The Tropical Cyclone-Induced Flux of Carbon between the Ocean and the Atmosphere

by

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Submitted to the Department of Earth, Atmospheric and Planetary Sciences
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Abstract

Tropical cyclones are known to cause phytoplankton blooms in regions of the ocean that would otherwise support very little life; it is also known that these storms entrain carbon-rich deep water, which can cause locally-significant air-sea fluxes. However, the relative magnitude of these two processes has mostly not been established, and questions about their global impact on the carbon cycle remain. A high-resolution model is developed, using established techniques and tabulated and published inputs, which tracks the physical, chemical, and biological evolution of the ocean’s mixed layer in response to atmospheric forcing. Its ability to recreate the observed ocean state is tested. This model is used to simulate a real region of ocean, both with and without the mixing induced by a tropical cyclone, in order to find the change in biological activity and carbon content, and to track the evolution of this anomaly through the end of the winter. After carefully examining a few specific cases that have been discussed in previous literature, one calendar year’s worth of storms are modeled, and their net effect is summarized. It is shown that many storms do enhance biological productivity, but only in a few rare cases does the amount of carbon sunk by phytoplankton decay exceed the amount mixed upward by the entrainment of cold, carbon-rich water. The sign of the storm-induced carbon flux is thus shown to be upward for nearly all storms. However, even this effect is small: the net efflux of carbon from the deep ocean to the mixed layer and the atmosphere in the year 2006 is found to be at most on the order of a few tens of teragrams. This is consistent with other studies.

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TMI data are produced by Remote Sensing Systems and sponsored by the NASA Earth Science MEaSUREs DISCOVER Project. Data are available at www.remss.com.

I have made extensive use of data from the Bermuda Atlantic Time Series, made available to the public by the Bermuda Institute of Ocean Sciences (bats.bios.edu).

I have used ECCO state estimates, available to the public at www.ecco-group.org.

Finally, I have used the Unisys Best Track database; it is headquartered at http://weather.unisys.com/hurricane/index.html, but I have in fact used the edition hosted by Kerry Emanuel:


This work would have been impossible without the advice and support of K. Emanuel; I am additionally indebted to M. Jansen, S. Clayton, A. Wing, and especially M. Follows for several insightful conversations regarding the nature of the problem at hand and the structure of a minimal solution. I would finally like to thank numerous friends and family for their continued moral support, and the entirety of PAOC for providing a curiosity-driven environment.
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Chapter 1

Introduction

The surface waters of the subtropical gyres are extremely poor in nutrients, with just enough phytoplankton to consume whatever is mixed in. Below the euphotic zone, where it is too dark for photosynthesis, nutrients are plentiful, but through most of the year, there is very little to mix these nutrients up to the mixed layer where they could support life. Tropical cyclones (“TCs”) are an exception to this rule; the strong winds that they bring deepen the mixed layer substantially in a very short time. If the storm mixes deep enough, it draws nutrients to the surface and stimulates a brief but sometimes significant phytoplankton bloom; these effects can be seen from space by color-sensing satellites (Babin et al., 2004). These phytoplankton consume both the nutrients that have been mixed up and some of the dissolved inorganic carbon (DIC) in the mixed layer; they grow quite quickly and have a lifespan of weeks. When they die, they decompose into dissolved and particulate remains, and the particulates sink out of the mixed layer. Clearly, if a sufficiently large amount of nutrients is entrained into the mixed layer, the phytoplankton life cycle could cause a meaningful amount of carbon to sink downward. Because air-sea fluxes work to bring the mixed layer DIC concentration into equilibrium with the atmospheric concentration of carbon dioxide,

\[1\text{Though TCs can also mix up chlorophyll left over from earlier biota, so not all the increase in chlorophyll concentration seen in their wake can be attributed to an increase in primary production (Gierach and Subrahmanyam, 2008). Hanshaw et al. (2008) reason that increased chlorophyll-a concentrations are “caused primarily by the upward mixing of chl-a”, as opposed to enhanced primary productivity due to nutrient entrainment.}\]
and because the deep ocean is not in direct communication with the atmosphere, storm-induced phytoplankton blooms have the potential to mediate a downward flux of carbon from the atmosphere to the deep ocean.

There is an invisible effect that works in the opposite direction: when cold water is mixed up from the deep and heated, the solubility of carbon dioxide (and all other gases) decreases, causing a tendency toward over-saturation and thus an enhanced efflux of CO₂; the waters of the mixed layer can therefore serve as a holding area in the storm-induced flux of carbon from the deep ocean to the atmosphere. Bates et al. (1998) examined the carbonate chemistry of three hurricanes that passed through the Sargasso Sea in 1995 and concluded that the CO₂ efflux induced by them was 55% higher than it would have been in their absence, and Bates (2007) surveyed two dozen storms that passed near Bermuda in a two-decade period and found that many of them caused over 10% of the efflux of CO₂ seen in the region in their respective seasons. However, this region of the ocean is particularly oligotrophic, so a storm would have to mix very deeply in order to cause a meaningful phytoplankton bloom (see Figure 4-10 and accompanying discussion). Also, full-season modeling studies of the North Atlantic basin have shown that while TCs do enhance the local flux of carbon out of the ocean, they leave in their wake a region of ocean with decreased oversaturation of mixed layer DIC with respect to atmospheric CO₂, so that the background outgassing of carbon following the storm is diminished and the net carbon efflux over the whole season is about the same in a model with TCs as in one without; over the course of fifteen modeled years, there was no correlation between the number of storms and the amount of carbon escaping the North Atlantic (Koch et al., 2009). Still, this remains a study of a single basin, and global results including regions with a shallower nutricline could still be informative.

In the South China Sea, where nutrients are available much closer to the surface, several studies have worked to describe the change in primary productivity induced by tropical cyclones. For example, Lin et al. (2003) estimated the deepening of the mixed layer after the passage of Cyclone Kai-Tak through the South China Sea in the summer of 2000; using climatological nutrient values, as well as satellite observations
of the change in sea color, the authors estimated that the production induced by Kai-Tak alone was equal to about 3% of the annual total for the South China Sea. They did not, however, attempt to model where the phytoplankton and their constituent chemicals came to rest as the temperature anomaly resolved itself. Also, it has been suggested that, when Kai-Tak passed over the South China Sea, that body of water had an anomalously warm sea surface temperature and an anomalously shallow thermocline and nutricline, leading to a very large entrainment of water normally associated with considerable depth; Kai-Tak has been called a “perfect storm” (Chiang and Wu, 2011). Still, if TCs play a meaningful role in the primary production of some parts of the ocean as has been suggested (Lin et al., 2003; Siswanto et al., 2009), then it is conceivable that they may play a role in the flux of carbon into the ocean.

Regardless of how large a phytoplankton bloom might be caused by a TC, or how much DIC may be mixed up from the deep, it means very little if the nutrient and carbon reservoirs that TCs tap would have been reached anyway as the mixed layer cools and deepens in the winter, since those chemicals are not replenished on short timescales; the possibility exists that strong TCs could alter the seasonality of the carbon flux between the deep ocean and the atmosphere without changing the annual total. Also, for biological sinking to create a long-term carbon surplus at depth, that carbon must sink to such a depth that it is not mixed back up to the surface the following winter (this idea dates at least back to Stommel 1979). Clearly, examining the storm and its immediate aftermath is insufficient; we must model the effect of a storm perturbation until after the mixed layer reaches its late-winter maximum. Likewise, since the profiles of temperature, nutrients, and DIC, as well as the seasonal atmospheric forcing, vary so much from place to place, we must study a broad class of storms in our best approximation of their real environment. For the most part, no attempt has been made to analyze this entire problem at once.\textsuperscript{2} It does have the potential to be significant; if one effect is much stronger than the other, then it would, for example, indicate a (anti-)correlation between tropical cyclone activity and the amount of anthropogenic carbon absorbed by the ocean in a given year.

\textsuperscript{2}Though see the conclusions, Chapter 5.
In this paper, I will describe a simple but defensible model that resolves the upper column of the ocean, including its biology, and with explicit tracking of carbon fluxes within and out of the domain, from specified atmospheric conditions. I will briefly explore its parameter dependence and discuss the assumptions that go into forcing it, and describe a simple methodology for determining the amount of carbon that has been injected into or removed from the mixed and deep layers due to the passing of a tropical cyclone. I will demonstrate the model’s ability to recreate a simple test case, and use it to make an estimate of the effect of a single powerful cyclone, including a discussion of the movement of carbon within the ocean with time. This example, combined with an attempt to model another well-documented storm, should provide a good summary of this modeling technique’s strengths and weaknesses, and convince the reader that the former outweigh the latter. Finally, the model will be used to simulate the carbon flux induced by an entire calendar year’s worth of storms – it will be seen that TCs fall into three categories that can be fairly easily differentiated – and a rough estimate of the total change in carbon flux due to tropical cyclone activity will be obtained.
Chapter 2

Model Description

2.1 Physics

The goal of this project is to construct the simplest model that can be expected to provide realistic results, both to allow both easy computation and attribution. I will then use that model to recreate, as best as possible, real regions of the ocean before, during, and after real TCs passed over them, and compare that with an identical model run sans tropical cyclone; this saves us from having to characterize what a “generic” storm over a “standard” ocean might look like. Because the primary effect of a tropical cyclone on the ocean is to enhance vertical mixing, there is plenty of precedent for modeling the ocean as a single column (e.g. Price et al. 1978; Zedler et al. 2002). Since the local relationship between the physical and biological states of the ocean is set by the behavior of the mixed layer, I have taken the model physics of Price et al. (1986) (PWP), which resolves mixed layer depth from the sources and sinks of energy and momentum, as well as a few stability criteria, in a one-dimensional framework (See also Glover et al., 2011). This model can be run at high resolution, therefore allowing smooth gradients of tracers (i.e. nutrients, carbon, biota, and detritus) and a gradual winter mixed-layer deepening.

The upper 400 meters of the ocean are modeled at 1m resolution. This is a larger domain than is strictly necessary, since any domain that consistently contains the mixed layer should provide identical behavior, however, since this is a one-dimensional
model that does not allow any energy fluxes through the sides or bottom, the diffusion of heat from the surface would warm the bottom of a shallow domain unrealistically; likewise diffusion works to smooth the temperature gradients near the bottom of the domain. Modeling the upper 400 meters ensures that the abyssal temperature will drift by no more than about 0.2 K year$^{-1}$. Since we are running this model for under two years, and not decades, this means that the accumulation of heat in the bottom part of the domain will not affect the vertical stability of the column. (See Glover et al. 2011 for further discussion of this behavior.) While numerical stability requires only that the model be run with a time step of roughly six hours, in order to allow the solar forcing to vary smoothly, a time step of a few minutes is used instead.

At every time step, the model first applies solar heating to the top several meters of the water column. Shortwave radiation is absorbed into the system as a double exponential curve: (Paulson and Simpson, 1977; Glover et al., 2011)

$$Q_I(z) = Q_I(0) \left[ Ae^{z/d_1} + (1 - A) e^{z/d_2} \right]$$

(2.1)

Where $A$, $d_1$, and $d_2$ are determined by the optical properties of the water. (See Section 3 for discussion of these constants.) Then, in the uppermost box, cooling due to surface fluxes, i.e. outgoing radiation, evaporation, and sensible heat flux, is applied (see Section 2.2). The density of each box is calculated from a simple linear state equation, and a preliminary mixed layer depth is found such that no static instabilities exist. The condition for static stability is merely the requirement that

---

1Note that drift $\sim \kappa \Delta z \Delta T \Delta z^{-2} \Delta t$. It should be acknowledged here that, as a consequence, this model can never be thought of as producing a “steady state,” even in response to constant forcing, when it is initialized from ocean state estimates; the temperature at the bottom of the domain is always slowly drifting. This could perhaps be countered by restoring the sub-mixed layer to a climatological temperature, but I chose to avoid artificial heat fluxes so I could easily show that the model was properly conservative. Fixing the temperature of the lowest box and allowing arbitrary heat fluxes out of it to maintain that temperature would also keep heat from accumulating, but this would be equivalent to forcing the abyssal temperature toward a constant vertical gradient, which is not seen in the real ocean. In any case, the abyssal drift seen between two runs with identical initial conditions should be very nearly independent of the surface forcing, so it should not compromise our ability to attribute changes to TCs by examining the difference between a TC run and a control run.
potential density \((\sigma)\) increase with depth:\(^2\)

\[
\frac{d\sigma}{dz} \leq 0
\]  

(2.2)

So the model is mixed to a depth \(z_m\) that satisfies this.

