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## A Long Term Study of Soil Erosion

Introduction:

Coon Creek in Northern Wisconsin drains about 360 square kilometers of rolling hills and bottomlands and empties into the Mississippi River near LaCrosse, Wisconsin. In the 1930s during the Depression and Dust Bowl, the Soil Conservation Service chose the Coon Creek basin for an intensive study of soil erosion that involved surveys throughout the basin, land use studies, and aerial photography.

In the 1970s and again in the 1990s, Stanley Trimble and others returned to the Coon Creek basin to measure how the land had changed in the intervening years. In many cases soils had moved from upland farms to become sediments in stream beds, stream channels had widened and moved, terraces had become swamps, swamps had grown up into forest. By repeating and extending the 1930s surveys they were able to estimate the amounts and rates of soil erosion that had occurred in the basin during the previous half century.

To extend their study further back in time, they dug down to markers of the first European settlements such as mill dams, house foundations, and old road beds; digging deeper, they located the original prairie soils dating to the 1850s when European farmers first settled the area.

Although farming started in the Coon Creek basin in the mid 19th century, soil erosion was not serious until around the turn of the century. After 1900 soil erosion increased dramatically, peaking in the Dust Bowl of the 1930s. Soil erosion decreased as farmers adapted soil conservation practices. Soil conservation techniques have continued to improve and recent rates of soil erosion in the basin are about 6 percent of what they were during the 1930s.

Trimble's paper has generated controversy within the academic community, which is a sign of an important contribution. Professor Trimble assured a warm reception when he concluded his paper with a mild challenge to the conventional wisdom: "The processes occurring on Coon Creek are indicative of many agriculturally disturbed basins in the U.S. and elsewhere."

How far these results can be applied to other watersheds is a subject of debate, but the extraordinary depth and breadth of this work should provide a benchmark by which other soil studies can be measured.

**ER:** Professor Trimble, what is your training?

**ST:** As an undergraduate I had a chemistry major with minors in physics and math. I had intended to be an engineer, but the school I attended did not have an engineering program so I just finished in chemistry. In the ensuing years of military service and travel I realized that I was interested in landscapes, especially in the human element of the landscape. I'd been living in Europe so I came back to this country and did another bachelor's degree, this time in geography. Then I enrolled in graduate school at the University of Georgia, where I did a Ph.D. in essentially human geography, interested particularly in human settlement and population patterns. It was during graduate school that I realized that I was more interested in how humans had changed the physical landscape.

**ER:** Almost an archeology kind of approach?

**ST:** Well, more of a demographic approach you might say: looking at for

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example, how people settle the land, how they take up agriculture, how a landscape develops over time.

I had done a master's thesis looking at soil erosion in the Southern Piedmont, so when the time came to do a dissertation, I went back to that. I had to pick up a lot of agronomy, geology, and hydrology along the way. That dissertation was published by the Soil Conservation Society of America, and it elicited quite a lot of attention.

**ER:** What is the southern Piedmont?

**ST:** The Piedmont is a foothills region that extends all the way from Alabama to Virginia. It's a huge chunk of land, big as most states.

After graduate school I accepted a position at the University of Wisconsin at Milwaukee. I had done my graduate work in Georgia, and so there I was in the upper Midwest, and I said, Well I'm here, I might as well look for projects here.

Through a friend — Stafford Happ, a retired geologist — I found out about a series of stream projects that were conducted back in the 1930s, and one of those was on Coon Creek. I found out that Happ and others had done a lot of work in this general region, the upper Mississippi River hill country, more generally called the driftless area, it's the unglaciated part of the upper Midwest.

So in 1973 I reconnoitered the area and was taken with first of all, the possibilities of a study because based on my earlier work I could see that there had been tremendous changes in the landscape. I could find buried bridges and other indicators in the landscape that showed me what massive erosion had occurred there.

So I went back to Stafford Happ and got whatever information he had at hand. But he said, Well look, there's a lot more of this information in the National Archives. He had seen to it that a lot of that old information had been retrieved and put in various repositories.

