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Wave interactions – the evolution of an idea

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This essay gives a personal and possibly incomplete history of the way in which the simple idea of weak resonant wave interactions grew to find application to a variety of phenomena in several contexts. The development involved incremental steps by many people in the past twenty years, gaining simplicity with maturity. The final stage seems to be approaching when the limits of usefulness of the idea are beginning to become apparent.

It is unusual in fluid mechanics, and perhaps in science generally, for a body of theory to spring in essence complete from the mind of one person. Much more characteristic is a process of evolution over time with sparks of insight contributed by first one, then another, interspersed with the patient examination of what seem in retrospect to be minutiae, discussion of particular cases, and sometimes even errors that lead to further understanding. Twenty years later, the whole thing may seem so obvious and so transparent that one wonders why it took so long and such pains to understand something that can now be explained in only a few pages. There are, of course, exceptions, or instances that seem to be exceptions. The theory of sound generation by turbulence in subsonic flow was set forth by James Lighthill in two monumental papers (1952, 1954) that provided the basis for much future work including extensions to supersonic flow, but even this did not arise without the treading of tortuous paths. Lighthill's efforts to construct the theory are recounted in an earlier unpublished report (1950) to the Aeronautical Research Council in London, in which one can see the basic ideas taking shape. One of the most illuminating parts of this report is § 3, entitled 'An erroneous approach'. Yet it is a very plausible one, and only by understanding why it is erroneous did the correct theory emerge.

The resonant interactions among waves provide an example that is perhaps more characteristic – a group of phenomena in which the basic ideas are fairly simple and to which many people contributed. It involved steps that were important at the time and hard to achieve yet which, with the passage of time, seem only small parts of a pattern whose overall outline is now fairly clear. Some pieces are still fuzzy but it may be of interest to trace the way in which our present understanding was painstakingly won.

It is perhaps surprising to remember that, as late as 1950, 'waves in fluids' usually meant surface waves and that the body of theory available was predominantly concerned with infinitesimal waves in which the surface boundary conditions can be linearized. There had been, of course, the classical works on finite-amplitude solitary waves in shallow water and the nineteenth-century works by Stokes on the profile shape and phase speed of a train of permanent waves in deep water. Some pioneers such as Long (1953*a*, *b*, 1954, 1955) were experimenting with internal gravity waves and developing a theoretical description of the phenomena that they measured, but there was little observation of these in the atmosphere or the ocean and little notion of the variety and richness of their behaviour that would later be found. Surface waves on deep water were regarded as essentially linear and spectra were constructed by the superposition of linear waves – nonlinear effects were generally thought to be restricted to the relatively trivial distortion from a sinusoidal profile described by Stokes so long ago.

For me, the genesis of the whole thing was, I suppose, the opportunity in the 1950s to work as a young research student at Cambridge. G. I. Taylor was active; George Batchelor, Alan Townsend and Ian Proudman were all in the forefront of turbulence research. The atmosphere was intense and pervasive – the very difficult problems of turbulence were nonlinear and nonlinearity meant energy transfer among different scales of motion. When later I became interested in surface waves as the result of a seminar by Fritz Ursell, I found that there did not seem to be anything in the wave literature analogous to this. The nonlinearities in the governing equations for waves might indeed be weak, not strong as in turbulence, but they were there. Did they provide energy exchanges among different waves in some way similar to that among different Fourier components in a field of turbulence?

Had I been more literate in the mainstream of ordinary physics, I would have known that a similar question had been considered by Peierls in 1929 in connexion with secular interactions between random lattice vibrations, but I didn't, so I undertook the task of laboriously examining the interactions among two gravity wave trains with arbitrary wavenumbers $\mathbf{k}_1, \mathbf{k}_2$ on the horizontal plane, to see whether and under what conditions they would transfer energy to build up a third component. Even to the second order in the Stokes expansion, the algebra was extraordinarily tedious (a harbinger of things to come) and the results were entirely negative – to this order there only appeared the bound harmonics at wavenumbers $\mathbf{k}_1 \pm \mathbf{k}_2$, $2\mathbf{k}_1$, $2\mathbf{k}_2$, whose amplitudes remain forever small compared with those of the primary waves. The mathematics used was very primitive – an embarrassment today – but sufficient to show what I did not want to find: no continuing energy transfer for any configuration, just bound harmonics analogous to those of Stokes.

But there had to be some sort of energy transfer, so I was forced to the third order of approximation where the algebra was worse. There I found what I was looking for – if there existed two wavenumbers with $2\mathbf{k}_1 - \mathbf{k}_2 = \mathbf{k}_3$, say, and frequencies such that $2\sigma_1 - \sigma_2 = \sigma_3$, say, in which, for each, $\sigma_i = (gk_i)^{\frac{1}{2}}$, then a steady-state solution for the triad did not exist. The amplitude of the third wave a_3, if initially zero, would grow linearly in time. It was like the resonant excitation of a linear oscillator – the two primary waves produce a perturbation at the wavenumber $\mathbf{k}_3$ and, if the frequency of this perturbation corresponds to the natural frequency of a free wave with this perturbation wavenumber, then the amplitude of the response would grow linearly. The paper describing this work (1960) was extremely tedious and stopped as soon as the fact of the energy transfer was found in this special case; it was still far from giving the capability of describing the energy transfer in a random field of waves such as that found at sea.

The algebra had already been daunting and it seemed that, if there were to be any hope of achieving this goal, the formulation had to be simplified. An attempt was made (1961) to find exact expressions for such things as wave kinetic energy density in terms of the surface properties, but this was not in the end fruitful. The next steps were to be taken elsewhere.