The acceleration due to wind stress \((\tau)\) is then applied to this preliminary mixed layer, and the Coriolis acceleration is applied to the entire column, so that the change in fluid velocity\(^3\) \((u)\) in one model time step \((\Delta t)\) is:

\[
\frac{\Delta u}{\Delta t} = -f \times u + \frac{\tau(t)}{-z_m \sigma} \bigg|_{z > z_m}
\]  

(2.3)

The bulk stability criterion is then applied; the condition for mixed layer stability is that the bulk Richardson number is greater than some critical value \(\bar{R}_{ib,crit}\):

\[
\bar{R}_{ib} = \frac{-\frac{g \Delta \sigma}{\sigma z_m}}{(\frac{\Delta |U|}{z_m})^2} = -\frac{g}{\sigma} \frac{\Delta \sigma}{(\Delta |U|)^2} z_m \geq \bar{R}_{ib,crit}
\]  

(2.4)

Where, in Equation 2.4 only, the \(\Delta\) operator represents the difference between the mixed layer and the layer immediately beneath it. \(|U|\) represents the magnitude of the horizontal velocity, and \(z_m\) is the mixed layer depth, which must be found iteratively. If \(\bar{R}_{ib}\) is found to be less than the critical value, the mixed layer is deepened by one level, its properties are recalculated, and the process is repeated.

The critical value \(\bar{R}_{ib}\) is a manifestation of boundary-layer turbulence and must be chosen empirically; while Price et al. (1978) use ocean data to show that, in a model of this type, a critical \(\bar{R}_{ib} = 0.65\) optimally recreates observations of mixed layer depth, different values of \(\bar{R}_{ib}\) will be explored in Section 3.

Once the depth of the mixed layer is established, the stability of the sub-mixed layer is calculated, and when shear instability between two layers exists they are

\(^2\)Note that while “depth” increases downward, the coordinate \(z\) decreases downward. The seemingly-spurious minus sign in Equation 2.3 arises simply because \(z_m\) is a negative number.

\(^3\)While this is a one-dimensional model, horizontal momentum is a vector property; momentum is also mixed by the bulk and gradient mixing specified by Equations 2.4 and 2.5.
allowed to mix together. This criterion is based on the gradient Richardson number:

\[ Ri_g(z) = -\frac{g}{\sigma} \frac{\partial \sigma}{\partial z} \left( \frac{\partial U}{\partial z} \right)^2 > 0.25 \quad (2.5) \]

When gradient instability exists at a given level, it is mixed with the level immediately beneath it; gradient mixing thus “sweeps” down the column to the depth where \( Ri_g \geq 0.25 \), which is the critical value for stability of a stratified fluid to infinitesimal perturbations (Miles and Howard, 1964).

Finally, molecular diffusion and Ekman pumping are applied. At the surface, the pumping velocity is proportional to the curl of the wind stress (e.g. Gill 1982):

\[ w_c = \frac{\nabla \times \tau_s}{\sigma f} \quad (2.6) \]

As an approximation, I have specified that the pumping velocity falls off linearly with depth, until it vanishes at \( z = 300 \) m. This neglects the more complicated behavior inside the Ekman layer, which we would expect in the upper 20 meters or so of the ocean, and indeed both the linear decrease in pumping velocity and the depth at which it vanishes are rather arbitrary. However, in this one-dimensional model some approximation of this sort must be made, since, unlike in the real ocean, we have no lateral convergence or divergence to balance the pumping. If we used a full Ekman spiral calculation (or, feeling less confident prescribing the Ekman layer depth, a mean Ekman pumping as Eqn. 2.6 and \( dw/dz = 0 \)), we would be pumping heat through the top and bottom of the domain. In the tropics where \( \nabla \times \tau_s/f \) is generally positive, this would result in raising the isopycnals by as much as \( O(10 \text{ m yr}^{-1}) \). Glover et al. (2011) recommends that for such a model one prescribe a compensating heat flux as a tunable parameter. This technique is not particularly desirable if we are trying to model the mixed layer depth through multiple seasons, and becomes untenable when we add tracers into the mix.

Allowing the Ekman pumping to fall off with depth therefore strikes a compromise between allowing the wind to pump the ocean upward and not wanting to raise the
isopycnals tremendously over the time we run the model. Still, some decision must be made regarding what to do with the mass that flows into or out of the domain as a result of the pumping that does occur; in a real tropical storm there is a region of downwelling outside the radius of maximum winds that balances the region of upwelling directly under the storm track, (see for example Bender et al. 1993) but this is beyond the scope of this model. Simply allowing material into and out of the domain is also undesirable, as this represents a spurious source and sink of heat and tracers. In order to allow the model to be perfectly conservative, I have made the decision to explicitly add or subtract heat and tracers to compensate for this effect.

By continuity the horizontal flux of any tracer $A$ induced by a change in vertical velocity is:

$$A \frac{\partial u}{\partial x} = -A \frac{\partial w}{\partial z}$$

And so adding a compensating flux can be done by simply changing the advection equation:

$$\frac{\partial A}{\partial t} = -w \frac{\partial A}{\partial z} - A \frac{\partial w}{\partial z} + A \frac{\partial u}{\partial z}$$

(2.7)

The above modification prevents mass from flowing through the sides of the domain; additionally, I have specified that no mass flows through the top or bottom of the domain. Together, these approximations mean that Ekman pumping thus takes the role of a stronger (near the surface), directional diffusivity. This may be the largest weakness currently present in the model: it effectively operates in an infinite, homogeneous horizontal domain. In the real ocean, the temperature anomalies in the wake of tropical cyclones are very much finite, and lateral (isopycnal) diffusion does play a role in removing the warm anomalies that are left at depth (e.g. Emanuel, 2001). Still, this is surely a better approximation than making some blanket assumption about what is actually happening in the region near the column we are modeling and allowing horizontal diffusion between our column of interest and some nearby column.
After all the physical processes have been resolved, the biological processes are allowed to step forward in time. The biological components are treated as passive tracers with regards to the physical model; they are moved with the fluid but do not affect its motion. See Section 2.3.

2.2 Forcing

Characterizing the flux of solar radiation is simple at the top of the atmosphere, but at the surface matters are complicated tremendously by clouds. While it would be easy to prescribe a mean cloud fraction and let the shortwave flux vary sinusoidally over the course of the year and the day, this is a poor estimation of reality; the presence of clouds can change the peak shortwave heat flux by more than 20% from one day to the next. Rather than attempt to parameterize this, I decided to use the surface shortwave flux provided by the three-hourly ERA-interim reanalysis (Dee et al., 2011). The ERA reanalysis is produced by a numerical weather prediction model constrained by observations; because it provides forecasts eight times a day, it can produce a much smoother diurnal cycle than other products. For example, the CORE dataset of Large and Yeager (2004) provides only a single daily value for radiation with no diurnal cycle; while this is works well for models that use large-scale parameterizations to establish the mixed layer depth, it does not provide enough information to run a model that resolves local static instability.

Because the ERA reanalysis provides a net surface shortwave flux which varies with cloud cover, it is natural to use the longwave flux associated with that product as well. Any attempt to fit the upward longwave flux to the Stefan-Boltzmann law would be bound to perform poorly. The downward longwave flux from the sun would be neglected; more importantly, prescribing a constant longwave cloud reflectivity is physically incongruous with allowing shortwave cloud transmittance to vary. Thus, the net flux of shortwave radiation in and longwave radiation out are prescribed in a manner completely independent of the temperature of the atmosphere or ocean. By itself, this would be hazardous, as there would be no mechanism to keep the ocean
temperature from drifting arbitrarily, however, the use of bulk parameterizations for the fluxes of sensible and latent heat ensures that the ocean temperature is always moving toward equilibrium with the atmosphere, as well be shown.

Sensible and latent cooling of the ocean can be easily prescribed with bulk formulae, respectively (Gill, 1982):

\[
Q_s = \rho_a C_p C_D |u| (T_a - T_s)
\]

\[Q_L = \rho_a L_v C_E |u| (q^*(T_s) - q_a)
\]

Where \(\rho_a\) is the density of air, \(C_p\) its heat capacity, \(L_v\) is the latent heat of vaporization of water (itself a weak function of \(T\)), and \(q^*\) the saturation specific humidity, the pressure dependence of which has been neglected. Wind speed, air temperature, and humidity are generally taken at a reference height of 10 m. \(C_D\) and \(C_E\) represent the bulk transfer coefficients of sensible and latent heat. Following Large and Pond (1982), I have taken these values to be \(1.13 \cdot 10^{-3}\) and \(1.15 \cdot 10^{-3}\), respectively. \(L_v\) and \(q^*\) are calculated using the conventional formulas found in Emanuel (1994).

ERA fluxes are available for sensible and latent cooling, but using bulk heat transfer parameterizations ensures that the sea surface temperature will always relax toward the atmospheric temperature; my own experiments have shown that this does not happen reliably when using all prescribed ERA fluxes. Note also that the ERA-Interim reanalysis does not interact with the ocean; for years between 2002 and 2009 it uses the NCEP Real-Time Global Sea Surface Temperature as a boundary condition; in earlier years, other NCEP ocean products are used. While in the annual mean, reanalysis latent and sensible fluxes are fairly close to the mean fluxes arising from turbulent parameterizations, they are not well-correlated.\(^4\) Regardless, since the goal is to model identical regions of ocean in the presence and absence of TC mixing (and the ensuing temperature anomaly), it would not be obvious how to use ERA turbulent fluxes for the no-TC scenario.\(^5\)

\(^4\)E.g., at the BATS site (Section 3) from September 2000 to September 2001, the correlation between the bulk and reanalysis latent fluxes was \(r^2 \sim 0.5\).

\(^5\)The same logic dictates against the use of ERA longwave fluxes to cool the surface, but since the latent cooling is much larger, this doesn’t change the outcome much. A surface temperature
Wind speed, which sets the rate of latent and sensible cooling and determines the amount of momentum deposited into the mixed layer and the strength of the Ekman pumping, is also provided by the ERA-Interim reanalysis via the 10-meter wind product. While earlier iterations of this model, which were forced by NCEP fluxes, used QuikSCAT wind observations, this was always problematic; due to the orbital and sensor configuration of the QuikSCAT satellite, only about ten observations per week are available for an arbitrary point at low latitude. While the high spatial resolution of the QuikSCAT satellite (when it happens to be observing a region of interest) can be very useful for finding snapshot wind speeds of a storm at arbitrary radii and angles, this does not help one characterize a fixed spot on the ocean surface. The ERA-Interim reanalysis, unlike the NCEP and earlier ERA products, does assimilate data from the QuikSCAT satellite in addition to other sources (Dee et al., 2011), and provides wind forecasts at three-hour intervals. It can at least be said that there is no reason to expect the ERA-Interim winds to be less realistic than temporally-interpolated QuikSCAT winds. A quick comparison shows that QuikSCAT winds and ERA-interim winds do tend to follow each other, though the QuikSCAT product sometimes produces winds that are very high, and it appears that these events may not be spurious. See Figure 2-1.

In addition to mean orthogonal winds for each three-hour forecast interval, the ERA-Interim product also provides a calculated maximum gust of specific duration, which is output by the model's boundary layer scheme. This is more useful than it may appear; if we can make an assumption regarding the shape of the distribution of wind speeds, then this gives us enough information to construct a characteristic distribution for each time period. As will be discussed, getting enough momentum into the mixed layer is one of the largest challenges involved in constructing this model, and using nonsteady winds helps this somewhat. Consequently, having a realistic method for estimating transient wind gusts, some of which will be larger depression of a few degrees should change the longwave cooling by only about five percent. I verified that when longwave cooling is artificially turned down by the same factor, latent cooling completely compensates.
than the mean, improves the quality of the model. See Appendix A.

Figure 2-1: Direct QuikSCAT observations and ERA-Interim 10 meter wind speed for calendar year 2006. The large spike in velocity on May 16 is the passing of Typhoon Chanchu, which appears in both products. The large spike seen in the QuikSCAT record on September 26 is presumably due to Typhoon Xangsane, and the winds associated with it are largely missed in the ERA-Interim database. QuikSCAT winds are calculated from all winds within 100 km of the observation point, with a Barnes Filter smoothing radius of 50 km and a correction parameter $\gamma = 0.8$ (see Koch et al., 1983 for an explanation of these terms), with linear interpolation of the orthogonal wind components between observations. The ERA-Interim winds have been calculated from the assumed Weibull distribution as described in Appendix A.