So in 1973 I wrote a proposal to the U.S. Geological Survey and brought Happ into the grant; they funded us and we restarted the Coon Creek project in June of 1974. So for the next five years I spent usually three months of every summer working up there twelve hour days, seven days a week.

**ER:** What sort of information was in the archives?

**ST:** The basic data consisted primarily of, first of all, borings across the floodplains. This had been done by an M.A. student at the University of Wisconsin by the name of Vincent McKelvey, later head of the U.S. Geological Survey. Working under the direction of Happ he would bore down through the modern sediment, which was highly stratified and generally brown in color. When he hit the original soil; that is, the original prairie soil that was there at the time of European settlement, it was usually dark and more homogenous with much more organic material in it. That contact was usually quite clear. So he did all these borings and recorded it all.

Then right behind him came a surveying team. The surveyors according to Happ, were the very best they could find. Remember, this was the Depression and many very good professionals were out of work. So the government could just cruise the Ivy League schools — Happ, for example, was a Ph.D. out of Columbia — and get the best people because the government could offer living wages. So they had this crack survey crew. The chief surveyor was a man named Witzgall and he apparently had eyes like a hawk. He could survey and close survey lines like no one I have

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ever seen. I've looked at his notes, and the guy was just beyond belief, particularly when you consider the primitive equipment that he had to use compared to what we have now. He resurveyed all of that area. At that point they had this wonderful data of the borings and they had the survey profiles on top of that. In many cases they put monuments in to show where the profiles were installed, steel pipes set in concrete on either side of the basin.

They had two more crews working, a hydrologic crew that established two stream sediment measuring gauges in Coon Creek to measure the rainfall and to measure sediment coming down the stream. Unfortunately that operation only lasted six years. The Second World War came up and the work they were doing here just obviously had to be dropped.

Before they stopped they also did a lot of documentation in the region. They had a crew that came through, including some of the people like Happ and McKelvey, and they made a tremendous number of photographs. They also did detailed land use studies, which I have. I also have aerial photographs made in 1934. I have a collection of time lapse photographs of that whole region showing how the landscape has changed, particularly since the 1930s.

Some of these government people would knock on doors and ask people if they had photographs going back earlier. I have photographs — there are a couple of them in the USGS paper — that go back to the turn of the century. So you can see landscape features in good condition at the turn of the century; you can see them in horrible condition in the 1930s; and then my photographs of the last thirty or so years show them in greatly improved states.

**ER:** How would you characterize the information from the borings and the surveys and the hydrology and the pictures?

**ST:** That information has given us time lapse snapshots of the riparian landscape. *[Riparian means streamside. Ed.]* Thirty years ago we were just beginning to understand how complex the movement of sediment was, that it can remain in storage for long periods of time and then feed out again. This study has allowed us to look at that process in detail.

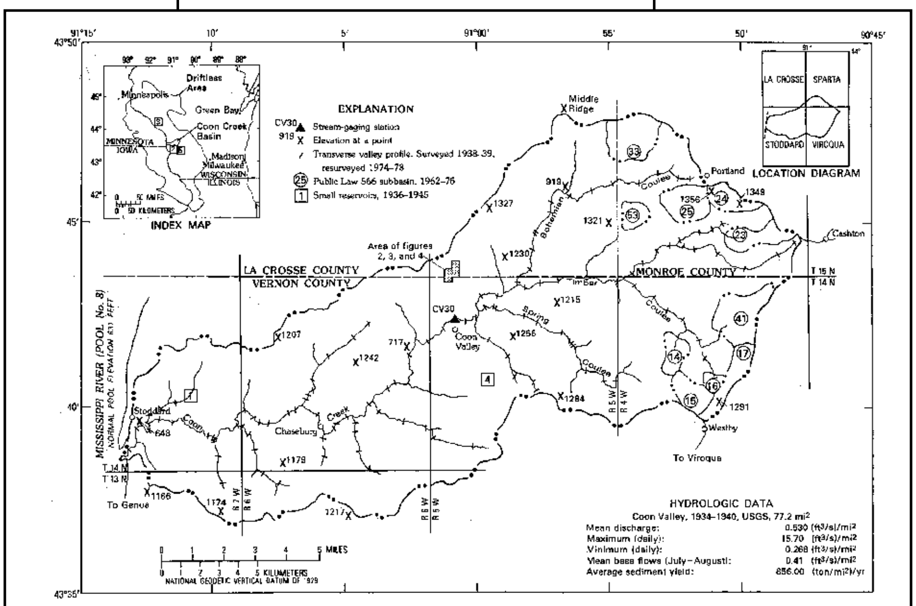
**ER:** What happens to a soil particle that is washed off a field?