At about this time, Sir George Deacon organized a meeting that was held at Easton, Maryland, to discuss ocean waves and I was invited to present my paper. The reaction from some well-known and senior people in the field was, to my astonishment, vigorous and hostile, expressing the flat disbelief that different wave components could exchange energy at all. My mathematics was certainly very primitive but Michael Longuet-Higgins, to whom I had sent a copy of the manuscript, offered his cautious and most welcome support and Klaus Hasselmann, whom I met there for the first time, was working quite independently along the same lines but with greater generality, so the sharp, intense encounter ended with a stand-off. It had been my first experience of scientific acrimony, and not a pleasant one.

Hasselmann had been trained as a theoretical physicist and his work, embodying a more powerful scattering theory formulation, appeared in 1962 and 1963. Already it was apparent that resonant interactions among surface gravity waves occurred among certain sets of four wavenumbers for which simultaneously

$$\left.\begin{aligned}\mathbf{k}_1+\mathbf{k}_2 &= \mathbf{k}_3+\mathbf{k}_4,\\ \sigma_1+\sigma_2 &= \sigma_3+\sigma_4\end{aligned}\right\} \tag{1}$$

and $\sigma_i = (gk_i)^{\frac{1}{2}}$; my 1960 paper had considered only the special case when $\mathbf{k}_1 = \mathbf{k}_2$. This work of Hasselmann's provided the basis for what I had tried to do without success – the calculation of the transfer of energy among different wave components at sea. The calculation was, however, still cumbersome and difficult; simplifications would appear only much later.

One positive result of the Easton meeting was Michael Longuet-Higgins' conviction that these resonant interactions really should be demonstrated experimentally and in 1962 he suggested how it should be done. He and Norman Smith undertook the experiment (as did we at Johns Hopkins), generating two wave trains from adjacent sides of a square tank, adjusting the frequency ratio about resonance and measuring the interaction product. We started from scratch by building the tank and the experiments at the (then) National Institute of Oceanography were finished long before ours. Nevertheless, they graciously held up the publication of their results for a couple of years until our experiments were completed in order that the two accounts could be published simultaneously (Longuet-Higgins and Smith 1966; McGoldrick, Phillips, Huang and Hodgson 1966). The results were clear – the resonant interactions did exist and the growth rates were close to those calculated.

By this time, though, there was not much doubt that these effects were real. In 1962, David Benney at M.I.T., using the then new techniques for analysis of slowly varying wave trains, had derived the complete set of interaction equations in the form (slightly corrected)

$$\dot{a}_1 = ia_1(g_{11}a_1a_1^* + g_{12}a_2a_2^* + g_{13}a_3a_3^* + g_{14}a_4a_4^*) + ih\sigma_1 a_2^* a_3 a_4 \tag{2}$$

together with similar equations for the rates of change of the other component amplitudes a_2, a_3 and a_4. The g_{ij} and h are real coefficients of the interaction depending on the configuration $\mathbf{k}_1, \ldots, \mathbf{k}_4$, but whose calculation required so much labour. Also in 1962, Michael Longuet-Higgins and I had realized that the degenerate form of (1) in which $\mathbf{k}_1 = \mathbf{k}_3$ and $\mathbf{k}_2 = \mathbf{k}_4$ represents a phase velocity modification that occurs when any two wave trains run together. This is immediately obvious from Benney's equation (2) being represented by the first group of terms on the right-hand side giving a rate of change of a_1 in quadrature with a_1. The first term in the group represents the fractional increase in phase velocity of $\frac{1}{2}(ak)^2$ found by Stokes in 1847. Benney showed also that the set of equations possess energy and momentum integrals as well as an integral specifying the partition of aa^*/σ – the wave action that has since become a pivotal quantity in studies of wave–current interactions.

If gravity waves on deep water interacted in this way, it was natural to enquire what modifications would result with the additional influence of capillarity, and Larry McGoldrick's thesis was devoted to this question. It appeared immediately that although there are no solutions to the set of equations

$$\left.\begin{aligned} \mathbf{k}_1 &= \mathbf{k}_2 + \mathbf{k}_3, \\ \sigma_1 &= \sigma_2 + \sigma_3, \end{aligned}\right\} \tag{3}$$

where, for gravity waves, $\sigma = (gk)^{\frac{1}{2}}$, there *are* solutions for gravity-capillary waves in which $\sigma = (gk + \gamma k^3)^{\frac{1}{2}}$, where γ represents the surface tension divided by density. Resonant interactions were found among gravity waves only at the third order, among sets of four waves, but for gravity-capillary waves they appear at the second order among three wave components. The evolution equations for the wave amplitudes are also much simpler:

$$\left.\begin{aligned} \dot{a}_1 &= ih\sigma_1 a_2 a_3, \\ \dot{a}_2 &= ih\sigma_2 a_3^* a_1, \\ \dot{a}_3 &= ih\sigma_3 a_1 a_2^*. \end{aligned}\right\} \tag{4}$$

There were three important consequences of this simplification. First, the algebra involved in the analysis was much simpler. More importantly, the interaction time scale is shorter, (wave slope)$^{-1}$ times the wave period rather than (wave slope)$^{-2}$ for gravity waves, so that the manifestations of the interactions are more rapid. Finally, the frequencies involved with capillary-gravity waves are higher than with pure gravity waves so that experimentation is simpler and possible with much smaller apparatus. McGoldrick's thesis was published in 1965 and subsequent work by him (1970, 1972), by Kim & Hanratty (1971) and Nayfeth (1971) has demonstrated many fascinating phenomena including subharmonic resonances and the near-resonance of short capillary waves riding on larger gravity waves.

