INTERNATIONAL ATOMIC ENERGY AGENCY
UNITED NATIONS EDUCATIONAL, SCIENTIFIC AND CULTURAL ORGANIZATION

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SMR/23/71

AUTUMN COURSE ON APPLICATIONS OF ANALYSIS TO MECHANICS

22 September - 26 November 1976

RELATIONSHIP BETWEEN THEORY AND EXPERIMENT IN CONTINUUM MECHANICS

(Part I)

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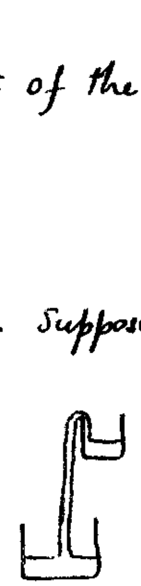
1. INTRODUCTION

The philosophical rationale of one of the leading journal concerned with the applications of analysis to mechanics is given on the inside cover of each issue. It is as follows.

> "The Archive for Rational Mechanics and Analysis nourishes the discipline of mechanics as a deductive, mathematical science in the classical tradition and promotes pure analysis, particularly in contexts of application."

In this set of lectures I shall take issue with the above view of mechanics & instead I shall propose the thesis that, central to the development of the subject, is the experimental method. For, otherwise we should all be studying science fiction, & 'The Archives' would be of no more value to science than another Azimov novel. Therefore I propose to give you my view of the role of experiment in mechanics, & of the part played by the applied mathematician in the development of the subject, through the influence of the empirical evidence before him.

Let me give you an example of what I am trying to say. Suppose that mechanics is a deductive discipline. Consider the following 'simple' demonstration (see sketch). Everyone will



what I want to know, <u>a priori</u>, is the minimal complete set of consistent axioms from which I can deduce the properties of this experiment. Even then, how would I know they are the 'correct' axioms to choose? This question is central to the whole of empirical science, & it is why science is an inductive rather than a deductive discipline.

I should like first to discuss the role, within mechanics, of the applied mathematician. The central objective of his work is to solve specific problems & this should remain the focus for his activities. But, loosely speaking, I think there are three main areas of work.

1) The solution of a specific problem (anything goes).

2) The development of techniques & methods for problem solving (includes numerical methods)

3) model building; mathematical properties of models; examination of the validity of special techniques from (2), say.

(↓ increasingly more exacting argumentation required, but also more remote from the core problem.)

All three areas should be equally healthy & active, if the subject is to be in a good state. It is becoming increasingly difficult for an individual to be really competent in all three of these areas, let alone be involved in the

do [illegible]

science, we will have to depend more & more on working within a group covering a wide range of technical skills & expertise. I would suggest a composition of such a grouping for mechanics to contain people with special interests in: (i) experimental mechanics; (ii) interpretative work & 'seat of the pants' model building; (iii) numerical methods; (iv) areas involving mathematical rigour (of lighter & darker shades).

In short, I think a fairly cosmopolitan approach to science is valuable, both from what can be achieved now, and also with regard to the training of the next generation of scientists.

But, to return to the main theme, the relationship between the theoretical & the empirical arms of science, & in this regard I would like to quote a book review, the text of which is given in the lecture notes. The function the experimental scientist performs is to report a set of observations & give an account of he thought were the important features of the experiment. You, as a scientist, have to assess how much weight to put on these particular observations & I think the story in this review is a good example of the scientific method

NY Review of Books (~Feb 1976) W.G. PRITCHARD

Paranonsense

Superminds: A Scientist Looks at the Paranormal
by John Taylor.
Viking, 183 pp., $10.95

The Magic of Uri Geller
by the Amazing Randi.
Ballantine Books, 320 pp.,
$1.75 (paper)

Martin Gardner

You suspect someone of habitually cheating at cards. Whom would you hire as a secret observer to settle the matter? A physicist?

A self-proclaimed psychic goes about performing miracles exactly like the feats of magicians who specialize in what the trade calls "mentalism." You suspect the psychic of cheating. Whom do you call upon as an expert witness? A physicist?

One of the saddest, most persistent aspects of the history of alleged psychic phenomena is that there always has been a small, noisy group of scientists who, combining enormous egotism with even greater gullibility, actually imagine that *they* are competent to detect psychic fraud. Let's take a quick look at a prize specimen: Johann Zöllner, an Austrian professor of astrophysics.