**ST:** Say a particle of soil has eroded off an upland field. Conventional wisdom, at least among many, was that when it erodes it's on its way to the Gulf of Mexico. But that particle of soil can be deposited anywhere from where it began. It may be stopped anywhere between the field and the stream, and even if it makes it to the stream it can then be redeposited, either in the channel or on the floodplain anywhere down the system.

So that particle of soil can literally take millions of years to get to the Gulf of Mexico. That's not to say that every particle of soil takes that long. It's conceivable that a particle of soil eroded does go to the Gulf of Mexico by the direct route, but that is a very minor part. Most sediment is going to be redeposited and is going to stay in storage for anywhere from moments to millennia.

**ER:** What did your fieldwork involve?

**ST:** First of all, most of those old profiles were hard to find. Very few in




Map of Coon Creek basin, Wisconsin, showing research locations. More than one hundred transverse valley profiles were surveyed (indicated by lines across streams) in the 1930s and again in the 1970s and 1990s.

fact had the permanent markers that I mentioned, the pipes, and of those that had them, many of the pipes had been destroyed. They were put alongside roads for the most part and, of course, with any road widening they would be destroyed, or a farmer would find one in his field twenty years later, and he would take his tractor and jerk it out. You don't know how many times that happened. So those pipes tended to disappear. Other markers were nails in trees. The profile was usually drawn on an aerial photograph, so that helped, plus, of course, I had descriptions of the location from the survey notes.

The first big job was finding the profiles. That's where Happ was so helpful because he could remember some of these things, and in other cases where he couldn't remember, he was such a consummate field person that he could usually find them from the old field notes. He had an old army mine detector and he'd sniff around these trees trying to hunt for nails. Sometimes it'd be a 10-penny nail which by that time would be several inches inside the tree. This man was so patient, if he thought there would be a nail in the tree, he would sit there with this little hatchet and he would chip away literally for hours. There have been days where I said, We've got to get on with this, and so we'd do something else and we'd come back, it might be the next day, and there would be a big pile of chips and a gap cut out of the tree and right in the middle of it would be this rusty nail, but it'd be shiny where he had hit it with the ax. I don't know how many times that happened.

But in many cases we just could not find many of our marks. Thirty years later directions changed because the change of magnetic north, and many of the old compasses were off. So in some cases we had to run trial survey lines. In other words, we had to keep doing surveys until we found that we were where our unaffected topography matched the old topography. But surveying those was really difficult because many of these survey lines were through almost impassible swamps. These swamps were old floodplains now permanently flooded because the stream had built up from all the sediment in it. Going across them was like... well, let me give the best example. The first year I went to the field I had an older student just back from the Vietnam War, a combat veteran. He had spent two years over there and he told me after the first week, I'm going to say it far less colorfully than he did, but he said, I didn't see terrain this bad in Vietnam.

**Thirty years ago we were just beginning to understand how complex the movement of sediment was, that it can remain in storage for long periods of time and then feed out again.**

The mosquitoes were terrible and we were always afraid of encephalitis, but the chiggers got him one day and he wound up going to the emergency room. He was one of several students that had to go to the emergency room from insect bites. Deer flies were another problem. they didn't bother me as much because I took antihistamines anyway, but if people were susceptible they swelled like a balloon.

One day it was 112 degrees out there. Down in these swamps we're wet up to our waist in water, the rest of us is wet with sweat, and we've got mosquitoes all over and around us. I don't mean to try to portray heroics here because there were some easy profiles and some cool, nice days, but some days and particularly those long ranges across the wider bottoms, were just hell on Earth. I'd come home in the afternoon and I'd be covered not just with mud, but with this old anaerobic muck out of those swamps with sulphur dioxide and all that stuff. Normally I'd come in and my wife would hose me off out in the driveway. It'd be dark anyway, so I'd just drop all my clothes outside, which she would then further hose off, and then go inside wrapped in a towel and shower.