The next conceptual step was taken by Keith Ball who described it to me one day in 1963 while we were riding in the train from London to Cambridge. He had realized that, in a system capable of supporting more than one type of wave motion, energy could be transferred among the different types in precisely the same way. He was concerned specifically with a two-layer system with a free surface and an internal interface separating fluids with densities ρ above and $\rho + \Delta\rho$ below. He pointed out that if two surface waves have wavenumber magnitudes (and hence frequencies) that are almost the same, then the small difference frequency can match that for an *internal* wave whose wavenumber is the vector difference of the two surface wavenumbers. A triad of three surface waves could not undergo resonant interactions, but one consisting of two surface waves and one internal wave could. Ball's paper was published in 1964; though pointing out that these interactions could provide a source for atmospheric and oceanic internal waves, he developed his calculations in greatest detail for the simplest case when both layers are shallow.

Oceanic internal waves were beginning to be observed then, following the pioneering measurements of Haurwitz, Stommel and Munk (1959), but it was far from clear how they were generated. The question of whether the energy flux from surface waves to internal waves by resonant interactions was significant or not, was of obvious oceanographic importance and Ball's (1964) work was followed shortly by that of Steve Thorpe (in his 1965 Ph.D. dissertation), Leonid Brekhovskikh and his group in the Soviet Union (1972) and finally Watson, West and Cohen (1976) in California. This last analysis, using a Hamiltonian formulation and the now familiar apparatus of statistical mechanics, predicted that, under characteristic oceanic conditions, the energy flux to internal waves by this process is indeed a substantial fraction of the total dissipation rate from internal waves as estimated earlier by Chris Garrett and Walter Munk (1972).

By 1970 or so, it had become apparent that another manifestation of this same interaction occurs when a train of short surface waves encounters an internal wave of much longer wavelength on an underlying thermocline, and suffers modulations as a result. This phenomenon had been observed in the Anadama Sea by Perry and Schimke in 1965 and one facet of interest is that the dynamics can be viewed in two different and complementary ways. From the point of view of resonant wave interactions, the interaction of a surface wave with wavenumber k with an internal wave with wavenumber $k_i = \delta k \ll k$ will generate a new surface component with wavenumber $k+\delta k$ in a resonant manner provided the frequency difference $\delta\sigma$ of the surface waves matches the frequency σ_i of the internal wave. Thus in (3), $k_i = \delta k$, $\sigma_i = \delta\sigma$ and the phase velocity of the internal wave

$$c_i = \sigma_i/k_i = \delta\sigma/\delta k = c_g, \tag{5}$$

the group velocity of the surface wave. The growth of this new surface wave component at a wavenumber different by δk from that of the one initially present, leads to an increasing groupiness of the surface waves with a group wavelength equal to that of the internal wave. In most regions of the ocean, the phase speeds of low mode internal waves on the thermocline range up to 50 cm/sec or so, and the condition (5) can be satisfied only by relatively short surface wave components. The modulations produced in this way appear as periodic bands with varying roughness of the sea surface and are seen frequently in satellite imagery of coastal waters.

The phenomenon can properly be viewed in terms of resonant interaction theory only when all the wave slopes are small and when the surface currents produced by the internal wave train are small compared with its phase speed. This condition is unduly restrictive. It is more rewarding usually to consider the process as one in which the short surface waves interact with a slowly varying, propagating current pattern U, supposed given and produced by the internal wave. It is then possible to use the conservation laws developed by Francis Bretherton and Chris Garrett (1969) and others for wave trains in slowly varying media. The restriction on the magnitude of the surface current implicit in resonant interaction theory is then removed but, on the other hand, we lose information about the modification of the internal wave as the result of the interaction. (It could possibly be recovered by a careful analysis, not yet done.) In a frame of reference moving with the internal wave, the flux of action in the surface wave train is

$$(\mathbf{U}+\mathbf{c}_g-\mathbf{c}_i)\mathscr{A},$$

where $\mathscr{A}$ represents the wave action density; if the internal and surface wave speeds satisfy (5) even approximately, then variations in the magnitude and direction of the surface current U induced by the internal wave can reverse the direction of the action flux and lead to local concentrations and reductions in the surface wave action density. If the phenomenon is viewed in this way, it is evident that a *train* of internal waves is not necessary to influence the surface wave pattern – a single current pulse associated perhaps with an internal soliton can produce the action flux divergence and a pulse modulation in the surface wave. The most complete early analysis of this problem by Ann Gargett and Blyth Hughes in 1972 was complemented by field observations made in the Strait of Georgia, British Columbia; comprehensive laboratory measurements in 1974 by John Lewis, Bruce Lake and Denny Ko placed the relation between observation and theory on a good quantitative basis.