After brief examination of the available data, I have determined that it is not too extreme an approximation to neglect fresh-water fluxes and take salinity to be constant throughout the column at all times.\footnote{Specifically, note that for the mixed layer to deepen due to dynamic instability, $([U_{ML}] - [U_{abyss}])^2$ needs to be large relative to the change in density/temperature across the ML boundary, and also that $U_{ML} \sim \int \tau_{\theta} dt \sim \int |U_{atm}|^2 dt$. Of course, since the column is subject to the Coriolis force, the mixed-layer velocity will not grow arbitrarily large, but it remains the case that having larger maxima of $|U_{atm}|$ should produce a deeper mixed layer.}

\footnote{Over the last ten years of BATS data (Sec. 3), the typical salinity range over the upper 300 m
since temperature differences shrink in the winter and fresh water fluxes are presumably concentrated into large events such as tropical cyclones, but the ability of the model to recreate my limited observations without including salinity is sufficient that I feel it is not worth the added complexity of adding freshwater fluxes and with them a new untunable forcing.

2.3 Biology

As my physical model is built upon PWP (Price et al., 1986), which describes the mixed layer from the simplest reasonable principles, my biological model is very heavily reliant upon the ideas set forth in Follows et al. (2007), which describes the most fundamental characteristics of the phytoplankton life cycle, and provides a framework in which generic ecosystems can be modeled as a series of ordinary differential equations. While their model was used to simulate many hypothetical species of phytoplankton in competition for the same resources, I have modeled just a single generic species, as my goal remains to use the simplest possible implementation that produces reasonable behavior.

A very quick overview of phytoplankton ecology is in order. Phytoplankton can be reasonably thought of as converting sunlight (here denoted as PAR, for Photosynthetically Active Radiation), nitrate, phosphate, and carbonate into more phytoplankton, which live, reproduce, and die over a timescale of one to several weeks. Their growth is primarily limited by sunlight – like all photosynthesizers, phytoplankton cannot live in the dark, but an overabundance of light also inhibits their ability to grow (Kirk, 1983) – and by the availability of nutrients, generally nitrate. While there are also a few situations in the real ocean where phosphate is limiting, this is neglected; phosphate is not resolved and is assumed to always be present in sufficient quantity to support growth. This is not a very great approximation, as at depth phosphorus and nitrogen are nearly in the Redfield ratio everywhere (Lenton and Watson, 2000),

\[
\text{of the ocean was } \approx 0.2\text{PSU, which corresponds to a change in density } \Delta \sigma_{300} \approx 0.15 \text{kg m}^{-3}, \text{ while the characteristic summertime temperature difference between the surface and 300 m depth is } \approx 10, \text{ or } \Delta \sigma_{300} \approx 2.5 \text{kg m}^{-3} \text{ – much larger.}
\]
so for a model that is concerned with nitrate entrainment from depth (as opposed to nitrate fluxes from e.g. rivers), phosphate should never be the limiting nutrient.

However, neglecting iron from our consideration is superficially much riskier. Unlike phosphate, it is the limiting nutrient for roughly half the world’s ocean (Moore et al., 2002). Also, iron is principally brought into the upper ocean by dust deposition from the atmosphere; while some iron is entrained from the deep ocean, this is about an order of magnitude smaller than the atmospheric flux (Fung et al., 2000). Worse still, measurements of both the atmospheric flux and concentration of iron are very sparse (e.g. Parekh et al., 2005) Since both of these must be included in order to discuss the role of iron limitation in phytoplankton growth, considering iron in a model of this complexity would entail making much more significant assumptions than are necessary to include nitrogen. However, modeling studies examining the relative importance of iron and nitrogen in the global ocean have been conducted, using the limited observations of atmospheric dust fluxes and oceanic iron concentrations that are available and extending these by analogy to the entire ocean. These suggest that, in local summer, iron-limited ecosystems are found throughout the Southern Ocean, in the Pacific and Indian oceans within a few degrees of the equator and away from the maritime continent, and in the far north Pacific. However, the parts of the ocean where TCs are commonly found, including nearly all the subtropical Pacific and Indian oceans, as well as the entire Atlantic ocean north of the equator, are much more probably nitrate limited (Moore et al., 2002). Observations of the ratio of iron to nitrate at depth in the ocean support at least the broad structure of this conclusion (Fung et al., 2000). Therefore, we can tentatively neglect iron from our model, though care will have to be taken to ensure that we do not draw important conclusions from storms that pass through iron-limited waters.

Carbonate is never limiting, but of course the purpose of this model is to observe the vertical movement of carbon, so it will be resolved. Phytoplankton will grow at the rate specified by its more limiting resource, i.e. light or nitrogen.

The flux of solar radiation is expressed in fundamentally different units in a biological model than a physical model. PAR refers not to the flux of solar energy but
of photosynthetically useful photons, i.e. those with wavelengths between 400-700 nm. PAR is generally measured in einsteins, 1 Ein = 1 mol photons. Through this project I have assumed that the surface flux of PAR was equal to the net shortwave flux scaled by a constant ratio of 0.24 MJ = 1 Ein PAR, which has an accuracy of “better than 10%.” (Kirk, 1983) Characteristic PAR fluxes through the surface of the ocean are \(O(\mu \text{Ein m}^{-2}\text{s}^{-1})\).

The dependence of phytoplankton growth on nitrate concentration is expressed in terms of a half-saturation, as proposed by Eppley et al. (1969). The growth rate (expressed in terms of its maximum value \(\mu_{\text{max}}\)) grows roughly linearly up to \(N = k_N\), \(\mu = \frac{1}{2}\mu_{\text{max}}\), and then logarithmically approaches \(\mu_{\text{max}}\) above that value (see Equation 2.17). The dependence of growth rate on light uses a somewhat complicated relationship described by McBride (1992) which accounts for both the stimulation and inhibition of phytoplankton growth by PAR (see Equation 2.18); during the daytime it produces a peak a few meters below the surface and falls off exponentially below that.

Zooplankton, which graze on phytoplankton, are neglected in the model. Omitting this predator-prey relationship would be an oversimplification for a model attempting to describe a real ecosystem, but because the flow of material from phytoplankton to zooplankton is a one-way process, and because the goal of this model is to characterize what are essentially short-term perturbations, this simplification does not meaningfully change the outcome. Instead, phytoplankton will be considered to die and decompose directly into their constituent chemicals at a constant exponential-decay rate. These chemicals will be grouped into Particulate Organic Nitrogen (PON), and Dissolved Organic Nitrogen (DON). Both of these groups are remineralized into nitrate with their own characteristic timescales (\(\lambda\)), but since PON is comprised of solids it falls out with its own sinking velocity \(w_s\); this is the only way in this model that carbon can move independently of the background flow. This mechanism also represents the only way for nitrogen to leave the system, and these one-way fluxes are explicitly tracked. Since the bottom of the domain is quite deep relative to the region of interest, though, PON will mostly be remineralized at a depth that is still
resolved by the model but is below the reach of the mixed layer.

Because of the diversity of phytoplankton in the real ocean, the remineralization time and the sinking velocity are difficult to quantify with a single number. However, the e-folding depth of PON \( z^* \) can be measured directly and can be expressed as the ratio of the two:  

\[
z^* = \frac{w_s}{\lambda_{PON}}
\]  

(2.10)

Worldwide, \( z^* \) is found to be about 100-200 meters (e.g. Schmittner et al. 2005); I have thus chosen constants from within the plausible range set forth by Follows et al. (2007) that uphold this relationship.

For notational convenience, phytoplankton concentration will be expressed in terms of the amount of nitrogen tied up in living biota, but the amount of carbon converted into phytoplankton is dictated by the nearly-constant Redfield ratio, which specifies that phytoplankton utilize carbon, nitrogen, and phosphorous at a ratio of 106:16:1.\(^9\) When phytoplankton grow, they consume carbon species from the seawater, and when their constituent DON and PON are remineralized, the corresponding carbon is returned to the system. In the mixed layer, carbon is also introduced or removed from the system via air-sea exchange, which will drive the system toward saturation, albeit with a fairly long timescale.

The carbon content of the ocean is partitioned between three species: \( \text{CO}_2(aq) \), \( \text{HCO}_3^- \), and \( \text{CO}_3^{2-} \). The ratio of these species is \( \mathcal{O}(1 : 100 : 10) \), but the equilibration time between them is on the order of minutes, so it is convenient to track the three species together as dissolved inorganic carbon (DIC). The saturation DIC is a function of temperature and the ionic species present, (Follows et al., 2006). However, solving for DIC\(^*\) using realistic ocean values shows that it very reasonably be approximated as varying linearly with temperature (Williams and Follows, 2011). The work of Bakker et al. (1999) experimentally supports the same conclusion. Air-sea exchange of \( \text{CO}_2 \)

\(^8\)As before, \( z^* \) and \( w_s \) are both negative, but will be freely referred to as positive numbers due to conflicting conventions.

\(^9\)This ratio is not a constant for all species or communities but it holds remarkably well for the phytoplankton community as a whole, so it is sufficient for a model which treats phytoplankton generically. The mechanism that sets the Redfield ratio is not settled (Falkowski, 2000).
brings the mixed-layer DIC into equilibrium on a timescale ($\lambda_{DIC}$) of about a year.\footnote{Strictly speaking, this depends on the wind stress, which means that in the presence of severe weather DIC will travel across the air-sea interface faster, but the chemical reaction is sufficiently buffered that it is not a great approximation to treat $\lambda_{DIC}$ as a constant.}

The biological and chemical model can thus be expressed mathematically:

\begin{align*}
\frac{dP}{dt} &= GP - mP \quad (2.11) \\
\frac{dN}{dt} &= -GP + \lambda_{DON}DON + \lambda_{PON}PON \quad (2.12) \\
\frac{dDON}{dt} &= r_{DON} mP - \lambda_{DON}DON \quad (2.13) \\
\frac{dPON}{dt} &= (1 - r_{DON}) mP - \lambda_{PON}PON - \frac{\partial}{\partial z} (-w_sPON) \quad (2.14) \\
\frac{dDIC}{dt} &= \frac{R}{t} \frac{dN}{dt} + \lambda_{DIC} \left( \frac{DIC^*(T) - DIC}{DIC_a} \right) \quad (2.15) \\
G &= \mu_{max} \min (\gamma_N, \gamma_I) \quad (2.16) \\
\gamma_N &= \frac{N}{N + k_N} \quad (2.17) \\
\gamma_I &= \left( \frac{k_p}{k_I + k_p} \right) \left( \frac{k_I}{k_I + k_p} \right)^{\frac{k_I}{k_p}} \left( 1 - e^{-k_pI(z)} \right) e^{-k_II(z)} \quad (2.18)
\end{align*}

The above equations use Lagrangian derivatives, but since advection and diffusion are explicitly resolved by the model, they can be stepped forward in time after the physics side of the model has finished. See Table 2.1 for a brief explanation of the constants, which are chosen from the middle of the range of those used in Follows et al. (2007).

In order to initialize the biological variables, I first set nitrate values to the World Ocean Atlas climatology (Garcia et al., 2010), Dissolved Inorganic Carbon to the GLODAP climatology (Sabine et al., 2005),\footnote{The WOA does not provide values for DIC.} phytoplankton to a near-infinitesimal value through the whole column, and the phytoplankton remains DON and PON to be everywhere zero. I then run the full model for 500 days, under repeated diurnal forcing equal to the first day I intend to model, with Ekman pumping disabled, and artificially restoring the temperature profile to that of the initial time every 24 hours. This produces a biological profile that is fully in equilibrium near the surface and
Table 2.1: Biology Model Parameters, Eqns 2.11-2.18

<table>
<thead>
<tr>
<th>Symbol</th>
<th>Name</th>
<th>Value</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\mu_{\text{max}}$</td>
<td>Maximum Phytoplankton Growth Rate</td>
<td>1.4 day$^{-1}$</td>
</tr>
<tr>
<td>$m$</td>
<td>Phytoplankton Mortality Rate</td>
<td>0.1 day$^{-1}$</td>
</tr>
<tr>
<td>$\lambda_{\text{DON}}$</td>
<td>DON Remineralization Rate</td>
<td>0.0028 day$^{-1}$</td>
</tr>
<tr>
<td>$\lambda_{\text{PON}}$</td>
<td>PON Remineralization Rate</td>
<td>0.0033 day$^{-1}$</td>
</tr>
<tr>
<td>$r_{\text{DON}}$</td>
<td>$DON/(DON + PON)$, Dead Phytoplankton</td>
<td>0.5</td>
</tr>
<tr>
<td>$k_N$</td>
<td>Nitrate Half-Saturation</td>
<td>$5 \cdot 10^{-4}$ mol N m$^{-3}$</td>
</tr>
<tr>
<td>$w_s$</td>
<td>Sinking rate, PON</td>
<td>0.5 m day$^{-1}$</td>
</tr>
<tr>
<td>$k_P$</td>
<td>PAR Saturation Coefficient</td>
<td>0.012 $\mu$Ein$^{-1}$ m$^2$ s</td>
</tr>
<tr>
<td>$k_I$</td>
<td>PAR Inhibition Coefficient</td>
<td>0.003 $\mu$Ein$^{-1}$ m$^2$ s</td>
</tr>
<tr>
<td>$R$</td>
<td>Redfield Ratio, C/N</td>
<td>106/16</td>
</tr>
<tr>
<td>$\lambda_{\text{DIC}}$</td>
<td>DIC Relaxation Time</td>
<td>1 year$^{-1}$ $z &gt; z_m$</td>
</tr>
<tr>
<td></td>
<td></td>
<td>0 year$^{-1}$ $z &lt; z_m$</td>
</tr>
</tbody>
</table>

which manifests the characteristic PON “rain” in the sub-photic zone. I take the final biological state of this spin-up run as the initial state of my dynamic run.