I normally tried to take the worst of it, but unfortunately students often got it too. It's unavoidable because we had to be out there with the instruments and generally there would be three of us doing this. That was just running the survey lines.

Remember, in the 1930s many of these profiles were run across quite open fields. In fact, some of them were even pastures at that time, so surveying those lines in 1938, '39, '40 was not so big a problem. When we came back they were not only grown up into swamps, but some have grown up into woodlands where you can't see ten feet ahead of you. It's a real lesson in plant succession. So for 800, 1000, and in one case 2500 feet, we had to cut a clear line of sight. Try cutting a half a mile through woodland with brush axes. Just cutting the line can take two days, and then you may find out you've got the wrong line. So for some of those older profiles we spent two weeks. The time required is what some people couldn't understand. They would say, You surveyed 150 profiles and it took five years?

**ER:** How do you measure a profile?

**ST:** You have an optical instrument which is sitting perfectly level. That's your level line. So you look along it and someone walks along a line with a vertical rod demarked to 0.01 feet. You start from one side of the stream channel or valley, and as you go out a number of feet, you place this measuring rod down on the ground. The person reading the instrument knows how high the level is from earlier marks, so the reading on the level rod shows how much lower that point of ground is than the level. From that you can get the elevation of the ground at that point.

In some of those profiles we had hundreds of measurements; every time we set the rod down we had a measurement. If it's relatively featureless topography like a plain, we might take readings at every fifty feet; but if we're in a complex valley or stream channel we might take readings at fractions of feet.

**ER:** First you had to find the right line.

**ST:** Right. Once we have found the line and cleared a line of sight, we then resurvey along that marked line. We hope to be directly on top of where they were fifty or sixty years ago. The new survey shows what has changed in that time.

At the downstream end of the basin it would usually show

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accretional material. We'd be standing anywhere from one to three feet higher than they were, maybe in some cases more than that. In some of the tributaries and middle valley, the stream channels may be two or three times as large as they used to be; in other words, channel erosion had taken place. In that case the stream is removing soil. Usually it's a mixture, in some places the stream has added material, in other places it's lost material. This survey gives us an insight as to process. By looking at these cross sections of the landscape over time, we can deduce the processes that occur.

**ER:** What do you mean by equilibrium in reference to soil erosion?

**ST:** Well, the general concept, even in the science up to the early or mid seventies was that stream sediment yields were indicators of upland erosion. The 50-cent word here is denudation, the stripping away of the land; lowering of the land is another term that's sometimes used.

Several of us showed in the mid seventies that was not necessarily the case: sediment could either be stored over long periods, thus reducing the amount of material going out, or conversely that material can come out of storage, increasing downstream sediment yield.

**ER:** How does soil come out of storage?

**ST:** Normally it comes out from channel erosion. That can either be vertical cutting or horizontal cutting or a combination of both. We find mostly horizontal cutting: a stream is meandering across the floodplain cutting away material from one bank but it's not putting as much back on the other side. So over a period of time there can be a significant loss of sediment along the stream channel.

Another thing we didn't realize until fairly recently is how many places within a stream system that this could occur. Within the stream system there can be reaches that are losing sediment, there can be reaches that are gaining sediment, and figuring out the balance can be very complex. There is probably some sort of system to it, but we don't know fully what that system is. We won't know until we see these sorts of things described more fully, and the only way to describe them is to get into the stream and measure them for a long period of time.

**ER:** The map of the site indicates you have a large sample.

**ST:** It is. And since then I've added more profiles because I knew that there would be attrition of these features over the following years. So I added quite a few, particularly in areas where I thought the stream was relatively dynamic. For the middle region of the basin, in particular, I added a lot of measurements because there's so much variance there that I knew we needed a larger sample. I added at least fifty new profiles and sure enough when I went back I couldn't find many of the profiles. That happens for many reasons.