In the meantime, it had become clear that resonant interactions could provide not only a source of internal wave energy and a mechanism for surface wave modification, but also a means for the redistribution of energy among different modes or wavenumbers in the spectrum of internal waves in a continuously stratified fluid. The interaction conditions (3) do admit solutions for small-scale internal waves for which $\sigma = N\cos\theta$, where N is the stability frequency and θ the inclination of the wavenumber vector to the horizontal. Interaction diagrams were illustrated in the first edition of *Dynamics of the Upper Ocean* (1966). Experiments by Seelye Martin, Bill Simmons and Carl Wunsch (1972) in a large stratified tank alongside the docks at the Woods Hole Oceanographic Institution showed the formation of new internal modes following mechanical stimulation of just one. The first systematic calculation, however, was undertaken by Henry McComas in his Ph.D. dissertation of 1975, but not published until 1977. The celebrated but empirical Garrett–Munk spectrum (1972) for oceanic internal waves had been very successful in synthesizing a variety of different types of internal wave measurements, including spectra of horizontal and vertical traverses, frequency spectra and coherences, and McComas' calculations showed that the Garrett–Munk spectrum was indeed quite close to a state of statistical equilibrium under the net effect of the wave interactions. This gave a substantial impetus to the belief that the internal wave spectrum was a consequence primarily of the balance among these interactions, rather than being limited by a 'saturation' process involving sporadic local instability somewhat analogous to that in surface waves under wind with local wave breaking. But all was not entirely well. With the energy levels of the Garrett–Munk spectrum, the time scales of certain of the interactions were not long compared with the wave period – the interactions were not weak as visualized in the simple theory. It had taken us quite a long way, but something better was needed. We will pick up this thread again later.

Another starting point came from experiment in an unexpected way. Brooke Benjamin and Jim Feir had been trying to produce a uniform train of rather steep surface waves in a long laboratory tank without much success – the wave train always degenerated into a series of wave groups. This troublesome phenomenon had indeed been known for some time by engineers who operate long ship-testing tanks and they had usually attributed it to an inadequacy in design of the plunger that generated the wave train. Benjamin and Feir's justly famous contribution lay in their recognition that the degeneration had nothing to do with plunger design but was the end result of an intrinsic instability in finite-amplitude surface waves. Although the paper describing this work was not published until 1967, the experiments and their theoretical analysis were in essence complete some years earlier. The phenomenon, they showed, depended on a delicate balance between amplitude dispersion and resonant de-tuning and, although their analysis was quite different, the same elements seemed to be involved as were contained in Benney's equations for weak surface wave interactions. I recall a series of informal discussions with them on this point as early as 1964, but it was not until 1967, in preparation for a discussion meeting at the Royal Society, that I was able to make the connexion by taking $\mathbf{k}_1 = \mathbf{k}_2$ in Benney's equation as the wavenumber of the primary wave train with $\mathbf{k}_2$ and $\mathbf{k}_3$ as neighbouring wavenumbers of a small perturbation. With this rather obvious simplification the instability appeared. The classical Stokes solution for finite-amplitude surface waves, known for over one hundred years, had been shown by Benjamin and Feir to be unstable, a fascinating but still isolated discovery that seemed at that time specific to surface waves.

Quite independently of this, I think, Andy Acrivos and Russ Davis, working in California, were finding experimentally that lowest-mode internal waves on a thin density interface were also unstable but in a different way, leading to an exponentially growing second mode perturbation. They did recognize this at the outset as a resonant interaction phenomenon, a simpler one than the Benjamin–Feir instability since the effect occurred at second order and there is not the resonant de-tuning required to compensate for amplitude dispersion as there is in the surface wave case. The general occurrence of these instabilities was perceived by Klaus Hasselmann, who published a short note immediately following the Davis–Acrivos paper (1967). He showed from equation (4) that *any* wave train with wavenumber $\mathbf{k}_1$, capable of undergoing resonant interactions as specified by (3), is unstable to a perturbation wave with wavenumber $\mathbf{k}_2$ or $\mathbf{k}_3$. Similar effects should then occur in a variety of contexts ranging from small-scale internal waves in the ocean to inertial oscillations, even waves on a planetary scale in the atmosphere or ocean of the rotating earth. Hasselmann's theorem alone makes comprehensible the qualitative features of a variety of phenomena including the degeneration of standing internal waves demonstrated by Angus McEwan (1971) as well as the subharmonic instabilities in gravity-capillary waves studied earlier by Larry McGoldrick; its extension to the third-order instabilities in surface waves (of which the Benjamin–Feir instability is the archetype) constrains the directions on the wavenumber plane of the energy fluxes in the vicinity of a sharply peaked ocean wave spectrum.

The study of resonant wave interactions was only one of the directions of research into the nonlinear behaviour of waves during these years. By the mid 1970s, these calculations were becoming almost routine, but extremely significant advances were being made elsewhere. The properties of finite-amplitude slowly varying wave trains were being elucidated in a series of beautiful contributions by Gerry Whitham (1960, 1962, 1965) using new powerful averaged Lagrangian methods, by David Andrews and Michael McIntyre (1978*a*, *b*) in developing a new and general formulation of the action conservation principle involved in the interaction of waves with slowly varying media, and by several others. Although Whitham's formulation provided the possibility of exact analysis, in the investigation of specific problems, approximations were still usually required. In 1967, David Benney and Alan Newell at M.I.T. showed that, in a slowly varying surface wave train of mean wavenumber k_0 and frequency σ_0, whose envelope is specified by $A(\mathbf{x}, t)$, the variation in the envelope could be specified by the equation

$$iA_t + i(\sigma_0/2k_0)\,A_x - (\sigma_0/8k_0^2)\,A_{xx} + (\sigma_0/4k_0^2)\,A_{yy} - \tfrac{1}{2}k_0^2\sigma_0|A|^2 A = 0, \tag{6}$$

correct to the third order. This has subsequently been called the 'nonlinear Schrödinger equation', although the A_{xx} term has the opposite sign from the classical equation. The same equation for the wave envelope was re-derived, apparently independently, by Hasimoto and Ono (1972) in Japan, Chu and Mei (1970) in the United States and Zakharov (1968) in the USSR and extended by Davey and Stewartson (1974) in the United Kingdom. Such is the efficiency of scientific communication!