“Equilibrium” is here defined as the time when the column shows no time evolution in the column total of PON, and has a vertical gradient of PON below the sub-photic maximum consistent with the exponential decay expected from $w^*$. This is not really a steady state – nitrate continues to diffuse upward into the euphotic zone, where it is consumed by phytoplankton and converted into PON (which sinks and maintains the balance of PON below the mixed layer) and DON, which does not sink. This upper-ocean DON has a life span of about a year, but since it is still in the euphotic zone when it breaks down into nitrate this will be quickly eaten by phytoplankton, starting the cycle again. There is, therefore, a small net accumulation of DON in the mixed layer that grows larger the longer the model is spun up. Also, while the PON concentration reaches equilibrium in the sub-mixed layer, there remains a small net export of nitrogen downward due to continual PON sinking. These factors combine to reduce the column-integrated nitrate concentration by around 2% per year, with about an equal portion going to the ever-growing DON reservoir near the surface and sinking out as PON. This is not considered a problem, but it illustrates that, even when we neglect that the dynamical part of this model does not produce a steady response to a steady input, the biological part of the model does not have a steady
state either. The 500-day spinup is chosen as it represents the minimum time to establish a steady PON rain.\textsuperscript{12}

The phenomenon of DON accumulation at shallow depths that ultimately has no place to go but down will be a recurring theme in the discussion of the results.

### 2.4 Simulating TC Mixing

In principle, the ocean model is entirely capable of resolving the changes to the mixed layer due to the passage of a storm, given accurate data describing the wind stress and its curl as the storm passes, and very similar models have been used for exactly that purpose (e.g. Zedler et al., 2002). However, storm wind data are not well-tabulated,\textsuperscript{13} and, for most storms, are not even available. While the ERA-Interim reanalysis does capture enough information about a tropical cyclone to output very high winds when a TC passes over, these winds are still lower than the estimated maximum wind speed.\textsuperscript{14}

Instead, I have measured the deepening of the mixed layer more or less directly. Using the Unisys Best Track database and data from the TMI sensor on the TRMM satellite, I characterize the depression of the sea surface temperature (SST) by subtracting the average of the SST in the week after the storm passes from the average of the SST in the week before the storm passes.\textsuperscript{15} I then artificially mix the model to

\textsuperscript{12}Consulting Table 2.1, we see that if the model were initialized with a large $N$ supply in the euphotic zone, nearly all this $N$ would be turned into DON and PON (leaving behind very small concentrations of $N$ and $P$) in a few dozen days. Also, if a large quantity of PON were created at the surface, it would take 400 days (the depth of the column divided by the sinking rate) for this to spread through the whole column.

\textsuperscript{13}Of course, the Best Track and IBTrACS databases contain records of the maximum sustained winds, but our model requires two-dimensional winds to operate, and a single wind vector, however it is melded into the larger wind data base, will not correctly deepen the mixed layer. While it would certainly be possible to devise a scheme to allow simulated hurricane winds to mix our model, perhaps following a climatology such as Willoughby et al. (2006), the technique outlined here produces the important effect far more simply.

\textsuperscript{14}E.g., when Typhoon Chanchu was a Category 5 storm on the Saffir-Simpson scale, the ERA-Interim wind speed was consistent with a Category 1 storm. This is strong enough to make it necessary to filter out the tropical storm in the control run, but not nearly strong or long-lived enough to produce the observed cooling. Koch et al. (2009) report a similar strong under-representation of TC winds in the NCEP/NCAR reanalysis.

\textsuperscript{15}The TRMM data is available at 0.25° resolution which I spatially average to 1° to ensure that the storm isn’t accidentally missed; this is therefore a fairly conservative estimate of the depression.
a depth such that the surface temperature is depressed by the same amount. In the model, this TC mixing is specified as occurring in the space of one time step; this is an acceptable approximation because while real mixing does occur over a timescale much longer than the model time step, it is still much faster than the characteristic growth rate of phytoplankton, so mixing for longer would not change the biological behavior in any meaningful way. (Note that for the particular case of Hurricane Felix, which occurred before the TRMM satellite was launched, I instead use the reported SST depression from Bates et al. 1998.)

This approximation neglects the cooling due to enhanced surface fluxes when the TC passes; back of the envelope scaling shows that this should be negligible, and Sanford et al. (2007) provide observational data that supports the same conclusion.

\[ \Delta T = \frac{1}{\tau_d} \left[ \int_{t_0}^{t_0+\tau_d} T_S \, dt - \int_{t_0}^{t_0+\tau_d} T_S \, dt \right] = T(0, t_0) - z_h \int_{z_h}^{0} T(z, t_0) \, dz \]

Given satellite SST \( T_s \), model temperature \( T \), and storm passage time \( t_0 \), this relationship finds TC mixing depth \( z_h \).
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Chapter 3

Model Tuning and Evaluation

There is a distinct shortage of observational data for the ocean’s state; most of what we do know comes from sporadic cruises, as well as a few moorings which measure the ocean’s state at a fixed location over a long time. One of the best products available is the Bermuda Atlantic Time Series (BATS) Hydrostation ‘S’ time series, which provides biweekly salinity, temperature, and density measurements at a location about 50 km east-southeast of Bermuda.\(^1\) I will use data from this site to check the behavior of my model, and to tune a few of the more negotiable parameters.

Because the mixed layer reaches its deepest at the end of winter, the goal is for the model to recreate observations from tropical cyclone season to the end of winter. For several summer-winter periods between 2000 and 2006, I initialized the model from BATS observations, ran the model with the usual ERA forcing, and compared the output to the observations over the same time period.\(^2\)

---

\(^1\)Specifically, 32°10’N, 64°30’W

\(^2\)I attempted to minimize \(\sum_{m} \int_{-100m}^{0} |T_m(z,t) - T_{\text{model}}(z,t)|^2 dz\) over five distinct years beginning in September 2000, where each \(m\) is a distinct measurement taken at Hydrostation ‘S’; while Figure 3-2 interpolates these measurements in time my cost function does not. The choice of time limits is slightly arbitrary, and the choice of 100 meters as the maximum depth of interest is simply to avoid placing authority at levels where little ever happens.
3.1 Physical Constants

The decision to treat the ERA radiation fluxes, temperature, wind, and humidity as physically unimpeachable left only a few constants to tune. Large and Pond (1982) show that, over the range of winds commonly seen over the ocean, it is appropriate to use constant values for the sensible and latent bulk transfer coefficients. While the momentum transfer coefficient does vary with wind speed above a threshold of about 14 m s$^{-1}$, these values rarely appear in the reanalysis data.

Using textbook values for the bulk transfer coefficients is intuitively better than using tuned values anyway; altering the way energy flows into the system undermines our best approximation of the inputs for no reason, and a model that produces apparently-physical output from non-physical heat fluxes is suspect at best. Instead, it would be better to adjust the values that determine the way heat is moved inside the system. This is what I have done. The physical constants I allow to be tuned are the diffusivity, the characteristics of the shortwave absorption curve, and the critical bulk Richardson number for dynamic instability.

The shortwave absorption curve (Eqn. 2.1) follows a double-exponential to account for the strong absorption of light at and beyond the edges of the visible spectrum in the upper few meters of the ocean, and the longer absorption scale of light in the middle of the spectrum. Paulson and Simpson (1977) show that, globally, values for $A$ (which represents the empirical partitioning between these two spectral classes) are in the range of 0.4-0.7; $d_1$ can be from several tens of centimeters to a few meters, and $d_2$ is 15-40m; the larger either depth is, the larger the fraction of the solar radiation that will be absorbed below the surface. While these values vary both in space and with season, at low latitude and away from land these variations are small enough that is not too large an approximation to use a single value (Jerlov, 1968).

Indeed, we would be justified in using textbook values for the optical parameters and avoiding tuning altogether. However, allowing these to vary permits us to modify the tendency of the mixed layer to reach a relatively shallow depth without changing the amount of energy that flows into the system. The three optical values, $A$, $d_1$, and $d_2$
will be allowed to vary over their full global range for the purposes of model tuning, and possible shading effects due to the presence of phytoplankton are neglected – i.e. phytoplankton are modeled as being transparent.

Following the original PWP model, I use a single diffusivity \((\kappa_z)\) for all properties. This arises from convenience and a need to accommodate transport driven by subgrid-scale processes. The diapycnal eddy diffusivity in the open ocean (the “pelagic” diffusivity) has been measured on the order of \(10^{-5}\) m\(^2\)s\(^{-1}\) (Ledwell et al., 1993). In a one-dimensional model such as this, we implicitly assume that the isopycnal surfaces are horizontal, and so the diapycnal diffusivity is the same as the vertical diffusivity. Because the eddy diffusivity is several orders of magnitude higher near topography, care will have to be taken to stay at least a few kilometers away from land and a few hundred meters above the bottom of the ocean, where any model like this would be sure to break down (Munk and Wunsch, 1998). Within the vicinity of the published values, we can safely tune the diffusivity somewhat to nudge our model output toward observations; we will allow the diffusivity to vary within a factor of 10 of the \(1.1 \cdot 10^{-5}\) m\(^2\)s\(^{-1}\) measured by Ledwell et al..

The decision to experiment with a variable bulk Richardson number arose from the persistent difficulty the model had creating a sufficiently deep summer mixed layer. While the model had no problem producing a plausible winter mixed layer, and can preserve a summer mixed layer reasonably well if initialized with midsummer values, the transition from winter to summer produces a mixed layer that is systematically too shallow (by roughly 10-30 m) and cool (by 2-3 K at 30 m depth, and perhaps half that at the surface). The absence of Langmuir circulations and turbulence induced by surface waves likely plays a role in this; relatively simple simulations in a large eddy simulation model show that the presence of Langmuir circulations enhances the mixed layer deepening when, and only when, the product \(\Delta \sigma z_m\) (Eqn. 2.4) is small, as would be the case in our model in spring (Noh et al., 2011). Also, the presence of surface wave breaking in such a model considerably increases the mixing of the upper 5-10 meters of the ocean (Noh et al., 2004). While it would be possible to include a semblance of the latter effect by specifying a separate diffusivity
for the upper few meters of the ocean, including Langmuir circulations in a model such as this would be considerably more complicated. A previous implementation of this model included a tunable stochastic multiplier for the wind speed, but this never had any real physical basis, and has been superseded by the current assumed-distribution scheme for generating wind speed time series. Rather than deepening the mixed layer by arbitrarily increasing the wind speed, which also has the undesirable effects of increasing the sensible and latent heat fluxes and the wind stress curl, I decided to allow the critical bulk Richardson number to vary between the theoretical minimum of .25 (equal to the value \( Ri_{g, \text{crit}} \) at which flows are instable to infinitesimal perturbations) and 1 (a natural consequence of assuming a single diffusivity for heat and momentum). Larger values will result in calmer winds producing a deeper mixed layer, as should be evident from Equation 2.4.

3.2 Tuning results at Hydrostation ‘S’

Having settled on the physical constants to tune, actually finding the optimal values is not intellectually challenging; the model was simply initialized with an observation from Hydrostation ‘S’ and run repeatedly over the full parameter space of diffusivity, optical properties, and \( Ri_{b, \text{crit}} \); the combination that gave a temperature evolution that most closely matched the actual twelve-month period was found. The results contain a comforting characteristic: the best values of diffusivity and critical Richardson number are independent of the other values, and the optimal values for the three optimal parameters depend only on each other. The optimal mechanical properties are \( \kappa_z = 4 \cdot 10^{-5} \text{ m}^2 \text{ s}^{-1} \), \( Ri_{b, \text{crit}} = 1 \); the radiation parameters from Equation 2.1 that produce the closest results are \( A = 0.4 \), \( d_1 = 2 \text{ m} \), and \( d_2 = 30 \text{ m} \).