**ER:** How would you characterize the results of your survey?

**ST:** Well, there is an inexorable movement of sediment downstream, of course, and it's clear that it moves in fits and starts. Eventually the stuff's going to work its way downstream and perhaps out of the basin, but that may take, as I said earlier, millennia. In other words there were huge amounts of sediment produced by European-style agriculture and if you came back 1,000 years from now, maybe 2,000, there would still be a lot of that sediment in that basin.

**ER:** How does that compare with the natural condition?

**ST:** Well, certainly streams have undergone changes of this magnitude under natural conditions when there have been strong climatic changes. The presence of stream terraces there tells you that. A stream terrace is an old floodplain, a relict floodplain, and any time you see a terrace above the present stream you know that the stream system was highly disturbed at one time. So yes, these sorts of changes can occur without human intervention during strong environmental changes.

**ER:** Fire, earthquake, flood.

**ST:** Well, generally a fire won't do it, or even prolonged fire won't in most areas. It has to be really a strong climatic change of some kind. Fire of course makes a huge difference in some California basins, but it would never be of that magnitude here. Yes, tectonic changes can set these things off too, you bet.

**ER:** But normally Coon Creek wouldn't have all this sediment moving around.

**There is an inexorable movement of sediment downstream, and it's clear it moves in fits and starts.**

**ST:** That's right. Generally in this region streams at the time of European settlement were quite stable. There are any number of observations of people standing by streams and being able to count the number of trout — brook trout by the way — and being able to describe bottom formations. I have the account written by a young Lieutenant in the Corps of Engineers named Robert E. Lee, who was coming up the Mississippi on a steamboat and he could observe formations on the bottom of the river from the steamboat. If you know the Mississippi River anywhere now, to imagine that it seems beyond belief, but the upper Mississippi was fairly clear apparently. At least the tributaries feeding it were clear.

**ER:** I've seen it on the lower reaches and it's anything but clear.

**ST:** Yes. It's anything but clear now almost anywhere. But again, I'm just trying to tell you what it was like then at the time of European settlement.

**ER:** When did serious erosion start in your study area?

**ST:** The settlers moved into the area and by roughly the turn of the century they had all the land taken up. By 1900 the extent of agriculture in the drainage was as great as it's ever been, but very little erosion had happened. The land had enough natural resilience to buffer many changes. The soil was originally quite fertile and deep, it had a high infiltration capacity and a lot of organic material. So these people came in and cultivated the soil and very little happened. But after some period of time you reach the end of the rope, and that time happened sometime after the turn of the century. By that time, number one, most of the land was in agriculture and much of it had been cultivated for forty or fifty years. So those great soils with all that organic material had begun to deteriorate from prolonged use. Additionally, they had a lot of cattle on the uplands that were compacting the soils, again reducing the capacity of the soil to infiltrate water.

So around the turn of the century the rates of erosion and the rates of water flowing off the land started reaching noticeable proportions. It was a threshold situation where you push a system to a point and it breaks suddenly. That period happened shortly after the turn of the century.

Aerial photographs of a selected region of Coon Creek basin. Picture on right from 1934 shows rectangular fields and gullies across fields from overland flow of water. Picture on left of the same area in 1967 planted in contour strips to reduce soil eroision. **Images courtesy of S. Trimble**

Generally it was identified with the First World War, but that in part is a coincidence, although during the First World War farmers pushed the land as hard as they could.

A Mr. Martiny had been living in Chaseburg, Wisconsin since the place was settled. In 1903 he felt confident in building a house on a terrace just a few feet above the creek.

Well, in 1907 the biggest flood on record came through and it was three feet to four feet above the foundation of the house. They were shocked by this. I talked to his son — when I first arrived in the area many of these old timers were still around — and they were shocked by this flood.

So they jacked the house up. They said, Surely it'll never happen again, so we'll just jack the house up four feet, put more rock under it, and we must be above any flood that's ever going to happen.

