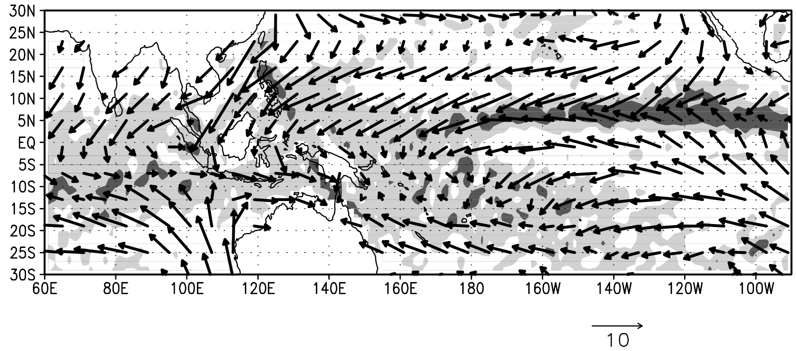


Figure 4.2 Climatological (1949-1992) surface wind ($m\ s^{-1}$) and divergence ($s^{-1}$, positive values not shown, contour interval $5 \times 10^{-6}$)for January, from the COADS data set. Adapted from Stevens et al. (2002).

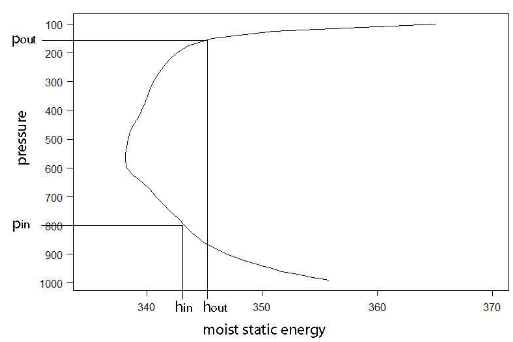


Figure 4.3 Moist static energy profi le, showing level of concentrated infbw, $p_{in}$, where moist static energy is $h_{in}$, and level of concentrated outfbw, $p_{out}$, where moist static energy is $h_{out}$, for a hypothetical region of idealized ascent, uniform for $p_{out} < p < p_{in}$, zero elsewhere (see text).

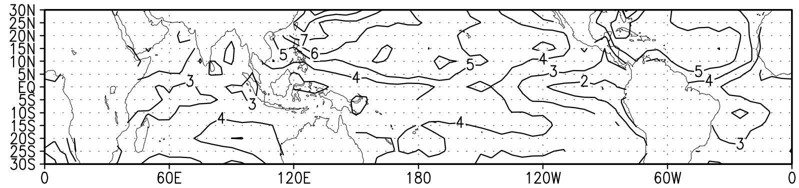


Figure 4.4 Climatological January surface evaporation ($mm\ d^{-1}$), from the COADS data set.

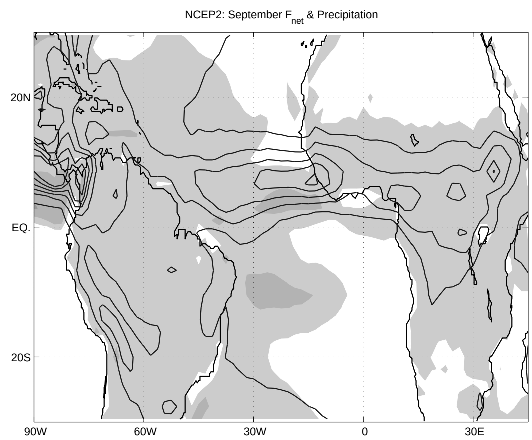


Figure 4.5 September climatology of precipitation (contours; contour interval $4\ mm\ d^{-1}$, minimum contour $2\ mm\ d^{-1}$, and total column moist static energy forcing $F_{net}$ ($W\ m^{-2}$, positive values shaded, white areas are negative), computed from the second NCEP Reanalysis.