In the 1870s Zöllner was bowled over by the miracles of an American medium, Henry Slade. Slade was a handsome scoundrel whose most unusual flimflam was causing knots to appear on closed loops of cord. He also was a virtuoso in producing, on blank slates, insipid chalked messages from discarnates in the "other world." He could wave his hand over a compass and make the needle gyrate. Small objects, sometimes water, had a way of falling out of the air near him.

Not once was Zöllner capable of seriously entertaining the hypothesis that so charming a gentleman as Slade could be a fraud. Indeed, Zöllner rushed into print an entire book about Slade, *Transcendental Physics*, in which he argued that physical space has four dimensions, and Slade had a supermind capable of moving test materials in and out of four-space.

The book is a classic case, written by an honest but stupid pedagogue. Magicians read it today with hilarity because Slade's methods are well known, and by reading between the lines they can reconstruct what Slade was doing. I myself, a lifelong student of conjuring, never expected to see a later volume that would demonstrate, with so much unconscious humor, how easily a scientist can be hornswoggled by the simplest of deceptions.

Until I read *Superminds*. This big, glossy book, plastered with sensational photographs of levitated tables and mediums, and forks twisted by the superminds of grinning children and pretty ladies, must be seen to be believed. That it should be written at all, by a man who now runs the risk of being remembered only as the British boob of the century, is more improbable than any "miracle" it describes. The book is subtitled, "A scientist looks at the paranormal." It should have been subtitled, "A scientist gapes at Uri Geller," because Geller, the young and handsome Israeli prestidigitator who insists he never prestidigitates, is both the book's immediate

cause as well as its superstar.

It all began in November, 1973, when John Taylor, then forty-two, appeared on a BBC television show with Uri. Taylor, a respected mathematical physicist at Kings College, London, had expected to be unimpressed. Instead, he became bug-eyed with astonishment. Uri performed his now familiar, increasingly tiresome, little bag of tricks. He made a fork bend. He started a stopped watch. He duplicated a drawing inside sealed envelopes.

Magicians watching the show were singularly unimpressed. But Dr. Taylor and his friends, who sometimes call themselves "counterculture physicists," suffer from a peculiar syndrome which I call PPE or "premature psi ejaculation." Instead of waiting for a psychic to be tested by skeptical psychologists, aided by competent magicians, whenever a CCP sees a self-dubbed psychic do a few magic tricks he instantly pronounces the feats genuine and fires off a press release, article, or book.

This is how Taylor describes his emotions after testing Uri in his laboratory:

> One clear observation of Geller in action had an overpowering effect on me. I felt as if the whole framework with which I viewed the world had suddenly been destroyed. I seemed very naked and vulnerable, surrounded by a hostile, incomprehensible universe. It was many days before I was able to come to terms with this sensation.

After Taylor regained his composure, he discovered that he had become a true Gellerite. Not only does Uri possess a supermind (the best), but hundreds of British children (Taylor believes) also can produce the "Geller effect"—making objects bend and break by a power of the mind. Like his CCP friends, Taylor is less interested in proving the Geller effect exists (after all, has he not *personally* witnessed it?) than in measuring it and developing a theory. What force of nature is responsible? Gravity? The weak force? Neutrinos? Conjectured particles such as tachyons, intermediate bosons, magnetic monopoles, quarks? He finally settles on electromagnetism as the most likely candidate.

Taylor's ignorance of conjuring is almost total. Describing Uri's reproduction of a drawing inside envelopes, he writes, "No methods known to science can explain his revelation of that drawing. . . ." Well, what about methods *not* known to science? I can assure Professor Taylor that there are more than thirty distinct techniques by which a mentalist can accomplish just such a feat, the methods varying with conditions under which the performer is restrained.[1]

After watching Geller make a compass needle rotate by waving his hand above it, Taylor attempted to imitate Uri's movements. He even tried stamping on the ground. Nothing happened. "Nor could Geller have been using a magnet," he writes with incredible presumption, "unless he could palm it with consummate skill. . . ."

It never occurred to Taylor—why should it?—that a magnet need not be palmed to make a compass needle move. When Slade performed this old trick he had the magnet in the tip of a shoe. He crossed his legs, and when he wanted the needle to turn he simply aised the tip of his shoe to the table's underside.[2] Another handy spot for a magnet is under the trousers on the knee. Today one can obtain powerful, flexible little magnets that are wafer-thin and easily concealed in the mouth between lower teeth and cheek. When Uri moved the compass needle on Tom Snyder's *Tomorrow Show* (August 14, 1975) he had the magnet either in his mouth or sewn in his shirt collar. This was evident from the fact that, each time the needle deflected, Uri's head darted close to the compass. By vigorously milking his left fist over the compass, Uri misdirected attention from his head. Then he put the compass on the floor, the camera glided in for a close up, and no viewer could see *where* Uri's head was.