Well, you look at the foundation now and it's totally buried. There's no sign of it, except a water pipe that sticks out. Mr Martiny's son, who was then mayor of Chaseburg (1976-77), took us down to the site, showed us the water pipe, and he said, dig here. Sure enough, a couple days later we get down to the rock. We hit rock at about four feet but we kept digging and then we could see the old soil on which the house was built.

So as late as 1903 Mr. Martiny felt perfectly confident in building his house there. That house had to be abandoned in the early 1920s; and by the time we came there in 1977 the place was buried four feet down. I mention all that just to show you how fast this happened. Once the soil erosion started it was just gangbusters. They didn't know what was happening to them.

Another example on the Whitewater River is the village of Beaver, Minnesota. These settlers had come from New England as a group and had built a beautiful little New England village complete with a commons out there; and the same thing happened to them. About 1910-1915 floods started increasing, and they were in a more vulnerable position on a somewhat lower terrace. I have a picture showing Beaver in 1880 in which you see the village with the commons and the churches. I have another picture made in 1938, and there's one house left. So it went from a flourishing village in 1890 to one remaining house in 1938, and then by 1975 when I first visited the place, it was just another wilderness. This place had gone from wilderness to wilderness in 125 years.

**ER:** How long does it take for soils to stabilize?

**ST:** Soil recovery doesn't happen immediately either, there is a lag period. You may treat a particular field and do everything you can to it and you would greatly reduce erosion in the short run. You wouldn't eliminate it but you could certainly reduce it. Over time the effectiveness of continued soil conservation measures become greater, and even more importantly, the movement of water off that field is improved. The first year the condition of the soil is essentially what it was before, so it's going to take time for that soil to improve. It's got to get more organic material, it's got to improve structure, and that may take a decade or decades.

**ER:** Was the Dust Bowl era the low point for this watershed?

**ST:** Yes. Much of American agriculture tended to be fairly exploitive, and I don't want to make too much of that because people in this case didn't mean to exploit it. There were farmers particularly in the Southeast, that used land like they would a wagon: you use it, wear it out and then buy a new one.

But these people, Germans and Scandinavians, were intelligent, educated people who generally had a good land ethic. They had never or rarely owned land in the Old Country but they came here with the idea that land ownership was a big deal, and they certainly did not intend to destroy this land. They did though, because they simply did not have the technology to deal with it. And the technology had not been developed in part because there had never been a demand for it, again because of cheap land. You use it up and move on. The term "inexhaustible soil" appeared in that era.

**Running survey lines in the lower reaches of Coon Creek, August 1992.**

These Germans and Scandinavians came over here and used the best methods they knew. In the Old Country, particularly in Sweden but also in Germany, those methods had worked just fine because the rainfall intensities and amounts in Europe are so much less. What they could have done in the Old Country with impunity for centuries, wrought disaster within half a century in this environment. Their techniques simply were not adequate to the situation.

These were people who for the most part were doing the very best they could. Now I'm sure in the minds of many of them or some of them maybe was the idea, Well, if it folds up here we can move over to Nebraska or other points west. But I don't think many people saw it that way. They had their communities and they had no intention for the most part, of selling and moving out, as was often the case farther south and in the east. I say that because my earlier work was in areas where people pretty much destroyed the soil and then when they could, moved.

**ER:** When did soil conservation get started in this country?

**ST:** We generally had not addressed soil erosion in this country in any organized way before the 1930s. At that time it was being addressed by the soil conservation movement in this country which was very strong. It was headed by H. H. Bennett who later was head of the Soil Conservation Service. Bennett was a North Carolina farm boy who became a soil scientist, and he was an evangelist for soil conservation. He went around the country making speeches spreading the word. To him and his ilk we owe a lot because they in fact turned things around in the 1930s including the Dust Bowl. I don't know if you've seen pictures of it but in the 1930s there were clouds not once but many times that covered square miles. Livestock would be asphyxiated; even people on occasion. These dust clouds were carried to the East Coast where they would literally darken the sky.