The diffusion coefficient is in the middle of range over which it was allowed to vary, a factor of four higher than that observed by Ledwell et al. (1993). The optimum falls here as a compromise between allowing enough heat to diffuse down from the mixed layer, and preventing too much heat from (adiabatically) rearranging itself in the abyss over the period of interest. It may in fact be better to choose a higher diffusivity
to reflect the fact that the former problem has more chance of producing misleading results than the latter, as will be discussed further in the following paragraphs.

The radiative transfer coefficients contain a standard value for $A$, which represents the partitioning of the solar shortwave radiation into longer-wave components with a shorter penetration depth and shorter-wave components with a longer depth. The penetration depths for both are in fact the largest values within the realm of plausibility, corresponding to an extremely clear ocean such as the Wedell Sea (Jerlov, 1968; Glover et al., 2011). It is reasonable to assume that if we allowed the penetration depth of shortwave radiation to become even larger, the model would perform better still; this was not done simply because it seemed impossible to justify. That the model best recreates observations with very clear water is further indication that the major physical problem is producing warm enough temperatures at intermediate depth; when solar radiation is allowed to penetrate deeper into the ocean, it produces at least a modest summer mixed layer due to heating (and static instability) alone. Of course, the warm oceans of low latitude are bound to be less clear than this, which would correspond to more heating at the very highest parts of the ocean, and less in the 10-40 meter range. I feel that it makes more sense to allow a slight breach of physical plausibility in order to produce a more realistic behavior. These assumptions do not alter the amount of shortwave energy that enters the ocean; they only change the depths at which it gets absorbed.

Tuning the critical Bulk Richardson number for dynamic instability quickly revealed an important characteristic of the model behavior: dynamic instability does not play a role in setting the depth of the wintertime mixed layer, or the maintenance of the summertime mixed layer. The first of those observations should come as no surprise – cooling the surface causes a static instability, which means that the depth of the winter time mixed layer is determined only by the amount of heat lost by the ocean in the winter time. Similarly, if the model is initialized with a warm and moderately deep mixed layer, then the model will continue to distribute heat from the sun throughout that layer, and because the mixed layer does not cool off enough at night to cause its temperature to fall below the stratified layer beneath it, its depth
will tend to be maintained through the summer. However, when the ocean is cool and has a deep mixed layer, as it does around the vernal equinox, then increasing the insolation will, in the absence of dynamic instability, tend only to form a warm mixed layer about as deep as the penetrative depth of shortwave radiation (15 to 25 meters); this shallow mixed layer will respond to changes in insolation, air temperature, and humidity, and will slowly warm the stratified layer beneath it by diffusion, but the mixed layer will not deepen. As would be expected from Equation 2.4, larger values of $Ri_{B,crit}$ produce a deeper mixed layer in the transition from spring to summer. However, the effect is rather small; initializing with observations from early September, the difference between summer time mixed layer depth in the case that $Ri_{B,crit} = 0.65$ and $Ri_{B,crit} = 1$ is only a few meters, and this causes a rise in temperature by late August of the following year of well under half a degree. There is ultimately not very much that can be done to bring about a sufficiently deep, warm summer time mixed layer without adding energy into the system artificially, or perhaps by providing some much stronger mixing force.\(^3\) Ultimately, the non-traditional value of $Ri_{B,crit} = 1$ was used simply because some improvement was better than none. The original problem remains only somewhat solved: springtime model forcing cannot turn a spring mixed layer into a summer mixed layer.

Figures 3-1 and 3-2 show the behavior of the model over the 2000-2001 test period with the optimized diffusion, radiation, and $Ri_{h,crit}$. As the blue region in Figure 3-2 shows, the performance is still problematic; the modeled summer mixed layer is initially too cold and shallow, at one point falling more than 3°C below the observed temperature; while the surface temperature comes closer to the appropriate value later in the summer (within about 1°C), the mixed layer remains too shallow. This is problematic, and not to be taken too lightly. The absence of lateral transport may be sufficient to explain this matter, but without moving to a three-dimensional model this remains a matter of speculation. It would be prudent to recall that the BATS site lies in a region of the ocean that receives more oceanic heat from low latitudes than it

\(^3\)Note that turning $Ri_{B,crit}$ up to a still higher value would not produce much mixed layer deepening. Since wind stress is deposited into the entire mixed layer, a 40 meter mixed layer requires twice the wind stress to produce the same increase in $|U|$ as a 20 meter ML.
passes along to higher latitudes, if not by much (Hartmann, 1994), and that this is a fundamental limitation of modeling a three-dimensional ocean with a one-dimensional model.

Also, the good performance of the model over the entire winter and spring is more than a small consolation; as discussed, this is the time where the behavior of the model is the most important, because the depth of the storm mixing versus the depth of the winter mixed layer is a major factor in the amount of carbon flux induced by the storm. Between the beginning of September and the middle of April the model stays within about half a degree of the observations, though in that span there is a tendency for the model to produce a mixed layer that is slightly too deep.

Finally, an attempt was made to tune the model off the observations at the HOT site (e.g. Fujieki et al., 2011). The observations there show an oscillation in temperature at depth that the model could never hope to recreate without lateral fluxes (not shown). Additionally, the modeled upper-ocean temperatures are systematically about half a degree colder than the observed ones, even when initialized with a direct measurement. This may point to an inadequacy with the model forcing, but none of the tunable parameters can change that. By eye, the modeled mixed layer does not appear to be shallower than the observed one, which is the best that can be hoped for; the values of the tunable parameters that best recreate observations are also unchanged between the BATS and HOT sites, although the raw value of the cost function is quite a bit higher.

We would expect that part of the reason that Figures 3-1 and 3-2 perform relatively well is because the model was initialized with observations to produce them; elsewhere, such observations are not available, necessitating the use of state estimates. Starting the model with the World Ocean Atlas monthly climatology (Locarnini et al., 2006) proved to be a somewhat unsatisfactory decision; at the BATS site, the climatology was systematically colder than the observations throughout the entire column and in all seasons, and the mixed layer was similarly too shallow; this is not the sort of problem that can be resolved by surface fluxes. This led me to try the ECCO product, which produces state estimates of the ocean via a model (the MIT GCM)
Figure 3-1: Modeled column temperature, BATS Site, Sept. 1 2000 - Sept 1 2001. Cf. Figure 3-2 The mixed layer cools without shoaling much as summer gives way to winter, and in the winter a cool, deep mixed layer is formed. The mixed layer that forms in June of the following year is both shallower and cooler than the one the model was initialized the previous fall.
Figure 3-2: Modeled - observed column temperature, BATS Site, Sept. 1 2000 - Sept 1 2001. While the model produces a mixed layer that is just slightly too short in the fall of 2000, and temperatures that are within a degree of observations in the winter, the mixed layer is clearly both too shallow and too cool the following summer; while surface fluxes create roughly correct surface temperatures, there is no way the model can remove the cold anomaly at depth. It will be necessary to avoid initializing the model with winter values if we hope to recreate summer values.
constrained by observations (Stammer et al., 1999). Initializing the model with ECCO estimates produces results that converge with those generated by initializing with direct observations after a few months near the surface, which is the only place such discrepancies could ever hope to be resolved; see Figure 3-3. This says nothing about the actual truth of the model, of course, but it gives us some confidence that initializing the model with ECCO estimates produces sufficiently similar behavior to initializing with observation, in particular, particularly concerning the depth of the mixed layer. Based on the example of the BATS measurements, we will use a 60-day spinup of our model in all locations, since the best we could ever hope for is for any discrepancies in the thermal structure of the ocean that are inconsistent with our modeling technique to be removed, and 60 days is roughly the time it takes for a shallow temperature anomaly to smooth itself out.

Figure 3-3: Model output, ECCO initialization - initialization by direct observation. The ECCO estimate is warmer than the observations near the surface (by over 1 °C), and cooler at depth (by around 1 °C). While this pattern generally holds in other years, it is not always as pronounced. After 60 days, the upper ocean is less than one degree warmer in the ECCO-initialized case than it is in the observation-initialized case.
Chapter 4

Results and Discussion

A standard methodology can be implemented: after doing a 500-day spinup of the biological and chemical component of the model with a strongly restored temperature profile, we will begin running the model 60 days before the storm passes. This is enough time for the ECCO state estimate from which it is initialized to come into rough equilibrium with the ERA forcing, but not so long that we run the risk of initializing a late-summer storm with a late-spring mixed layer, as the specified forcing would do a poor job of establishing a proper mixed layer in the intervening months. The TC and control runs will have identical winds, atmospheric conditions, and radiative forcing, with the single difference that tropical cyclone winds (which do appear, after a fashion, in the ERA-Interim reanalysis) are filtered out of the control case, and in the TC run the mixed layer will be artificially and instantly mixed to a depth that satisfies the observed temperature depression. Both the TC run and the control run will be allowed to go through the end of winter and the re-establishment of a shallow mixed layer; Northern Hemisphere runs will go through June 1, and Southern Hemisphere storms will be run to December 1. The relative change in the contents of the various carbon reservoirs at the end time can be summarized to a single number, $\Delta C_{\text{abyss}}$, which represents the amount of carbon that has entered (if positive) or escaped (if negative) the part of the ocean below the deepest winter mixed layer. As we will see, there are a few broad classes of effects that storms can fall into.
4.1 Typhoon Chanchu, May 2006

In order to demonstrate the basic functionality of my model, I have modeled the passage of the Category 5 Typhoon Chanchu over the South China Sea on May 15, 2006, which deepened the mixed layer such that the sea surface temperature fell by an estimated $2.5^\circ$C. Because this storm occurred quite early in the season, it was necessary to run the model until after the winter mixed layer deepening had passed, i.e. until June 1, 2007.

The modeled no-TC scenario plausibly constructs the seasonal cycle of the upper ocean (Figure 4-1(a)–4-5(a)). A phytoplankton bloom occurs in late winter and early spring of both 2006 and 2007, both of which are inside the modeling period. They stimulate modest “showers” of PON, and Figure 4-3(a) makes it clear that the phytoplankton bloom was somewhat stronger in early 2007 than in 2006; it is unknown whether this is consistent with reality. Total organic carbon ($R(P + PON + DON)$, Figure 4-4(a)) is roughly constant in the mixed layer until the larger phytoplankton bloom in the second winter; this is a good sign.

The TC mixing produces a mixed layer cold anomaly which is removed over a timescale of a few months, although the deeper warm anomaly, which can only be removed by diffusion, persists for much longer; as the ML cools in the winter it entrains up some of this relatively-warm water, which speeds the removal of the anomaly, but note that in this particular case the storm-induced mixed layer depth (about 100 m) is modeled to be deeper than the deepest ML depth reached in the winter (about 70 m). This is not the case for most storms (see Section 4.3), and for those storms whose ML deepening is less than the wintertime depth we would expect more of the deep heat anomaly to be removed.

Likewise, the biological implications of the TC passage appear to be intuitive: a large phytoplankton bloom is induced by the mixing event, and the winter bloom is very slightly stronger than in the control run, due to a small increase in the winter

\[^1\text{Mixed layer phytoplankton concentrations following the TC mixing are about twice as high as those associated with the wintertime bloom in the TC run.}\]
time mixed layer maximum. Additionally, a long-term PON “rain” is observed in the control run, such that particulate sinking and remineralization balance each other since particulates have an e-folding depth $z^* = 200m$ (Fig. 4-3(a)). TC mixing disrupts this equilibrium, creating a large surplus of PON in the mixed layer that takes some time to fall out due to the availability of sunlight, which allows the nitrate produced by the PON remineralization process to be re-utilized by phytoplankton. There is a relative DON surplus of similar size in the upper ocean as well, but this does not sink out.

This DON abundance gives rise to an interesting situation: at the end of the winter there remains a non-negligible reservoir of organic carbon in the mixed layer (Figure 4-4). This DON reservoir will break back down into N and DIC with a characteristic timescale ($\tau$), but because it remains at a sufficiently shallow depth that growth is not inhibited by darkness, the constituent particles will almost immediately be used to produce more phytoplankton, which in turn will decompose into sinking PON and neutrally-buoyant PON. It is clear that, in the context of this model, any DON remaining in the mixed layer, or at a depth that the wintertime mixed layer reaches to, will eventually deposit its carbon and nitrogen into the stratified part of the column. Given the decay timescales, this process takes several years, which is longer than the model is run for; indeed, in the real three-dimensional ocean this DON surplus would spread out considerably. However, as long as the DON surplus that forms in the wake of a TC remains in a location where nitrate availability is the limiting factor in phytoplankton growth, the carbon associated with it will eventually sink down. For the typhoons that occur in the oligotrophic gyres of the Pacific Ocean, this is a safe assumption; for storms that reach higher latitudes, where surface waters circulate toward the pole, it is riskier. In this section, the changes in the deep carbon reservoir will be shown assuming all the shallow DON sinks, as well as a brief discussion of the

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2Because Typhoon Chanchu mixed to a greater depth than the winter mixed layer, the presence of a warm anomaly at depth causes a lower static stability as the mixed layer cools and therefore more mixing. However, if the warm anomaly were created at a depth that were shallow relative to the winter mixed layer, it would tend to cause a shallower winter mixed layer, as all the heat in the anomaly would have to be removed by surface fluxes before the mixed layer deepening could resume. This will be seen in the following section. More concisely, the warm anomaly encourages mixed layer deepening within its own vertical span, but discourages it from extending deeper.
changes if we assume that none of it sinks. The reality must of course lie somewhere in between.