But I'm already in trouble with some of my magic friends for revealing too much. On page 52 Taylor describes a closely related miracle. Geller produced bursts of noise in a Geiger counter. Did Taylor or any of the other brilliant CCPs present think of inspecting Geller afterward to see if he had a bit of harmless radioactive substance concealed on his person? It never occurred to them!

Magicians are understandably reluctant to expose Geller's methods. There is even a small group of mentalists who regard Uri as "one of the boys," no different from Kreskin and other pros except that by acting the role of a psychic more convincingly, and introducing innovations such as bending car keys and silverware, Uri is making more money than they are.

I disagree. There is, in my opinion, a qualitative difference between Uri's career and the careers of the great mentalists of the past. Uri is not in the tradition of such entertainers as Anna Eva Fay and Joseph Dunninger. Uri is in the tradition of the great physical mediums of the nineteenth century. True, the spirits of the departed have departed from most of today's psi laboratories, but in their places are those mysterious energies "unknown to science," possibly emanating from superminds in outer space. There is the same damage to science and to individual lives.

Consider some of the effects of Uri's ruthless pursuit of money and adulation. Andrija Puharich, at one time respected by his colleagues in parapsychology, has had his career reduced to shambles. Prominent journalists and scientists, and one brave astronaut who walked on the moon, have been made to appear almost feeble-minded. Thousands of others, especially among the young, have been so dazzled by Uri that they believe him to be in the vanguard of a new revolution in human consciousness.

For these and other reasons I cannot agree with those magicians who snipe at James Randi, professional conjuror, for his just published paperback, *The Magic of Uri Geller*. It is an excellent book, sensible, well-informed, witty, compassionate, and utterly devastating. Uri has often hinted that anyone who attacks him is in danger of being whammied by his great powers. Indeed, in Puharich's addlepated book, *Uri*, Geller professes to be dismayed over an occasion on which he became angry at someone, wished him harm, and the poor man obediently died. But Randi doesn't scare easy.

The Magic of Uri Geller is a collection of eye-opening articles by skeptics, mixed with amusing and acid commentary by the Amazing Randi. Joe Hanlon, who wrote the *New Scientist*'s excellent special issue on Geller (October 17, 1974), gives his reasons for believing Uri to be a mountebank. Andrew Weil's two-part article in *Psychology Today* is here: Part 1 telling how Uri bamboozled him, Part 2 recording his disenchantment after seeing Randi bend keys

inside details of Uri's failure to impress Leon Jaroff and other editors of *Time* is faithfully told in contrast to Puharich's muddied account.

Did Uri once teleport himself from a sofa in Puharich's home in Ossining to the streets of Rio, then return with a thousand cruzeiro note? Uri says yes, but read Randi's chapter 8! Yale Joel, a *Life* photographer, discloses how Uri was caught faking a photo supposedly taken of himself without removing the camera's lens cap. How does Uri repair "broken" watches? See chapter 12. Why did Uri bomb on the Johnny Carson show? Was it because Uri was nervous, or because Johnny, an ex-magician, never allowed Uri or his friends access to the test materials before the show began? How did Uri manage to "melt" a spoon so convincingly for Barbara Walters that for a year her outlook on life was changed?

One of Uri's most sensational achievements in clairvoyance, when he was tested at the Stanford Research Institute, was guessing eight times in a row the top face of a die that had been shaken in a metal file box by "one of the experimenters." Is there a simple way Uri could have cheated? There is indeed. Joaquin Argamasilla, a young Spanish magician, convinced many CCPs that his "X-ray eyes" could see through steel and silver boxes. Houdini exposed the Spaniard's technique in a rare pamphlet that Randi reprints; then Randi makes a shrewd guess about how Uri could have used the same basic method.

Perhaps the most damaging chapter in Randi's explosive book is the translation of an article about Uri that appeared last year in a Tel Aviv weekly paper. Itzhaak Saban, a former friend

[1] For details about some of them, see *Confessions of a Psychic*, an anonymous booklet that purports to be the secret diary of Uri's chief rival, Uriah Fuller. The booklet is intended for sale only to magicians, but interested readers can obtain a copy postpaid by sending $3.30 to the publisher, Karl Fulves, Box 433, Teaneck, NJ 07666.