**ER:** What did the farmers do to hold back the soil?

**ST:** Well the big things they did then were practices like contour plowing, contour strip cropping, and greatly improved rotations. The old European three-year rotation was one year of corn, one year of oats, and one year of hay, then you're back to corn. But in the Upper Midwest you actually need about three years of hay, and if you do that you can make the soil more or less recover. One year of hay isn't going to do that, and they didn't know that, but later on they did. There are a whole host of treatments to reduce erosion. That's what we might think of as first generation soil conservation techniques. Now we're into minimum till, no till, and so forth, which in a relative sense are as important as those earlier techniques. In other words, the improvement the new methods represent is almost as great as the improvements they got by going from the old cropping up and down hill to contour strip cropping and so forth.

**ER:** How do you generalize from these results in Coon Creek to other watersheds, different climates, different soils?

**ST:** With great care. But I'd say first of all that Coon Creek, and even the upper Mississippi River is not the only place I've seen. There is scattered evidence from throughout the East that the model of Coon Creek is not inappropriate. The chronology, the absolute numbers may be quite different, but the general trend is similar.

In the Southern Piedmont some similar things occurred. What was different there though is that much of that land simply reverted to forest. Rather than improvement of cropping, it was a great reduction of cropping and the lands reverted to forest or in some cases pasture, which was a far better land use. I don't mean to downplay soil conservation measures, they were important. But the change in the Coon Creek area was not change of land use, it's change of land treatment. They're still growing corn, they're still growing soybeans, they're still growing wheat and in almost the same acreage, or close to it. It's just that they do it far differently.

The only generalization I would make is that I think my work only addresses in general the humid eastern United States. However, I'm willing to go on record here saying that I think there has been great improvement in almost all regions of the United States. Not to say recovery, and probably not to the extent of the recovery that we see in Coon Creek. Soil erosion remains a problem, but I think it's not a crisis no matter how you define it.

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## Emerging Marine Diseases

Introduction:

In a recent article in *Science* magazine Professor Drew Harvell and other biologists present an overview of recent trends in marine ecology. Major groups of marine animals have suffered disease outbreaks and mass die offs[1]. Reports of epidemics in corals and some marine mammals have increased. A dramatic increase in coral bleaching around the world occurred in 1997-98 coinciding with increased sea surface temperatures due to that year's El Niño.

It is thought that environmental stresses may compromise animals' resistance to disease and increase the frequency of opportunistic diseases. Human-caused stresses include pollution and increasing sea surface temperatures as a result of global warming.

Disease outbreaks can occur when a disease causing organism changes its host. For example, the HIV retrovirus that causes AIDS in humans probably originated in apes, and flu often comes to us from birds. Disease can also move into new geographic regions either when the disease organism changes or the environment changes. Malaria for example is expected to move into more temperate regions in the future as global climate warms. These patterns of disease movement and host shifting also occur in the oceans, however marine scientists have only recently begun to study them systematically. We spoke with Professor Harvell about marine diseases and what we need to do to understand them better.

**ER:** Professor Harvell, what is your training?

**DH:** I've been a professor at Cornell University since 1986. Before that I received a Ph.D. from the University of Washington followed by postdoctoral training at Friday Harbor Labs in Washington and at Woods Hole Oceanographic Institute. My graduate work focused on the evolutionary ecology of marine invertebrate defenses against predators, working largely in temperate oceans.

After moving to Cornell I started tropical work with defenses of gorgonian soft corals. I became interested in this study system because soft corals are well known for having interesting chemical defenses. I wanted to tie together work that was being done with plants and animals. Plant ecologists were focussing on chemical defense and scientists working with inducible defenses of animals were focussing on morphological defense. Since corals are colonial animals and are like plants in several respects I thought this would be a good study system for evaluating the role of secondary chemicals in defense of colonial invertebrates.

For many years we worked on the predator deterrent aspects of secondary compounds, such as the chlorinated briaranes and the hydroquinones of several species of Caribbean gorgonian and that's the project that led me into the disease work. Through a collaboration with Bill Fenical and Paul Jensen at Scripps, and later Kiho Kim at Cornell, we surveyed the common Caribbean gorgonians for biological activity and detected significant anti-bacterial and anti-fungal activity in extracts of many of them. So to understand the puzzle of this diversity of interesting compounds, it became clear to me that we were going to have to understand the role of pathogens.