Now, equation (6) is linear except for the final cubic term; it seemed to me while at Seattle in the summer of 1979 that if Benney's equations (2) capture the dynamics of surface waves to the third order, then the nonlinear Schrödinger equation should be derivable from them under the additional restriction that the wave train be slowly varying or that the spectrum be very narrow. Two separate approximations are involved in equation (6), that the wave slopes be small, $\epsilon \ll 1$, and that the spectral band-width, the ratio of wavelength to group length, $\Delta = \delta k/k \ll 1$; Benney's equation (2) requires only the first of these. The derivation is fairly simple but does not seem to have been published previously and so is given in the appendix. Henry Yuen and Bruce Lake (1975) gave a derivation based upon Whitham's averaged Lagrangian which requires $\Delta \ll 1$ but does not severely restrict ϵ – equation (6) was recovered by an expansion in powers of ϵ. The self-interaction term, the final term on the left-hand side, is of order ϵ^3, while the middle terms are of the order $\epsilon\Delta^2$. The first two terms in the equation are individually of order $\epsilon\Delta$, but their sum $(\partial/\partial t + c_g \partial/\partial x)\,A$ is of order ϵ^3 or $\epsilon\Delta^2$. When $\Delta \ll \epsilon \ll 1$, we recover an almost uniform wave train and (6) reduces to $iA_t = \frac{1}{2}k_0^2\sigma_0|A|^2 A$, which specified the amplitude dispersion or the Stokes increase in phase velocity to the third order. When $\epsilon \ll \Delta \ll 1$, we have linear propagation of small-amplitude wave groups. The interesting new phenomena uncovered in the past couple of years occur when $\epsilon \sim \Delta \ll 1$.

The Benjamin–Feir instability is a narrow-band phenomenon and can be described by the nonlinear Schrödinger equation as well as by the more general resonant interaction equations. Henry Yuen and Bruce Lake recovered Benjamin and Feir's analytical results from this equation in 1978. More interesting, however, are the new phenomena uncovered by the TRW group in California from a study of equation (6), the most striking of which is the existence of envelope solitons.

Envelope solitons are groups of finite-amplitude surface waves whose envelope propagates without change of form and which are capable of running through other such groups without permanent change, except possibly for phase shifts. They must be distinguished from pulse solitons or solitary waves in shallow water (and their internal wave cousins on a thin or shallow thermocline) in which a single wave crest can have these properties.

The dynamics of envelope solitons in deep water involves a balance between amplitude dispersion and linear dispersion about the carrier frequency. For sufficiently small slopes and band-widths, the nonlinear Schrödinger equation (6) can be used to find the envelope shape, and an exact solution to the one-dimensional equation was given by Zakharov and Shabat (1971) in the USSR and explored in much more detail by Henry Yuen and Bruce Lake (1975, 1978) and their co-workers. It is of the form

$$A = b_0 \operatorname{sech} \epsilon\{k_0(x - c_g t)\} \exp\left(-\tfrac{1}{4} i \epsilon^2 \sigma_0 t\right), \tag{7}$$

where b_0 is the central amplitude, and $\epsilon = k_0 b_0$. Note the necessary balance involving amplitude dispersion and linear dispersion, with the band-width equal to the wave slope. The number of waves in the envelope soliton scales as ϵ^{-1}.

Yuen and Lake showed that an initial pulse approaches the asymptotic form (7), together with a dispersive residual, in the time scale ϵ^{-2} wave periods characteristic of all weak interaction phenomena, and careful experiments confirmed this. They also demonstrated experimentally that, when envelope solitons with different carrier frequencies run together in a long tank, there is no interaction except for a change in phase. This is not at all surprising in view of the selectiveness of weak interactions – in the configurations chosen resonance is not possible so there is no energy interchange. Envelope solitons *will* interact significantly only when their wavenumber bands overlap, or, with obliquely travelling solitons, when the ratio of their carrier frequencies is appropriate for instability. (Note, though, that, because of the finite interaction time of the groups, $(\epsilon\sigma)^{-1}$, the amplitude of the interaction product in this case will be smaller by order ϵ than the amplitude of the original groups.)

These envelope solitons are long-crested, but I would guess that there also exist analogous two-dimensional wave patterns that, to this order, propagate without change in envelope and which have the non-interaction property that I have described. If the spectrum consists of two narrow peaks about wavenumbers that form a parallelogram and lie on the central figure-of-eight interaction diagram for surface waves, the energy flux among the four components will cancel out for a specific shape of the spectral peaks and the same net balance of amplitude and linear dispersion should result, but I do not think that anyone has yet worked out the details.

The situations in which resonant interaction theory has received good experimental support are those in which the wavenumber magnitudes involved in the interaction are of the same order. If the wavenumber scales are very disparate, the simple theory that has been developed to date comes unstuck. It may not be widely appreciated that, if the wave field contains components with widely different wavelengths, the Stokes expansion underlying Benney's equations imposes an intolerable restriction on the amplitudes of the longest components. If both long and short waves are present, the shortest vertical length scale of the motion in the water is the short wavelength; for the Stokes expansion about $z = 0$ to be useful, a necessary condition is that the maximum displacement of the free surface (at a long wave crest) must be small compared with the shortest wavelength. In all but the most trivial oceanic situations, this condition is strongly violated.