[2] Slade was actually caught using this method (well known to magicians of his day) by an astute investigator who observed Slade's foot in a mirror he had secretly placed on his lap. See the *Proceedings of the American Society for Psychical Research*, Vol. 15 (1921), page 556. CCPs seldom adopt such sneaky stratagems because: (1) they don't know enough about magic to think of them; (2) if they did, they would hesitate to set a trap because it would indicate a lack of trust and that could disturb the delicate operation of psi.

of Uri who served as his chauffeur in Israel, told the reporter how he once "stooged" for Uri by secretly signaling information from a front seat during Uri's stage performances. Hannah Shtrang, older sister of Uri's inseparable companion, Shipi, tells how Uri and Shipi first became interested in conjuring, how they developed Uri's stage show, and how Shipi became Uri's number one confederate. In those days Uri introduced Shipi as his brother. He would refuse to perform unless "little brother" had a front row seat Hannah herself sometimes took over signaling chores.

"Uri and Shipi used to train for long hours together," said Hannah, "even after he was already famous, drilling together on the drawing and reproduction of certain objects after they cast only a quick glance at them." Saban showed the reporter how Uri uses sleight of hand to change the hands on a watch, apparently "without touching it in any way." When anyone mentions the scientists who have certified Geller, the reporter writes, Saban "reacts with a wide know-it-all grin."

The most hilarious sections in Randi's book are those in which he tells of his adventures in England early this year while gathering material for his book. Disguised as James Zwinge (his real name), a supermind from Canada, Randi visited the offices of *Psychic News*, London's leading psychic periodical. The office was soon in an uproar as Zwinge leaped about "Gellerizing" object after object. The result: a picture of Zwinge on the front page of the July 26 issue, and a big story about the awesome powers of this strange man "with a gray beard and intense eyes" who "seemed to radiate a magnetic aura."

A spoon bent and broke. A cabinet key twisted. A paper knife bent 45 degrees. "All could vouch Zwinge had not been near it," said *Psychic News*. A clock on the wall suddenly gained two hours. Another office clock gained two and a half hours. "Certainly Zwinge had no opportunity to interfere with them. He had been under constant surveillance.... I was fully alerted to any suspicious moves. But Zwinge made none."

Randi telephoned Taylor. The professor cut him off with "I have all the evidence I need" and hung up. But Taylor didn't know what Randi looked like, and Zwinge's face had not yet graced the London newsstands. Posing as a reporter from *Time*, Randi had no difficulty getting the interview he wanted. What happened in Taylor's office is one of the book's funniest highlights.

Take a look at the photograph on page 159 of *Superminds*. It shows Taylor's holiest relic—a plastic tube, corked and sealed at both ends. Inside is an originally straight strip of aluminum, now bent into an S shape by the supermind of a teen-age boy. Did Taylor actually *see* it bend? Well, no. There is, you see, what the professor calls (so help me) the "shyness effect." Things bend only when one is not looking at them. Superboy took the tube home, came back with the strip bent. Now, students, let's turn to Randi's account of what happened when he surreptitiously examined this tube. While pretending to admire the sacred relic, Randi tugged on one of the rubber corks. It came right out in his hand! Taylor didn't notice, so Randi quickly jammed the cork back in. "The screw-and-sealing wax precaution hadn't mattered a bit," comments Randi. "It was a very poor piece of preparation. . . ."

Since Randi wrote his book, two psychologists at Bath University had no difficulty designing a simple test for six young superminds. The observer was instructed to relax vigilance after twenty minutes. Rods and spoons Gellerized beautifully while the unsuspecting children were secretly videotaped through one-way mirrors. "*A* put the rod under her feet to bend it; *B*, *E* and *F* used two hands to bend a spoon. . . . We can assert that in no case did we observe a rod or spoon bent other than by palpably normal means" (*Nature*, September 4, 1975, page 8).

Taylor is only the latest of the many casualties that psychic hustlers leave in their luminous wakes. Will Randi's book result in fewer future casualties? I doubt it. The mind sets of true believers go so deep that, even if Uri were to confess all, they wouldn't believe him. Remember Margaret Fox, who started modern Spiritualism by cracking her toes? (Her facial features, by the way, resembled Uri's so closely as to suggest a reincarnation.) None of Margaret's supporters believed *her* when she confessed.

Uri once said to Stefan Kanfer, of *Time*: "Randi is jealous of me because I'm young and good-looking, and have nice wavy hair."