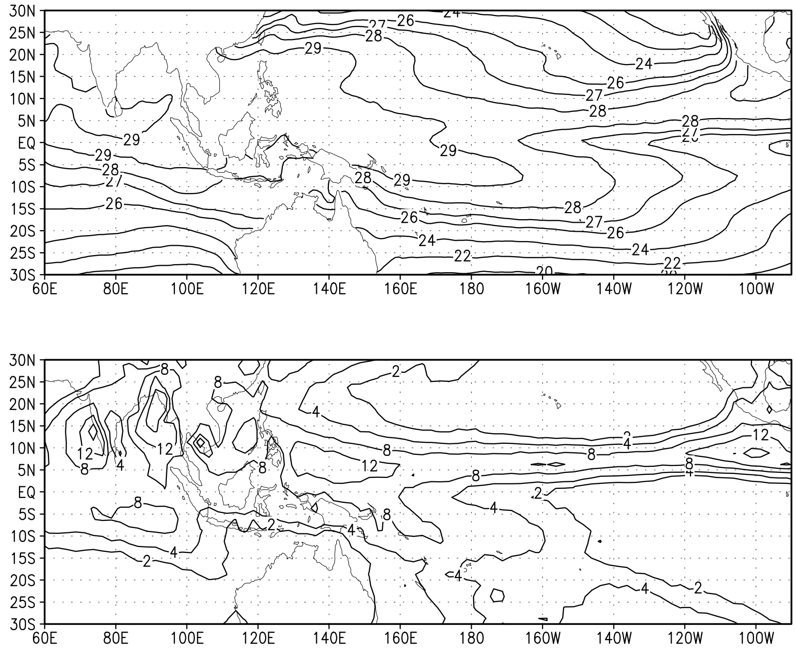


Figure 4.6 June climatologies of (top) SST and (bottom) precipitation, using the same data sets and plotting conventions as in fig. 4.1.

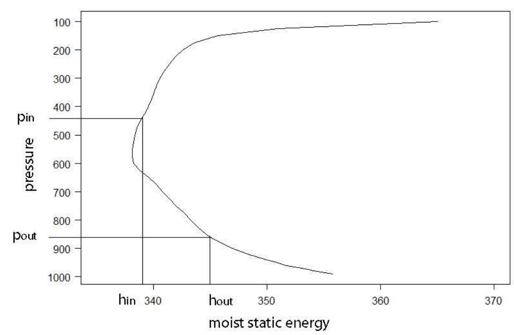


Figure 4.7 As in fi g. 4.3, but for a descent region.

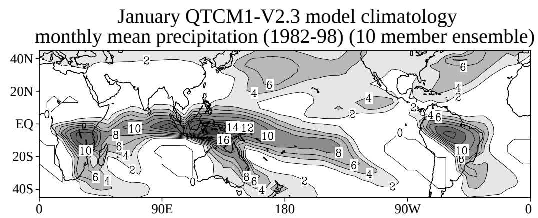


Figure 4.8 January precipitation climatology from the quasi-equilibrium tropical circulation model developed by Neelin and Zeng (2000). Figure courtesy of Joyce Meyerson and David Neelin.

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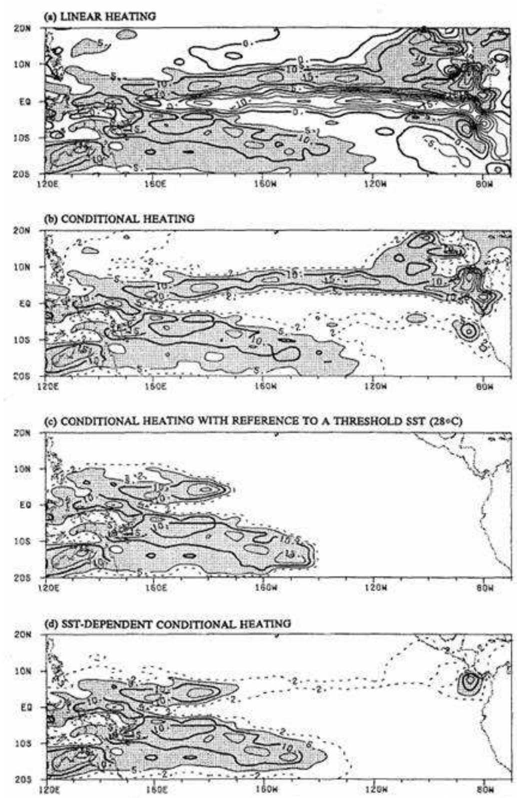


FIG. 6. January mean precipitation rate (in units of mm/day) simulated by the present model with a scheme of (a) linear heating, (b) conditional heating, (c) conditional heating with reference to a threshold SST, and (d) general SST-dependent conditional heating.

Figure 4.9 Precipitation simulations with a simple model by Wang and Li (1993).