The interactions between long and short waves cannot usefully be represented in terms of a Stokes expansion and cannot be included among the weak interactions. The point is not just a mathematical quibble – even the simplest physics of the oceanic situation involves orbital velocities of long waves that are comparable with or exceed the phase and group velocities of the short waves. A physical short-wave train is convected, distorted and turned by the distribution of long-wave orbital velocity – it is certainly not a single Fourier component. The same kind of restriction came up in McComas' studies of internal waves – the fluid velocities associated with large-scale internal waves may well exceed the propagation speed of the small-scale components so that they are significantly convectively distorted in a similar way. Michael Longuet-Higgins (1978) and I (1981) have looked at the strong interactions among long and short surface wave components; it is not yet clear how these convective distortions will modify the weak interaction processes though Bruce West is addressing the problem in more detail and at greater depth than I can discuss here. Perhaps the simple ideas developed in the past twenty years by many people have reached their natural limit, at least in this direction, and further progress will be dependent on new mathematics, new physics and new intuition.

Appendix. Derivation of the nonlinear Schrödinger equation from the resonant interaction equations

Consider a wave group or a slowly varying wave train in which the surface displacement

$$\zeta = \int a(\mathbf{k})\, e^{i\chi} d\mathbf{k}, \quad \chi = \mathbf{k}.\mathbf{x} - \sigma t, \tag{A 1}$$

and the only significant contributions come from a small range of wavenumbers and frequencies about $\mathbf{k}_0$, σ_0. Equation (A 1) can then be written as

$$\zeta = \exp\{i(\mathbf{k}_0.\mathbf{x} - \sigma_0 t)\} \int a(\mathbf{k}) \exp[i\{\boldsymbol{\kappa}.\mathbf{x} - (\sigma(\mathbf{k}) - \sigma_0)\, t\}]\, d\mathbf{k}$$

(in which $\boldsymbol{\kappa} = \mathbf{k} - \mathbf{k}_0$), specifying a wave train or group with wavenumber and frequency $\mathbf{k}_0$ and σ_0 and with a local amplitude (or envelope) of

$$\begin{aligned} A &= \int a(\mathbf{k}) \exp[i\{\boldsymbol{\kappa}.\mathbf{x} - (\sigma(\mathbf{k}) - \sigma_0)\, t]\}\, d\mathbf{k} \\ &= \int a(\mathbf{k})\, e^{i\phi} d\mathbf{k}, \quad \text{say.} \end{aligned} \tag{A 2}$$

Now, the component amplitudes $a(\mathbf{k})$ may be slowly varying functions of time t as a result of weak interactions, so that the rate of change of the envelope

$$A_t = \int \dot{a}\, e^{i\phi} d\mathbf{k} - i \int (\sigma(\mathbf{k}) - \sigma_0)\, a\, e^{i\phi} d\mathbf{k}. \tag{A 3}$$

Since the spectrum is narrow, $\sigma(\mathbf{k})$ can be expanded about σ_0:

$$\sigma(\mathbf{k}) - \sigma_0 = \kappa_i \left.\frac{\partial \sigma}{\partial k_i}\right|_0 + \tfrac{1}{2}\kappa_i \kappa_j \left.\frac{\partial^2 \sigma}{\partial k_i \partial k_j}\right|_0 (1 + O(\Delta)), \tag{A 4}$$

where $\mathbf{k}_0\, \Delta$ is the spectral width. Since $\sigma = (gk)^{\frac{1}{2}}$,

$$\left.\frac{\partial \sigma}{\partial k_i}\right|_0 = (\tfrac{1}{2} g^{\frac{1}{2}} k^{-\frac{3}{2}} k_i)_0 = \frac{\sigma_0}{2k_0^2}\, k_i|_0$$

and

$$\frac{\partial^2 \sigma}{\partial k_i \partial k_j} = \frac{\sigma_0}{2k_0^2} \left.\left(\delta_{ij} - \frac{3k_i k_j}{2k^2}\right)\right|_0$$

and, if the 1-direction is chosen as that of $\mathbf{k}_0$, it follows from (A 4) that

$$\sigma(\mathbf{k}) - \sigma_0 = \frac{\sigma_0}{2k_0}\kappa_1 - \frac{\sigma_0}{8k_0^2}\kappa_1^2 + \frac{\sigma_0}{4k_0^2}\kappa_2^2, \tag{A 5}$$

to the second order in the band-width Δ. This can be substituted back into (A 3), giving

$$A_t + i\frac{\sigma_0}{2k_0}\int \kappa_1 a\, e^{i\phi}\, d\boldsymbol{\kappa} - i\frac{\sigma_0}{8k_0^2}\int \kappa_1^2 a\, e^{i\phi}\, d\boldsymbol{\kappa} + i\frac{\sigma_0}{4k_0^2}\int \kappa_2^2 a\, e^{i\phi}\, d\boldsymbol{\kappa} = \int \dot{a}\, e^{i\phi}\, d\boldsymbol{\kappa}. \quad \text{(A 6)}$$

But, from (A 2)

$$A_x = i\int \kappa_1 a\, e^{i\phi}\, d\boldsymbol{\kappa},$$
$$A_{xx} = -\int \kappa_1^2 a\, e^{i\phi}\, d\boldsymbol{\kappa},$$
$$A_{yy} = -\int \kappa_2^2 a\, e^{i\phi}\, d\boldsymbol{\kappa},$$

so that (A 6) simplifies to

$$A_t + \frac{\sigma_0}{2k_0}A_x + \frac{i\sigma_0}{8k_0^2}A_{xx} - \frac{i\sigma_0}{4k_0^2}A_{yy} = \int \dot{a}\, e^{i\phi}\, d\boldsymbol{\kappa}. \quad \text{(A 7)}$$

Note that, to this point, there is no nonlinear dynamics at all. The first two terms can be written $(\partial/\partial t + c_g \partial/\partial x)A$ and represent envelope propagation with the group velocity. The next two represent the effects of (linear) dispersion about the carrier wavenumber and frequency, the band-width Δ being finite but small.