"Well," Randi concludes his book, "I'm no longer as young as I'd prefer to be, and most of the hair has departed during the years, that's true. But I sleep well, Uri." □

As I have already mentioned, the role of the experimenter is to port what he actually observes in his experiment. In that respect, he is mewhat like a witness giving a testimony in a court of law. On the basis the way he gives his evidence, of the detail he observed & of the proven reliability the witness with respect to previous similar events, you as the judge have to ake an assessment of how heavily to weight the evidence presented by the particular itness. So, how is it that the empirical method enables us to know as much s we do? This question has concerned philosophers for centuries &, as an xample, Hume, in his discourse on Human Understanding wrote:

"But though animals learn many parts of their knowledge from observation, there are also many parts of it, which they derive from the original hand of nature; which much exceed the share of capacity they possess on ordinary occassions; and in which they improve, little or nothing, by the longest practice & experience. These we denominate Instincts, & are so apt to admire as something very extraordinary, & inexplicable by all the disquisitions of human understanding. But our wonder will, perhaps, cease or diminish, when we consider, that the experimental reasoning itself, which we possess in common with the beasts, & on which the whole conduct of life depends, is nothing but a species of instinct or mechanical power, that acts in us unknown to ourselves; and in its chief operations, is not directed by any such relations or comparisons of ideas, as are the proper objects of our intellectual faculties. Though the instinct be different, yet still it is an instinct, [illegible] as much as that, which teaches

As an example, consider how it is that one goes about learning a language? Take this example in the light of the kinds of questions Bertrand Russell posed: "how comes it that human beings, whose contacts with the world are brief & personal & limited are nevertheless able to know as much as they do know?" In his studies Russell attempted to discover the principles of non-demonstrative inference that justify scientific inference. He concluded that 'part of empiricist theory appears to be true without any qualification, namely that words which I can understand derive their meaning from my experience with no need to admit any exceptions whatever'. But another part of empiricist theory is untenable & Russell concluded, in a similar way to Hume, that we need certain principles of inference that cannot be logically deduced from facts of experience: "Either, therefore, we know something independently of experience, or science is moonshine."

Russell believes that we need no more than five postulates to validate scientific method. These he called: ‡

I. The postulate of quasi-permanence. (Given an event A, it happens frequently that, ~~at~~ any neighbouring time, there is at some neighbouring place an event very similar to A.

II The postulate of separable causal lines. (It is frequently possible to form a series of events such that from one or two members of the series something can be inferred about all the others.)

III The postulate of spatio-temporal continuity. (When there is a causal connection between two events that are not contiguous, there must be intermediate links in the causal chain such that each is contiguous to the next.)

IV The structural postulate. (When a number of structurally similar complex events are ranged about a centre in regions not widely separated, it is usually the case that they all belong to causal lines having their origin in an event of the same structure at the centre.)

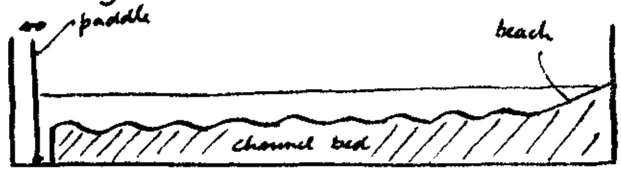
V The postulate of analogy. (Given two classes of events A & B, and given that, whenever both A & B can be observed, there is reason to believe that A causes B, then if, in a given case, A is observed but there is no way of observing whether B occurs or not, it is probable that B occurs; & similarly if B is observed, but the presence or absence of A cannot be observed.)

But I don't want to labour this point since, being scientists, & not philosophers, we quite rightly take all this for granted: clearly the [illegible] arising from turning on the water tap are described to an extremely good accuracy by the Navier-Stokes equations. If these equations were suddenly to fail to describe the motion of water they would have to fail (we believe) in an exceedingly complicated, or unusual, way in order to account for the good agreement obtained from them with experiments for more than 100 years.

What I would like to do in these lectures is to describe a number of particular pieces of work with which I have been involved, & to concentrate on two particular aspects. First I shall try to explain the kinds of things that can 'go wrong' with an experiment, in the hope that it will help you to be able to criticize, more easily, pieces of experimental work. Secondly, I shall try to indicate the influence the theoretical developments had on the experiments and vice versa.