![Temperature, Control](image_url)

(a) Reference

![Temperature, TC-Control](image_url)

(b) TC - Reference

Figure 4-1: Temperature Profile, Typhoon Chanchu. TC mixing warms the upper thermocline by pulling heat from the mixed layer; while the mixed layer cold anomaly is quickly removed by surface fluxes, the deep layer anomaly can only be removed by vertical diffusion, and by the deepening of the mixed layer in the winter. In the case of Typhoon Chanchu, the storm mixes to a deeper level than the mixed layer is modeled to reach in the winter, so a warm anomaly remains.

If our goal is to see the change in carbon distribution through the column, we need a strategy for partitioning the reservoirs of carbon, as well as the sources and sinks. We can first recall a few characteristics of the biological and chemical side of the model. While carbon can enter or exit the domain both through air/sea exchange, and can exit through biological sinking at the bottom, nitrogen can only exit the domain through biological sinking. However, every mole of nitrogen that sinks out in the form of PON carries $R$ moles of carbon with it (where $R$ is the Redfield ratio).
Figure 4-2: Phytoplankton Profile. The reference run produces a fairly weak, deep phytoplankton bloom in response to winter mixed layer deepening, which gives way to a near-infinite layer immediately underneath the mixed layer; the following winter it is dispersed by a strong mixing event late in the year and a stronger phytoplankton bloom occurs. The TC run shows rapid, strong phytoplankton growth following the TC passage, and a stronger phytoplankton bloom the following winter as well, likely because as TC-induced phytoplankton sink and remineralized, they deposited nutrients at a level that the mixed layer would reach the following winter.
Figure 4-3: Particulate Organic Nitrogen Profile. The PON “rain” rate is visible to some extent in the upper thermocline throughout the year, spreading downward and space and forward in time from strong phytoplankton growth. The PON deficit seen at the time of TC passage in the lower panel is due to the mixing of the PON present in the thermocline with the (PON-free) mixed layer.
Figure 4-4: Profile of Organic Carbon, $OC = R(P + PON + DON)$. Combined with Figures 4-2 and 4-3, we can infer that most of the organic carbon remaining in the mixed layer more than two or so months after TC passage is DON, though note also that those figures show nitrogen content, which is a factor of $R \approx 6.5$ smaller than carbon content.
Figure 4-5: Profile of Dissolved Inorganic Carbon. While the control run is in roughly steady state, since the mixed layer depth does not vary tremendously over the course of the year at this site; the passage of the TC create a DIC deficit at depth due to entrainment, and near the surface due to strong phytoplankton growth. The latter is removed by air-sea fluxes and DON remineralization. The former diffuses out somewhat, and is partially reduced by remineralization of PON sinking through, but mostly persists. However, if the winter time mixed layer were deeper, it would substantially remove the deep anomaly.
Of course, since the bottom of the domain is quite a bit deeper than the characteristic remineralization depth (400 m vs 200 m), very little detritus will actually sink out of it, instead most will remineralize into nitrate and DIC in the part of the domain that is resolved. In order to include this, we will break the domain into the “abyss”, defined as the part of the column below the deepest point the mixed layer reaches in the winter, and the “transient mixed layer”, defined as the part of the column above that depth. Carbon will be considered to have sunk deep enough to be beyond the reach of the conventional seasonal cycle if it sinks into the abyss. Additionally, carbon that remains in the mixed layer in organic compounds (phytoplankton, DON, and PON) will be counted as having a final destination below the atmosphere, as will be discussed.

We can therefore break the amount of carbon in the system into six categories: Dissolved Inorganic Carbon in the abyss, DIC in the transient mixed layer, Organic Carbon in the abyss, OC in the transient mixed layer, organic carbon that has sunk out of the domain due to PON sinking, and inorganic carbon that has entered or left the domain due to air-sea fluxes. Note that the sum of these six quantities will always be a constant equal to the total amount of carbon in the system at the initial time, and that checks are performed to verify that carbon is actually conserved. Both the TC run and the control run will have the same amount of total carbon in each reservoir at time zero.

\[\text{This is the case due to the approximation made in Equation 2.7}\]
These six quantities can be expressed as follows:

\[ \text{DIC}_{\text{abyss}}(t) = \int_{z_{\text{w}}}^{z_{\text{max}}} \text{DIC} dz \]  
(4.1)

\[ \text{OC}_{\text{abyss}}(t) = \int_{z_{\text{max}}}^{z_{\text{w}}} R(P + DON + PON) dz \]  
(4.2)

\[ \text{DIC}_{\text{ML}}(t) = \int_{z_{\text{w}}}^{z_{\text{0}}} \text{DIC} dz \]  
(4.3)

\[ \text{OC}_{\text{ML}}(t) = \int_{z_{\text{0}}}^{z_{\text{w}}} R(P + DON + PON) dz \]  
(4.4)

\[ S_p(t) = \int_{0}^{t} \left( \frac{\partial}{\partial z} [-w_n PON] \right)_{z_{\text{max}}} dt \]  
(4.5)

\[ F_a(t) = \int_{0}^{t} -\text{DIC}_a dt \]  
(4.6)

\[ \Delta A = A_{TC} - A_{\text{control}} \]

Where \( z_{w} \) represents the maximum winter-time mixed layer, i.e. the dividing line between the abyss and the transient mixed layer, and \( R \) is the Redfield ratio of carbon to nitrogen. \( S_p \) denotes sinking of particles out of the domain (see Equation 2.14) and \( F_a \) represents the air-sea carbon flux, with \( \text{DIC}_a \) defined in Equation 2.15. Both \( S_p \) and \( F_a \) grow more positive as carbon leaves the domain – due to the way the model is constructed, \( S_p \) is always non-negative and has a non-negative time derivative, though \( \Delta S_p \) can still be negative if less carbon has sunk in the TC run than at the equivalent time in the control run. These quantities are only really meaningful when comparing the TC and control runs, so the \( \Delta \) values will be what we examine. The difference between the TC and control values of all the carbon reservoirs will be shown in Figure 4-6; we will briefly examine these before moving on to a more tractable grouping of variables.

Though it may not be clear to the eye, the six curves in Figure 4-6 do sum to zero. Positive numbers indicate that a reservoir contains more carbon in the TC run than in the control run. As we would of course expect, there is no difference between the
Figure 4-6: Summary of the change in the six reservoirs of carbon between the TC and control runs, with a maximum winter mixed layer depth of 71 m. The storm mixes a large quantity of DIC out of the abyss, but all the entrained DIC and more is consumed in the ensuing phytoplankton bloom, which causes a relative influx of CO$_2$ into the mixed layer ($F_a$). The decrease in abyssal OC after the TC passage is due to the entrainment of the existing PON reservoir into the mixed layer, but as PON sinks out of the mixed layer, a surplus of organic carbon remains at depth. This sinking is also responsible for the persistent DIC deficit in the mixed layer.
two runs until the tropical cyclone passes in May; when that happens, a few hundred mmol m$^{-2}$ of DIC is mixed upward in a single day, and along with it enough nitrate to convert even more than that into OC. This nitrate is very rapidly consumed in a phytoplankton bloom (see also Fig. 4-2(b)), which consumes all the DIC mixed up and more, creating a net DIC deficit in the mixed layer that peaks in June. In spite of the fact that DIC has been mixed upward by the TC, the strong biological activity creates a deficit of DIC in the mixed layer in the TC case relative to the control case, and so there is a reduced air-sea carbon flux out of the ocean.$^4$ Over the course of the summer, fall, and winter, OC leaves the mixed layer due to PON sinking, and soon there is a surplus of organic carbon in the deep ocean in the TC case (Figure 4-4(b)). By the time the wintertime convection has ended, the mixed layer has restored itself to a relatively shallow depth, the situation is somewhat ambiguous: there is a surplus of about 100 mmol m$^{-2}$ of OC in the mixed layer, a much smaller OC surplus in the abyss, a DIC deficit in the abyss, and a DIC deficit in the ML. It is not entirely obvious what to make of this.

Figure 4-7 should clarify matters. We should remind ourselves that any surplus or deficit of mixed layer DIC (but not OC) will tend to be removed by air-sea fluxes – that given a long enough run, we would expect $\Delta DIC_{ML}$ to go to zero, with that anomaly pushed into the $F_a$ reservoir, because the mixed layer temperatures will re-converge (Figure 4-1(b)) and therefore will be driven toward the same equilibrium by air-sea fluxes. Of course, since the lower parts of what I’ve termed the “transient mixed layer” are well-mixed only for a few weeks or months per year, this could take quite some time, so this a significant approximation, but no more so than the one-dimensional approximation upon which our model is built. Also, as previously reasoned, we expect that the organic carbon surplus that has accumulated in the mixed layer will eventually be deposited into the abyss. With these assumptions in hand, our six carbon reservoirs can be reduced to two: carbon that has been or is expected to be exchanged between the atmosphere and the ocean, and carbon that

$^4$Note, however, that in both the TC and control cases, CO$_2$ is being released from the mixed layer in May and June (not shown).
has been or will be expected to be deposited into the deep part of the ocean:

\[ \Delta D I C_{\text{abyss}} + \Delta O C_{\text{abyss}} + \Delta O C_{\text{ML}} + \Delta S_P = - \left( \frac{\Delta D I C_{\text{ML}} + \Delta F_F}{\Delta (\text{ML} + \text{atmospheric carbon})} \right) = \Delta C_{\text{abyss}} \]  

(4.7)

Figure 4-7 shows the time evolution of \( \Delta C_{\text{abyss}} \), and separately shows the change in abyssal carbon and the change in shallow organic carbon that is expected to sink.

Figure 4-7: See Eqn. 4.7. The black curve represents the carbon deficit that is modeled as occurring in the abyss, and the green curve represents the OC surplus in the upper part of the ocean that we believe can only ever sink due to its existence in or near the euphotic zone. Their sum, \( \Delta C_{\text{abyss}} \), is shown in the red curve. The choice to consider shallow organic carbon as ultimately destined to sink changes the result from an upward flux of about 60 mmol m\(^{-2}\) to a downward flux of about 25 mmol m\(^{-2}\).

It is instructive to look at the model behavior when biology is disabled, leaving only the thermal processes and air-sea exchange. This is shown in Figure 4-8. For this run, biology was disabled, but the model was still “spun up” as described earlier.
to allow DIC fluxes from the ocean to the atmosphere stabilize somewhat before starting the run. As we expect, because there are no biological process to reduce the concentration of DIC in the mixed layer, it becomes over-saturated with respect to the atmosphere (as suggested above) and there is an outgassing of carbon dioxide that is still ongoing as the modeled period ends.

Figure 4-8: As Figure 4-6, but with biological processes removed from the model. The organic carbon reservoirs are static, and there is no PON sinking, so these are not plotted. A figure analogous to Figure 4-7 would show that $\Delta C_{\text{abyss}} = \Delta DIC_{\text{abyss}}$; we can therefore say that there has been a net carbon export of roughly 200 mmol m$^{-2}$ out of the abyss and into the mixed layer and atmosphere. We can see in this figure that $\Delta F_a$ is increasing as $\Delta DIC_{ML}$ is decreasing, indicating an enhanced outgassing in the TC case that removes the positive DIC anomaly in the mixed layer; the further decrease in $\Delta DIC_{ML}$ is due to the decreased outgassing tendency post-storm in the TC run, so CO$_2$ that is effluxed at a faster rate in the control run than in the TC run.

Having modeled precisely one storm with and without biological processes, we are presented with the temptation to conclude that biology plays a significant role in the
Figure 4-9: DIC Profiles for the no-biology run; contrast with Figure 4-5. Here, a similar DIC deficit is created at depth, though it is more persistent as no PON remineralize in the deficit region, and the DIC deficit at the surface, cause in the biology run by phytoplankton growth, is replaced with a surplus, due simply to the entrainment of carbon-rich water.