The final term in (A 7) is non-zero only because of the weak interactions. It is already of order ϵ^3 in the Stokes expansion parameter, so that, if the band-width Δ is small, the lowest order of approximation Δ^0 suffices to give its leading term. From the weak interaction equation in the form appropriate to a continuous distribution of $a(\mathbf{k})$

$$\int \dot{a}\, e^{i\phi}\, d\boldsymbol{\kappa} = iT(\mathbf{k}_0)\int \ldots \int \delta(\mathbf{k}+\mathbf{k}_1-\mathbf{k}_2-\mathbf{k}_3)\, a^*(\mathbf{k}_1)\, a(\mathbf{k}_2)\, a(\mathbf{k}_3)\, e^{i\phi}\, d\mathbf{k}\, d\mathbf{k}_1\, d\mathbf{k}_2\, d\mathbf{k}_3, \quad \text{(A 8)}$$

where, since all the wavenumbers concerned are nearly equal,

$$T(\mathbf{k}\ldots\mathbf{k}_3) = T(k_0)(1+O(\Delta)) = \tfrac{1}{2}k_0^2\sigma_0.$$

Now if ϕ_1, ϕ_2, ϕ_3 represent quantities like (A 2) for the other wavenumbers, in view of the resonance conditions, $\phi = -\phi_1+\phi_2+\phi_3$; the delta function can be integrated over $\mathbf{k}$ and the integrals on the right-hand side of (A 8) can be separated:

$$-\tfrac{1}{2}ik_0^2\sigma_0\int a^*(\mathbf{k}_1)\, e^{-i\phi_1}\, d\mathbf{k}_1 \int a(\mathbf{k}_2)\, e^{i\phi_2}\, d\mathbf{k}_2 \int a(\mathbf{k}_3)\, e^{i\phi_3}\, d\mathbf{k}_3 = -\tfrac{1}{2}ik_0^2\sigma_0 A^* A^2.$$

Thus (A 7) becomes

$$A_t + \frac{\sigma_0}{2k_0}A_x + \frac{i\sigma_0}{8k_0^2}A_{xx} - \frac{i\sigma_0}{4k_0^2}A_{yy} + \tfrac{1}{2}i\sigma_0 k_0^2 |A|^2 A = 0,$$

or, as it is usually written,

$$i\left(A_t + \frac{\sigma_0}{2k_0}A_x\right) - \frac{\sigma_0}{8k_0^2}A_{xx} + \frac{\sigma_0}{4k_0^2}A_{yy} - \tfrac{1}{2}\sigma_0 k_0^2 |A|^2 A = 0. \quad \text{(A 9)}$$

REFERENCES

Andrews, D. G. & McIntyre, M. E. 1978*a* An exact theory of non-linear waves on a Lagrangian-mean flow. *J. Fluid Mech.* **89**, 609–646.

Andrews, D. G. & McIntyre, M. E. 1978*b* On wave-action and its relatives. *J. Fluid Mech.* **89**, 647–664.

Ball, K. 1964 Energy transfer between external and internal gravity waves. *J. Fluid Mech.* **19**, 465–478.

Benjamin, T. B. & Feir, J. E. 1967 The disintegration of wave trains on deep water. *J. Fluid Mech.* **27**, 417–430.

Benney, D. T. 1962 Non-linear gravity wave interactions. *J. Fluid Mech.* **14**, 577–584.

Benney, D. J. & Newell, A. C. 1967 The propagation of non-linear wave envelopes. *J. Math. Phys.* **46**, 133–145.

Brekhovskikh, L. M., Goncharov, V. V., Kurtipov, V. M. & Nangol'nykh, K. A. 1972 Resonant excitation of internal waves by non-linear interaction of surface waves. *Izv. Akad. Nauk SSSR, Fiz-Atmos. Okeana*, **8**, 192–203. (Engl. trans.: *Atmos. Oceanic Phys.* **8**, 192–197.)

Bretherton, F. P. & Garrett, C. J. R. 1969 Wavetrains in inhomogeneous moving media. *Proc. Roy. Soc.* A **302**, 529.

Chu, V. H. & Mei, C. C. 1970 On slowly varying Stokes waves. *J. Fluid Mech.* **41**, 837–887.

Davey, A. & Stewartson, K. 1974 On three-dimensional packets of surface waves. *Proc. Roy. Soc.* A **338**, 101–110.

Davis, R. E. & Acrivos, A. 1967 The stability of oscillatory internal waves. *J. Fluid Mech.* **30**, 723–736.

Gargett, A. E. & Hughes, B. A. 1972 On the interaction of surface and internal waves. *J. Fluid Mech.* **52**, 179–192.

Garrett, C. & Munk, W. H. 1972 Space–time scales of internal waves. *Geophys. Fluid Dyn.* **3**, 225–264.

Hasimoto, H. & Ono, H. 1972 Non-linear modulation of gravity waves. *J. Phys. Soc. Japan* **33**, 805.

Hasselmann, K. 1962 On the non-linear energy transfer in a gravity wave spectrum. Part 1. *J. Fluid Mech.* **12**, 481–500. Part 2 (1963) Ibid, **15**, 273–281. Part 3, (1963) Ibid, **15**, 385–398.

Hasselmann, K. 1967 A criterion for non-linear wave stability. *J. Fluid Mech.* **30**, 737–739.