II Some Experiments with Water Waves

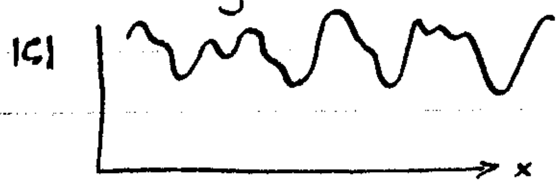
2.1 I would first like to describe a very simple experiment to show how easily one can get 'wrong' answers. Because this experiment is so straightforward we were easily able to determine whether or not we were doing the experiment we thought we were doing, & I think it provides a useful lesson for the way one might go about interpreting more complicated ituations. Some time ago we had a visitor in Colchester † who was interested in the formation of sand bars in the oceans, & following a suggestion (by Prof. T.B. Benjamin) as to a echanism for the possible formation of such bars we decided to make an xperiment in which long waves propagated along a channel, the bed of hich had a sinusoidal depth structure,



s shown in the sketch. We were interested n examining the structure of the wave field for waves whose wavelength was oughly twice that of the wavelength of the ripples on the bed. In order to ave enough waves on the channel bed, for the tank we had available, we ttled on an operating range & proceeded to make a control experiment th a flat bed in place of the bed with the periodic structure.

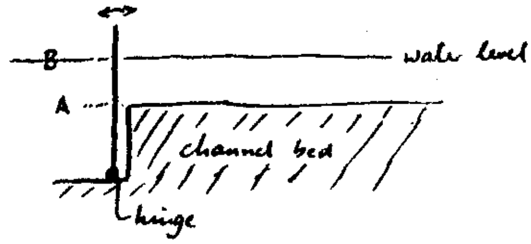
Because of wave reflection from the beach & because of viscous amping of the waves we had expected to see a pattern for the wave amplitude, $|\zeta|$, long the channel that was periodic in the distance, x, from the wavemaker, & the mean level of which decreased gradually along the channel (see sketch).

what was actually observed was as shown



Of course we were being silly in that we had, inadvertently, chosen a frequency of excitation which produced waves whose wavelength was less than the width of the tank. By so doing, we had generated cross waves in the tank which could propagate along the channel. By increasing the wavelength of the primary motion to a value larger than the width of the channel we obtained a pattern close to that expected originally, because under these conditions the cross waves are generated as decaying modes (cf. §3 below).

Having made such an elementary mistake, we decided to check that the amplitude of the progressive was close to that predicted by linear theory. Previous experiments suggested that this should be so (cf. Ursell et. al 1962)*, though some experiments by some French workers in the 1950's had given inexplicably large answers — about 60% in excess of the predictions from the linear theory. Our measurements suggested values about 70% greater than the exact prediction of the linear wrong. Since we did not doubt the theory, why was it that we got the 'wrong' answer. The resolution of the problem again lay in our stupidity: the arrangement the wavemaker & the bed of the channel was as shown in the sketch.



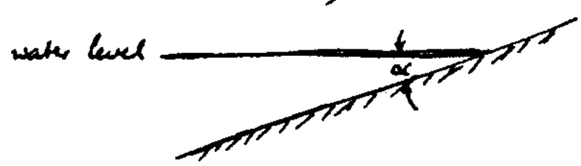
We had calculated the forcing for the waves by taking a driving condition based on the motion of the paddle between the levels A & B (as shown in the sketch) Although the gap between the paddle & the raized bed of the channel was only about 3 mm, clearly we cannot neglect the motion of the paddle below the level A in predicting the size of the waves. On removing the raized portion of the channel bed, & doing the experiment 'properly', we observed wave amplitudes in fairly good agreement with the theoretical prediction.

The reason for describing these rather silly mistakes, is to show just how easily one can go wrong, & that great care is needed in interpreting experimental measurements. (A nice example of a dilema of this kind is afforded by the attempts in the 1930's the determine the drag coefficient of a sphere. cf. Goldstein (modern developments in Fluid mechanics).)

2.2. Wave reflection from beaches †

The question of how much energy is absorbed by a wave impinging normally on a plane beach has been one of long-standing importance. The story I want to relate gives, I think, a very good example of how a simple experimental observation can yield valuable clues about how to approach a problem theoretically.

As indicated in §2.1, experimental measurements of the reflection have to be inferred from the wave pattern far away from the beach. Accordingly there have been very few experimental measurements of the reflection coefficient. On the other hand, there has been a lot of theoretical work concerned with waves on beaches, among which have been considerable efforts to find the fundamental solution for a dipole in a wedge-shaped domain (see sketch). The fluid is assumed to be inviscid. If, however,



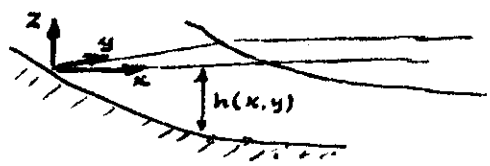
one asks a direct question, namely; what is the ratio of the reflected wave amplitude to that of the incident wave?, it is possible to answer the question fairly easily. Indeed, using an energy argument, & assuming that the waves do not break, it can be shown that the reflection coefficient is 100%, irrespective of the wave amplitude.