The flux of carbon between the abyss, the mixed layer, and the atmosphere, turning a storm that might have otherwise produced a moderate flux of carbon out of the deep ocean into one that moved a modest amount of carbon downward – at least if one accepts the argument that mixed layer organic carbon will eventually be cycled to lower depths. As we will see, this is a premature conclusion. Next, we will explore a well-documented storm.
4.2 Hurricane Felix, August 1995

Because Hurricane Felix passed directly over Bermuda and the Bermuda Testbed Mooring project, it received considerable attention in the literature (Bates et al., 1998; Dickey et al., 1998). This gives us something to compare our model against. Additionally, because this is the site to which our model is tuned, it makes sense that we might examine what happens when an actual storm passes over.

Bates et al. (1998) used direct measurements of the CO$_2$ concentrations in the upper ocean before and after the hurricane passed over, and combined that with a relationship between the wind speed and the piston velocity to estimate the amount of CO$_2$ efflux caused by the tropical storm. While our model has no mechanism for modifying the piston velocity due to enhanced winds, it stands to reason that even at the standard rate of air-sea exchange, any DIC entrained into the mixed layer by the storm will stay there and be slowly outgassed. This logic breaks down somewhat if we suppose that a large amount of nitrate and a large amount of DIC are entrained at the same time – in that case, since we have no mechanism for enhanced air-sea fluxes, it is conceivable that the carbon that would escape the ocean during the storm could become tied up in OC immediately afterwards if it remained in the mixed layer; if enough OC were created to bring the DIC concentrations from oversaturation to undersaturation, then this could permanently mask the air-sea carbon flux, rather than simply delay it. However, if we look at the nitrate concentration in the upper ocean (Figure 4-10), we know in advance to expect that biology will play a very small role in this storm. Consequently, as long as we can consider the mixed layer carbon and the outgassed carbon together, and as long as we compare a TC run against a control run, then we expect that Equation 4.7 should still hold since the piston velocity only affects the rate of change between two reservoirs that are counted together.

Because direct measurements of the temperature depression are available in the literature, it makes sense to use them. However, the cooling and deepening of the mixed layer is specified in two different ways: Bates et al. (1998) noted that the
Figure 4-10: Climatological Nitrate in upper 100 meters of the ocean, from the World Ocean Atlas (Garcia et al., 2010), annual average. This is plotted on a logarithmic scale. Bermuda is in a region where total nitrate in the upper 100 m is under 150 mmol m$^{-2}$. While having high nitrate quantities does not guarantee strong biological sinking, if e.g. the winter mixed layer depth is deeper than the storm ML depth, low nitrate quantities do ensure that biology cannot play a large role.
temperature depression in the storm’s wake was about 4°C, and Dickey et al. (1998) wrote that warming was observed to depths of about 70 meters. While both these statements are surely true, it indicates an idiosyncrasy in our model: in order to depress the SST by 4°, it must mix to about 90 meters; mixing to 70 meters corresponds to a temperature depression of about 3°. Zedler et al. (2002) estimated from the available measurements that somewhere in the vicinity of a quarter of the heat lost by the mixed layer was advected horizontally rather than mixed down. We will examine runs that mix to both depths in this section.

Using the methodology discussed in Section 4.1, we can calculate $\Delta C_{\text{abyss}}$ in the same way. The first thing that we should note is that this region of the ocean has very low concentrations of nitrate in the upper hundred meters; since the amount of carbon mixed downward is at most equal to the amount of nitrogen mixed upward times the Redfield ratio, this places a sharp limit on the extent to which $\Delta C_{\text{abyss}}$ could surpass zero. Yet, simply running the model and looking at the output, we see that $\Delta C_{\text{abyss}}$ becomes quite large (i.e., more carbon exists in the deep ocean at the end of the TC run than the control run). See Figure 4.2. More strangely still, this change doesn’t take place until five months after the storm passes, even though the biological processes happen quickly, as we saw in the case of Typhoon Chanchu. It seems that something quite different is happening here.

Examining the modeled mixed layer depth (not shown) reveals that the winter time mixed layer is meaningfully shallower in the TC run than in the control run. This is quite unlike what we see with Chanchu and most other storms, where the mixed layer evolution proceeds in essentially the same way in both cases beyond a few weeks after the storm passes. This is a clue, though probably an unhappy one. It becomes clear from examining Figure 4-12 that, in our one dimensional model with no “meridional” heat flux, this is a region where the mixed layer becomes quite deep in the winter, and therefore a large efflux of carbon occurs. This is not the worst assumption – remember that we have recreated the winter observations at this very site with a reasonable degree of accuracy (Figure 3-2) – but because of the simplistic way we have modeled CO$_2$ outgassing, and because the ocean here is oversaturated.
with respect to the atmosphere, a large deepening of the mixed layer results in a large outgassing of carbon. However, in the TC run, the heat that is deposited at depth slows the winter time mixed layer deepening, because until that anomaly is removed, the model does not have to deepen the mixed layer as much to maintain the same surface heat flux; in addition, this warm anomaly will have less carbon to ventilate out. The combination of these two effects is enough to explain the large $\Delta C_{\text{abyss}}$.

This curious result does not depend on any biological processes at all. Though it is easily understandable in the context of the model framework, it would probably not apply to a three-dimensional ocean, because a warm anomaly at depth would spread out over quite a large area (the isopycnal diffusivity is many orders of magnitude higher than the diapycnal diffusivity), and so would not be able to inhibit the deepening of the mixed layer by very much at any location. The idea of partitioning of carbon into shallow and deep reservoirs is based on the idea that storms do not change the winter time mixed layer depth very much, and our model lacks the breadth (quite literally) to properly diagnose the extent to which the winter time mixed layer would change due to storm mixing. The final value of $\Delta C_{\text{abyss}}$ must therefore be treated as spurious. It will be shown in the next section that such errors are easy to diagnose in our modeling framework, and are relatively uncommon.

Finally, we should note that, although the model produces an end-of-winter value for the carbon flux out of the abyss that is almost certainly deeply flawed, it does produce a carbon flux from the mixed layer to the atmosphere in the immediate aftermath of the TC passage that is roughly consistent with that observed by Bates et al.. Recalling that we expect any air-sea exchanges that happen in our model to occur more slowly than those caused by a real tropical storm, as we have no mechanism to relate the piston velocity to the wind speed, we see in Figure 4-12(b) that a relative surplus of DIC accumulates in the mixed layer immediately after the storm, and that it is slowly removed by air-sea fluxes until about 200 mmol m$^{-2}$ of extra CO$_2$ has been released in the TC case than the control case. This is, in fact, about 75% more than was seen (in two days) by Bates et al.. If we decrease $\Delta T$ to 3°C, we find that the accumulated air-sea exchange of carbon over the months
following the storm is approximately equal to their calculation. So, in spite of our model's problems calculating $\Delta C_{\text{abyss}}$ when there is a large release of heat in the winter time, we can still be somewhat confident that in this case it did a reasonable job of mixing carbon up from the abyss to the mixed layer.

If this sort of result were a frequent occurrence, we would have cause for concern; it would suggest that our model, and probably all one-dimensional models, are inadequate to the task. Fortunately, there are a few simple criteria that we can use to diagnose these anomalous storms that produce erroneous results. First, these are the only storms that produce large results (of either sign) when the storm mixed layer deepening is less than the winter mixed layer deepening, and second, they are the only storms where the winter time mixed layer depth is very different in the TC case than the control case. As we will see in the following section, these storms are rare.

### 4.3 A Survey of Tropical Storms from 2006

Using the same techniques described in Typhoon Chanchu, a semi-automated analysis of all the storms that occurred in both hemispheres in calendar year 2006 was undertaken. For each storm, the location along the storm path where $\Delta T$ was the largest was taken as a representative sample. This naturally biases the results toward large magnitudes; while it would be possible to analyze each point along the storm's track, it will be shown that this is not really necessary to get an understanding of the net effect of storms on the carbon content of the ocean.

Of the 95 storms that were in the database for that year, ten were rejected because their maximum $\Delta T$ was under 0.1 K; four of them had $\Delta T < 0$, i.e. the region of storm passage was warmer in the week following the storm than in the week before it. This is surely due to lateral effects that have nothing to do with the storm; since the most negative maximum value of $\Delta T$ found in the 95-storm database was -0.3 K, we should assume that an error of at least that magnitude is possible with any of the other locations. The other six were mostly tropical depressions or very short-lived storms; it was assumed that their temperature depression was not sufficiently different
Figure 4-11: Change in carbon for each reservoir, Hurricane Felix, with specified mixing-induced cooling $\Delta T = 4^\circ C$, as Fig. 4-6. The value seen for $F_a$ can be argued to be roughly consistent with the air-sea $CO_2$ flux observed in Bates et al. (1998), though it is almost twice as large and takes much longer to manifest itself. The very large values eventually seen for $\Delta DIC_{abyss}$ are due to a decrease in winter time mixed layer depth, and therefore outgassing, in the TC case, due to TC-caused heating at depth decreasing the amount of deep water mixed up in winter; this is almost certainly unrealistic. Note that the very small values of $\Delta OC$ are expected, as this region of the ocean has a very deep nutricline, and so has essentially no possibility for biological stimulation. If we constructed a plot like Figure 4-7, we would see that $\Delta C_{abyss}$ very closely follows $\Delta DIC_{abyss}$ since there is very little biology.
Figure 4-12: Change in Dissolved Inorganic Carbon in Hurricane Felix, with specified mixing-induced cooling $\Delta T = 4^\circ C$, as Fig. Note that while Organic Carbon (not shown) is not zero, its residual between the TC and control run is more than an order of magnitude smaller than DIC, as Figure 4.2 indicates. These figures make it clear that the large positive value of $\Delta DIC_{\text{abyss}}$ seen in Figure 4.2 is due to a large amount of DIC ventilated in the control run that is left out of reach of the mixed layer in the TC run because the mixed layer does not reach as deeply in the winter.
from zero to comfortably attribute any effect to the storm. Additionally, one was rejected for having a $\Delta T$ that would have caused mixing to an extremely deep depth; presumably this is also due to advective cooling that happened in reality but not the model. While discarding these storms cannot really be justified, we can at least reasonably expect the storms with a negative or small $\Delta T$ to have a final behavior similar to those with a rather-small, positive $\Delta T$, of which there is no shortage in the record, and that the rejected storm with a large $\Delta T$ was not the storm that caused the largest inferred temperature depression, rather, the storms with larger temperature depressions occurred over more strongly-stratified regions of the ocean. So while our technique cannot handle every storm, we can at least look at a broad, representative sample. Not more than five storms (all in the east Pacific or west Indian oceans) are found in regions of the ocean that have much probability of containing iron-limited ecosystems (Moore et al., 2002), and, because all the storms in questionable areas happened to be weak in 2006, all are associated with near-zero $\Delta C_{\text{abyss}}$ in this model. They have therefore not been removed from the results.

We find that the storms can be broken down into four categories: (I) those that effect very little change in $\Delta C_{\text{abyss}}$ because the depth to which they mix is less than the depth reached by the wintertime mixed layer, (II) those that mix below the end-of-winter mixed layer depth, but do not entrain enough nitrate to offset the carbon that they mix up, causing a nontrivial negative $\Delta C_{\text{abyss}}$ and removing carbon from the deep ocean (III) those that mix below the end-of-winter mixed layer depth and do mix up a meaningful amount of nitrate, which allows biological sinking to offset the outgassing tendency, (IV) and those that produce a large value of $\Delta C_{\text{abyss}}$ due to the heating of the ocean reducing the wintertime outgassing, as was modeled in the case of Hurricane Felix. As discussed, this final result is believed to be erroneous; in 2006 there were only two storms that fell into this category. Typhoon Chanchu, discussed previously, is one of the stronger examples of storms falling into the third category.

Figure 4-13 shows the calculated end-of-season $\Delta C_{\text{abyss}}$ for each of the storms in 2006 that was not excluded for the reasons listed above. The two storms with
relatively shallow mixing but a large $\Delta C_{\text{abyss}}$ are off the scale. Slightly under half the storms have $ML_{TC} < ML_{\text{winter}}$, and therefore nearly zero end-of-winter $\Delta C_{\text{abyss}}$. A handful of storms do mix up a locally-significant amount of carbon from the deep ocean, and for most of them biological sinking offsets this to some extent, though not always by much (not shown). Only three storms (other than the two probable errors) manage to create $\Delta C_{\text{abyss}} > 25 \text{ mmol m}^{-2}$; while most storms move very little carbon at all, our model shows that moving it upward is easier than downward.