Haurwitz, B., Stommel, H. & Munk, W. H. 1959 On thermal unrest in the ocean. *Rossby Mem. Vol.* pp. 74–94. New York: Rockefeller Inst. Press.

Kim, Y. Y. & Hanratty, T. J. 1971 Weak quadratic interactions of two-dimensional waves. *J. Fluid Mech.* **50**, 107–132.

Lewis, J. E., Lake, B. M. & Ko, D. R. S. 1974 On the interaction of internal and surface gravity waves. *J. Fluid Mech.* **63**, 773–800.

Lighthill, M. J. 1950 Aerodynamic noise, or, turbulence as a source of sound. *Aero. Res. Counc., London.* Rep. no. FM 1467 10 Aug. 1950.

Lighthill, M. J. 1952 On sound generated aerodynamically I. General theory. *Proc. Roy. Soc.* A **211**, 564–587.

Lighthill, M. J. 1954 On sound generated aerodynamically. II. Turbulence as a source of sound. *Proc. Roy. Soc.* A **222**, 1–32.

Long, R. R. 1953*a* A laboratory model resembling the 'Bishop Wave' phenomenon. *Bull. Amer. Met. Soc.* **34**, 205–211.

Long, R. R. 1953*b* Some aspects of the flow of stratified fluids. I. A theoretical investigation. *Tellus* **5**, 42–57.

Long, R. R. 1954 Some aspects of the flow of stratified fluids. II. Experiments with a two-fluid system. *Tellus* **6**, 97–115.

Long, R. R. 1955 Some aspects of the flow of stratified fluids. III. Continuous density gradients. *Tellus* **7**, 342–357.

Longuet-Higgins, M. S. 1962 Resonant interactions between two trains of gravity waves. *J. Fluid Mech.* **12**, 321–332.

Longuet-Higgins, M. S. 1978 The instabilities of gravity waves of finite amplitude in deep water. Part I. Superharmonics. *Proc. Roy. Soc.* A **360**, 471–488.

Longuet-Higgins, M. S. & Phillips, O. M. 1962 Phase velocity effects in tertiary wave interactions. *J. Fluid Mech.* **12**, 333–336.

Longuet-Higgins, M. S. & Smith, N. D. 1966 An experiment on third order resonant wave interactions. *J. Fluid Mech.* **25**, 417–435.

Martin, S., Simmons, W. & Wunsch, C. 1972 The excitation of resonant triads by single internal waves. *J. Fluid Mech.* **53**, 17–44.

McComas, C. H. 1977 Non-linear interactions of internal gravity waves. *J. Geophys. Res.* **82**, 1397–1412.

McEwan, A. D. 1971 Degeneration of resonantly excited internal gravity waves. *J. Fluid Mech.* **50**, 431–448.

McGoldrick, L. F. 1965 Resonant interactions among capillary-gravity waves. *J. Fluid Mech.* **21**, 305–332.

McGoldrick, L. F. 1970 An experiment on second order capillary-gravity resonant interactions. *J. Fluid Mech.* **40**, 251–271.

McGoldrick, L. F. 1972 On the rippling of small waves: a harmonic non-linear nearly resonant interaction. *J. Fluid Mech.* **52**, 725–751.

McGoldrick, L. F., Phillips, O. M., Huang, N. & Hodgson, T. 1966 Measurements on resonant wave interactions. *J. Fluid Mech.* **25**, 437–456.

Nayfeth, A. H. 1971 Third harmonic resonance in the interaction of capillary and gravity waves. *J. Fluid Mech.* **48**, 385–395.

Peierls, R. E. 1929 Zur kinetischen Theorie der Wärmeleitungen in Kristallen. *Ann. Phys.* **3** 1055–1101.

Perry, R. B. & Schimke, G. R. 1965 Large amplitude internal waves observed off the northwest coast of Sumatra. *J. Geophys. Res.* **70**, 2319–2324.

Phillips, O. M. 1960 On the dynamics of unsteady gravity waves of finite amplitude. Part I. *J. Fluid Mech.* **9**, 193–217. Part II (1961) *ibid.* **11**, 143–455.

Phillips, O. M. 1981 The dispersion of short wavelets in the presence of a dominant long wave. *J. Fluid Mech.* (in the press).

Watson, K. M., West, B. J. & Cohen, B. I. 1976 Coupling of surface and internal gravity waves: a Hamiltonian model. *J. Fluid Mech.* **77**, 185–208.

Whitham, G. B. 1960 A note on group velocity. *J. Fluid Mech.* **9**, 347–352.

Whitham, G. B. 1962 Mass, momentum and energy flux in water waves. *J. Fluid Mech.* **12**, 135–147.

Whitham, G. B. 1965 A general approach to linear and non-linear dispersive waves using a Lagrangian. *J. Fluid Mech.* **22**, 273–283.

Yuen, H. C. & Lake, B. M. 1975 Non-linear deep water waves: theory and experiment. *Phys. Fluids* **18**, 956–960.

Yuen, H. C. & Lake, B. M. 1978 Non-linear wave concepts applied to deep water waves. In *Solitons in Action*, pp. 89–126. Academic Press.

Zakharov, V. E. 1968 Stability of periodic waves of finite amplitude on the surface of a deep fluid. *Zh. Prikl. Mekh. Fiz.* **9**, 86. (Engl. transl. *J. Appl. Mech. Tech. Phys.* **2**, 190.)

Zakharov, V. E. & Shabat, A. B. 1971 Exact theory of two-dimensional self-modulating waves in non-linear media. *Zh. Eksp. Teor. Fiz.* **61**, 118. Also *Soviet J. Exp. Theor. Phys.* **34** (1972), 62–69.