So, how is it that beaches are effective in absorbing wave energy? Is it because the waves break?, or is it a result of the 'run up' on the beach?, or what? We had been making some measurements of wave reflection from beaches, with extremely small waves, & it was evident from the experiments that the waves were not breaking & yet the reflection coefficient was about 10%. In discussing this we were led to the conclusion that the only possibility of accounting for this was the mechanical degradation through the boundary layer on the beach itself. The effect of the boundary layer on the, essentially inviscid, outer flow is to change the boundary condition on the beach from one of zero normal velocity to that of a small normal velocity required to 'match' onto the boundary layer. It turns out that the parameter measuring the significance of the bottom friction is $2\nu\sigma^3/g\alpha^2$, where ν is the kinematic viscosity, σ is the frequency & g is the acceleration due to gravity. For natural beaches $\alpha \sim 0.01$ & this parameter is not necessarily small. Calculations of the reflection coefficient for one of the experiments gave a value of 13% & the measured value was 12%. This the right kind of damping effect.

I think this example gives a good idea of how a very simple-minded experiment can yield very fruitful results, & help guide the theoretical argument.

2.3 Trapped waves

Consider waves over a sloping beach, & suppose that the amplitudes are small enough that we can take the free surface condition in its linear form. Choose (rectangular cartesian) coordinates as shown in the sketch.



Suppose that the water depth is $h(x,y)$ & let ϕ be the velocity potential. If the motions take place in a channel of width b we non-dimensionalize with respect to b & $(b/g)^{1/2}$ for the length & time scales respectively. (Otherwise we use another characteristic length in the problem.). It then follows that

$$\Delta\phi = 0 \quad , \quad \text{on} \quad -h(x,y) < z < 0 \ , \quad 0 < y < 1, \quad 0 < x \ ,$$

together with the boundary conditions

$$\phi_z + \nabla h . \nabla \phi = 0 \qquad \text{on } z = -h$$

$$\phi_{tt} + \phi_z = 0 \qquad \text{on } z = 0$$

& a boundedness or radiation condition at infinity.

For $h = x \tan \varepsilon$, it was shown by Stokes that the function

$$\phi = \sin \omega t \cos \pi y \ \exp\{ z \sin \varepsilon - x \cos \varepsilon \}$$

satisfies the above problem with finite kinetic energy. In 1951 Ursell showed that other discrete edge modes were possible and, being eigenmodes, are potentially resonant modes. Ursell made some experiments to show that, at frequencies corresponding to these modes, the response in the channel was abnormally large.

In order to get a feel for how these ideas might be extended to different kinds of beaches, let us consider the depth-averaged shallow-water equations for the vertical displacement, ζ, of the free surface. This is given by

$$(\nabla^2 - h^{-1}\,\partial^2/\partial t^2)\,\zeta \;+\; h^{-1}(\nabla h)\cdot(\nabla\zeta) \;=\; 0 . \qquad (1)$$

This is basically a wave equation with propagation speed $\sqrt{h}$, so there is a tendency for wave crests to be refracted towards shallow water. To gain an idea of the qualitative features possible with different kinds of seabed structures, we look at some particular examples.

(i) Suppose that $h = h(x)$, & consider waves of the form

$$\zeta(x,y,t) = A(x)\exp\{imy - i\omega t\},$$

where m, ω are constants. We find that

$$\frac{d}{dx}\left(h\frac{dA}{dx}\right) + (\omega^2 - m^2 h)\,A = 0 .$$

oscillator $F'' + qF = 0$, & the nature of the solutions depends crucially on the sign of $q = \omega^2 - m^2 h$. For $q \geq \varepsilon > 0$ we can expect oscillatory solutions and for $q \leq -\varepsilon < 0$ we can expect decaying solutions. The transition occurs at the 'caustic' at $x = x_c$, defined by $q(x_c) = \omega^2 - m^2 h(x_c) = 0$.

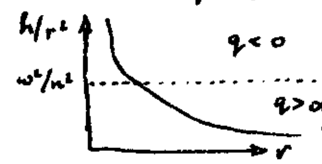
(ii) Axisymmetric topography.