Figure 4-13: $\Delta C_{\text{abyss}}$ vs mixed layer deepening for 84 storms in calendar year 2006. The $y$-axis corresponds to the final value of the red line in Figure 4-7, and indeed Typhoon Chanchu can be seen at $(1.75, +25)$. Not shown, Cyclone Daryl and Hurricane Isaac, both of which have $ML_{TC} < ML_{\text{winter}}$ and large $\Delta C_{\text{abyss}}$ due to TC-induced heating causing a decrease in winter time mixed layer depth, as in the case of Hurricane Felix.

Finally, we can take the end-of-winter $\Delta C_{\text{abyss}}$ values and multiply them by a characteristic storm size,\textsuperscript{5} and come up with an estimate of the total carbon flux

\textsuperscript{5}This is based on the length of the storm track multiplied by a constant characteristic diameter of
between the deep ocean and the mixed layer/atmosphere. If we discard the two storms presumed to have Felix-type errors and their large positive $\Delta C_{\text{abyss}}$ values, and if we accept the previous argument that organic carbon remaining near the surface will ultimately sink, we find that tropical storms are responsible for a net efflux of about 0.04 petagrams of carbon per year worldwide; if we assume that organic carbon remaining in the mixed layer will not sink, this value increases to 0.07 PgC. Given that, in the preindustrial world, about 70 PgC of carbon were exchanged between the atmosphere and the ocean (Watson and Orr, 2003), we have found that TC-induced carbon flux is very small indeed.

200 km. It is, in principle, possible to make a better estimate of mixing radius based on the radius of maximum winds, but this is somewhat poorly tabulated, and further, a relationship between $R_{\text{MW}}$ and the mixing radius would have to be found. In any case, it will be shown not to matter much.
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Chapter 5

Summary and Conclusions

We have constructed a model that quantifies the role that tropical cyclones play in moving carbon into and out of the deep ocean, and we have found that the amount of carbon moved is about four orders of magnitude smaller than the natural background flux. While there are several ways where our model could be improved – most notably with regard to the way the storm mixing depth is determined, though being able to specify a meridional heat flux in a rational manner would also improve things somewhat – it does not seem that any of these changes would alter the modeled flux of carbon much in either direction, since what we care most about is the winter time mixed layer depth and the amount of nitrate and DIC entrained. Finding the winter time ML depth is not difficult even with a crude model, since it depends only on static instability, (i.e. the ocean heat content and the air temperature), and our estimation of the nitrogen and carbon content of the ocean is based on the best data available. While our 1-dimensional model may overestimate the ML deepening caused by the storm, the slope of the N and DIC concentrations with depth is shallow enough that it certainly could not make a difference of an order of magnitude, much less two! We have, in a sense, reached a definite negative conclusion. But still, that conclusion deserves to be recapitulated.

With this model, we have found that tropical cyclones can have three possible effects on the distribution of carbon in the ocean: they can cause an upward flux of carbon due to the entrainment of cold, carbon-rich water into the mixed layer, they
can cause biologically-mediated carbon flux of carbon from the mixed layer to the deep ocean, with the mixed layer negative anomaly being removed by air-sea fluxes, or, if the TC mixes to a shallow depth relative to the winter mixed layer maximum, they can do nothing at all.

There are two reasons why storms that mix to a shallower depth than the natural winter time mixed layer have very little chance of causing carbon to sink. First, they entrain very little nitrate, as the nutricline generally lies near or below below the depth of the winter time mixed layer, but more importantly, any nitrate that they do mix upward would have come up in the winter anyway, and is no longer available if it was mixed upward in the summer or fall. The seasonality of the biological cycle may be altered somewhat, but the net effect is unchanged. As the depth of the storm mixing approaches the winter time mixed layer depth, a modest net carbon flux upward from the abyss can be induced, on the order of 10–30 mmol m$^2$; this is because the winter mixed layer is at its deepest for only a short time, and so a brief TC-induced ventilation will allow some DIC to escape but still leave plenty to be mixed upward the following winter.

When storms do mix deeper than the wintertime mixed layer depth, the biological effect and the thermal effect compete wherever the nutricline is shallow, but the thermal effect nearly always wins; in regions where the nutricline is very deep, there is no contest. However the efflux of CO$_2$ that can be attributed to storms even in several dozen cases a year where carbon is mixed upward that otherwise would not be is still quite modest when one considers the relatively small area of the ocean that is mixed by TCs. Still, while the numbers involved are small, the sign is unambiguous; while the entrainment of cold water does not remove much carbon from the mixed layer, it is still far more than biological sinking adds to it.

This conclusion agrees with the very recent work of Lévy et al. (in press); their model, which was phrased in three dimensions, had an understandably coarser resolution, and studied many more years of storms, reached a comparable conclusion: while storms do remove carbon from the ocean, nearly all of that would escape anyway.
Appendix A

Calculating non-steady winds from a reanalysis

When running a mixed layer model, one quickly runs into issues regarding the maintenance of a sufficiently-deep mixed layer. When forced with the slowly-varying winds that one might find in either reanalysis data (which provides one sample every three or six hours) or directly from QuikSCAT data (which provides roughly ten observations per week), the model had a very strong tendency to generate a summer mixed layer that was far too shallow at the BATS site, though if initialized with a realistic mixed layer it could preserve it. Combining Equation 2.4 with the observation that the deepening of the mixed layer in the winter due to surface cooling was much more adequately captured, this can be taken as a strong indication that the flux of momentum from the atmosphere to the ocean is too small. Some experimentation revealed that the summer mixed layer could indeed be pushed to a greater depth by adding a stochastic multiplier to the mean wind, but this is solution is impossible to defend on any physical basis other than the fact that it produces better results. That such a modification might be necessary is unsurprising, since ocean winds are not steady (though they are much less gusty than the surface winds over land), but the NCEP reanalysis gives us no guidance about how the winds might vary over the course of the forecast interval.

On the other hand, the ERA-interim reanalysis provides a single additional piece
of information that can be very useful. In addition to providing mean $U$- and $V$-
component winds at 10 meters, a maximum three-second gust predicted during the
forecast period is also provided. This gust is provided as a byproduct of the model’s
boundary layer scheme, and can therefore be safely said to be as accurate as the
vertical turbulent fluxes – the current implementation of the ECMWF model attempts
to include effects from surface friction, stability, and deep convection.¹ This still
represents a very convenient place from which to build. If we can safely assume
the wind speeds conform to a distribution, then the knowledge of the mean wind
and maximum gust is enough to allow us to generate pseudorandom winds that are
neither steady nor completely arbitrary.

If we want to construct a wind distribution that is constrained by both the mean
value and the expected maximum gust,² we will need a two-parameter distribution.
The one-parameter Rayleigh distribution, which can be shown to describe the mean
wind speed of any wind whose orthogonal components are independent, uncorrelated,
and each have mean 0, is clearly not adequate for this purpose; it can be fit to the mean
wind or the maximum, but not both.³ Two of the most commonly-used continuous,
two-parameter distributions defined over the nonnegative numbers are the Weibull
distribution and the Gamma distribution; both will be examined for applicability.
There exists a substantial body of literature that supports the idea that the wind
speeds over the ocean conform to a Weibull Distribution; it has been shown that this
distribution can be fit to wind data with sampling frequencies from one month down
to ten minutes. See for example Pavia and O’Brien (1986), where ship observations
were used to support this idea. Extending the use of this distribution down to a three-
second sampling interval would be hard to justify if the data in question were samples
from a single anemometer existing inside a turbulent cascade, but since the three-
second gusts provided by the ERA-Interim reanalysis are representative of a friction
parametrization representing the entire $(0.703^\circ)^2$ box, it is at least not obviously

¹See http://www.ecmwf.int/research/ifsdocs/CY36r1/PHYSICS/IFSPart4.pdf Section 3.11.4
(Dee et al., 2011).
²That is, the gust expected to occur in one three-second interval every three hours, which corre-
sponds to a p-value of $1 - \frac{3\text{ sec}}{3\text{ hrs}} = 0.99972$
wrong. Though the Gamma distribution has fallen out of use for representing low-frequency winds, the winds resulting from assuming a Gamma distribution will be shown as well.

The Weibull distribution is described by two parameters, the scale parameter $\alpha$ and the shape parameter $\beta$,\(^4\) and has probability density function $w$ and cumulative distribution function $W$ (Ross, 1998):

$$w(x) = \frac{\beta}{\alpha} \left(\frac{x}{\alpha}\right)^{\beta-1} \exp\left\{-\left(\frac{x}{\alpha}\right)^{\beta}\right\} \quad (A.1)$$

$$W(x) = \int_0^x w(t)dt = 1 - \exp\left\{-\left(\frac{x}{\alpha}\right)^{\beta}\right\} \quad (A.2)$$

Where $\{x, \alpha, \beta\} \geq 0$. Note that the previously-discussed Rayleigh distribution is a special case of the Weibull distribution with $\alpha = \sigma \sqrt{\pi / \Gamma(\frac{3}{2})}$, $\beta = 2$. It can be shown that the mean of this distribution is

$$\bar{x} = E(x) = \int_0^\infty xw(x)dx = \alpha \Gamma \left(1 + \frac{1}{\beta}\right) \quad (A.3)$$

Since we know the mean ($\bar{u}$) and have an expected maximum ($u_{max}$) and associated cumulative probability ($p = W(u_{max})$), we can use equations A.2 and A.3 to solve for $\alpha$ and $\beta$. These equations do not lend themselves to simultaneous solution, but each combination of $\alpha$ and $\beta$ gives rise to a unique distribution, so we can safely solve for these variables iteratively:

$$\alpha = \frac{\bar{u}}{\Gamma \left(1 + \beta^{-1}\right)}$$

$$\beta = \frac{\ln(-\ln(1-p))}{\ln \left(u_{max}/\alpha\right)}$$

\(^3\)It should be noted that Monahan (2006) derives a wind speed distribution from the assumption that fluctuations in orthogonal winds are independent and uncorrelated, but allowing the wind to have non-zero mean and, due to the inability of a Gaussian distribution to capture the observed skew in the distribution of wind speeds, permitting non-Gaussian wind components. It is shown that the distribution derived in that paper captures the real statistics of observed winds much better than any discussed here could hope to, but because it utilizes higher-order statistical moments than could be inferred from the boundary-layer scheme, we shall have to utilize a less-accurate approximation.

\(^4\)[\(\alpha, \beta\)] are elsewhere called [\(\lambda, k\)] and [\(\sigma, \beta\)].
While the above converges quite quickly, numerical solvers are also a possibility.

For the Gamma distribution, the PDF is given by (Ross, 1998):\(^5\)

\[
g(x) = x^{\beta-1} \frac{e^{-\frac{x}{\alpha}}}{\alpha^\beta \Gamma(\beta)}
\]

And the CDF and expected mean can be calculated as above. Here the parameters must be obtained through a numerical solver due to the presence of the lower incomplete gamma function in the CDF, but again each set of parameters produces a unique distribution, so finding the solution is not difficult.

Figure A-1 shows the resulting PDFs and CDFs for the case that \(\bar{u} = 5, u_{\text{max}} = 20, p(u_{\text{max}}) = 1 - \frac{1}{3600}\); the maximum gust has been exaggerated for effect – on average, maximum gusts over the ocean are well under double the mean wind speed in the ERA-Interim reanalysis. They illustrate the general point that, for equivalent constraints, the Weibull distribution will produce a larger standard deviation and a smaller skew (i.e., the distribution will be less right-skewed) than the Gamma distribution. If one generates a large number of random variables from these distributions, the Weibull distribution will have more numbers below the mean than the Gamma distribution, but the numbers above the mean will be larger to compensate. As discussed in the literature, this is preferable when the sampling period is longer than a few minutes, though it may potentially not be when it is very small. However, because our distribution is being used to generate three second winds, and because the model runs with a time step between five and thirty minutes, binned averages will be used, which will smooth the data considerably. For the example used here, generating three hours of pseudorandom data at three second intervals produced a maximum five-minute average of 5.9. Over most values actually seen, this technique does not create a very large variance in the wind speed, but the occasional strong gusts it produces do help increase the dynamic instability of the mixed layer.

\(^5\)Note that I have again used \(\alpha\) and \(\beta\) to denote the scale and shape parameters, but these parameters are not trivially convertible to equivalent Weibull parameters – the Gamma distribution is not a special case of the Weibull distribution.
Figure A-1: Gamma and Weibull distributions, each tuned to produce the same mean and the same \( p \)-value for the maximum gust. Note that for this choice of mean and expected maximum, both distributions produce very similar medians, however the Weibull distribution is quite a bit wider.
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Bibliography