Consider modes of the form $\zeta(r,\theta) = P(r)\exp\{in\theta - i\omega t\}$, where n, ω are constants. From (1) we have that

$$\frac{d}{dr}\left(rh\frac{dP}{dr}\right) + \left(\omega^2 - \left(\frac{n}{r}\right)^2 h\right)P = 0,$$

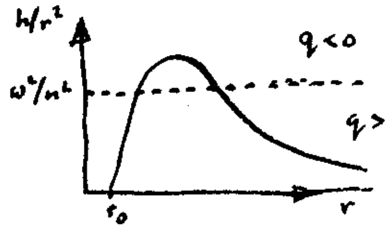
and a similar situation to that described in (i) can arise.

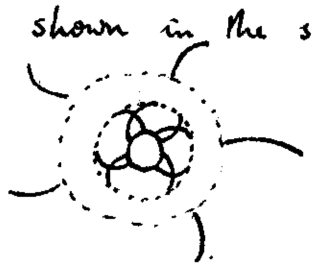
(a) Consider a sea mount (); we can expect a variation of h/r^2 as shown in the sketch, and for the given value of ω^2/n^2 we can expect oscillatory behaviour at large radii, & damped behaviour at small radii.



(b) An island with a shore :

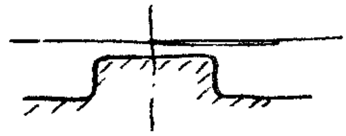
In this case $h/r_0^2 = 0$, & so the function h/r_0^2 will be of the form shown in the sketch



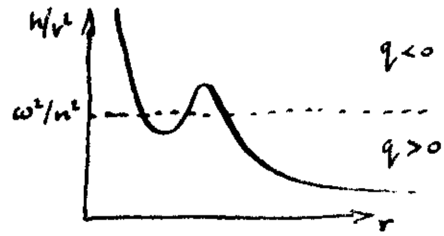


For the value of ω^2/n^2 indicated in the sketch there are two caustics indicating that oscillatory behaviour should ...

Between these two regions there is a zone in which we can expect decaying solutions. Thus, there is the possibility of wave trapping near the shore, & this zone is linked to the outer oscillatory zone by a ring of damped waves.



(c) A submerged reef, of the kind shown in the sketch. For sufficiently steep edges to the reef it is possible for the function h/r^2 to take the form indicated, & the structure should be a slightly more complicated version of that arising in (b).



These ideas apply only to very special wave modes, & therefore provide too narrow a basis to support the conclusions properly, but more general analyses suggest that they are sound. (See Longuet-Higgins 1967, Shen et. al. 1968.). Indeed, these analyses indicate that the inner region of trapped waves are coupled to the outer wave-like region through the damping annulus, and it follows that the inner trapped wave can 'leak' energy to the outer zone implying that these are 'leaky' modes in contrast to the Stokes edge wave, for example. On the other hand, this coupling implies that the trapped mode can be excited by waves incident on the island from infinity, & for conditions under which the leakage rate is small it is possible that such modes could have very

This is the case for (c), & a recent analysis by Lozano & Meyer (1976) has shown that the leakage rates can indeed be extremely small for (b), at certain frequencies.

These ideas were developed as a result of some oceanographic observations off the southern coast of Australia, & they have led to the very interesting proposition that 'nearly resonant' modes can exist in an infinite ocean & that these modes can be excited by waves incident from infinity. However, there are a number of approximations involved in the development of these models & it would be nice to see if such modes could be observed in the laboratory.

The reason for introducing this topic is to ask how, supposing one had made an experiment, we would decide whether or not the above phenomena are likely to be observed in practice? For the submerged reef (case c) it is possible to calculate the wave patterns over the reef, but for the island (case b) this is much more difficult, & in reality there is only an approximate estimate of the most dangerous frequencies. So the question -

we are close to a resonant or nearly resonant mode.

In making the experiments I decided that the best course to follow was to measure some functional of the wave field, such as the supremum of the field, or the energy integral of the waves over some suitable domain (the energy integral over the entire field is not bounded). For case (c), the submerged reef, the theory indicates that these two functionals have large peaks near the dangerous frequencies, & hopefully a similar property would prevail in the case (b) of the island. [Incidentally, the experimental results shown no real evidence of these 'peaks'; I'm fairly confident the trouble lies with the bottom friction which we saw playing a profound role in the experiments in § 2.2.]

The reason for talking about this work was to try to indicate how very important it is to try to present one's results in a form in which they are useful to the people you hope will be interested by them, especially if numerical computations should, at some stage, be involved).