We started working on a fungal disease outbreak in sea fans. Most of the diseases of corals have not been identified; that is, we don't know the causative microbial organism. And the wonderful thing, at least from our perspective, about the sea fan pathogen is that it was a fungus that was identified by Garriet Smith and Dave Geisler, so we knew the causative agent. It could be cultured in the lab, and that gave us the ability to assay the chemistry of the sea fans against the living pathogen. As far as I know that's never been done for any other coral diseases.

**ER:** How often does a marine disease appear because it crosses to a new host?

> ...many of these epidemics are like lightning strikes: they hit quickly, unexpectedly, they run through a population and then they're gone.

**DH:** My sense is that we don't have enough data to evaluate that very well. We gave a couple of examples in the *Science* paper. One example is the fungus that's affecting our sea fan. We're studying a sea fan that's impacted by a fungal pathogen, *Aspergillis sydowii*. Prior to this epidemic in the sea fans, this fungus had never been reported as a pathogen in the ocean. However a closely related species, *Aspergillis fumigatus* has long been known as a human pathogen and indeed a killer of cancer patients.

The simplest hypothesis to explain the outbreak of disease in the sea fan corals is that the *Aspergillis sydowii* fungus was washed in from terrestrial soil where it's known to occur naturally and, for whatever reason, the environment changed or the genetics of sea fan changed and the sea fan became a suitable host. That's one possible example of a host shift, in this case changing from terrestrial to a marine environment.

One of the problems with this hypothesis of a new introduction though, is that we know *Aspergillis sydowii* has been in the ocean before. It's been recorded from marine sediments. So even with this case which seems to some clear cut, we're struggling to understand more precisely the origin of this outbreak.

**ER:** What is the extent of the disease?

**DH:** It's Caribbean wide. Ivan Nagelkerken , who's been working in Curaçao, first documented that sea fans throughout the Caribbean were affected by this epizootic. *[An epizoitic is an epidemic in animals. Ed]* Following that, Kiho Kim (then a postdoc) and I initiated monitoring studies in Florida to understand the epidemiology of the disease. So we're asking questions like, Were small individuals infected or large individuals? What's the probability of the sea fan dying once it's infected? Kiho is trying to correlate intensity of infection with environmental variables, such as turbidity of water, nitrogen level and chlorophyll A.

At least at the sites where we have permanent transects, the disease has had a significant impact on the populations. At some sites as many as 20 percent of the fans have died. There were probably similar impacts throughout the Caribbean although we don't have the data to know.

**ER:** Can you keep sea fans going, or at least samples of them going in the lab?

**DH:** Its possible to maintain them in the lab, but it is not our current focus. All the sea fan work we do is either in the field or in the lab directly adjacent to the field. To work with host resistance it's important to have healthy hosts. We don't want to be in a situation where our hosts are stressed physiologically, so we've developed methods of running experiments in the field so that we're sure we're not adding lab artifacts.

The fungus grows well in petri dishes, allowing us to work comfortably with one half of the association here in New York and then the other part of the work we conduct in the field.

**ER:** What about this idea of chemical defenses?

**DH:** There is a rich literature on natural products chemistry of marine invertebrates. In fact a large proportion of the so-called hits that the National Cancer Institute finds for potential anti-cancer drugs is from marine organisms. There's a growth industry there, just to understand the chemistry and to isolate biologically active compounds. Many of the compounds that the National Cancer Institute is examining are interesting because they disrupt mitosis or have other biological functions.

The novel chemicals chemists are finding in marine organisms also have some important ecological roles in nature, and it's the rich tradition of natural products chemistry that has driven the growth of chemical ecology. Many of these organisms have chemicals that slow or inhibit bacterial growth, slow or inhibit fungal growth, viral growth, as well as having effects on predators or competitors. So now the real task in my mind is to understand what role those compounds play in nature.

> Many of these marine organisms have chemicals that slow or inhibit bacterial or fungal or viral growth, as well as having effects on predators or competitors.

Some of the groups of animals that have been the prime study targets of the natural products chemists are organisms like soft corals or sponges, which are sessile, and they seem to have a high allocation to chemical defense in the same way that plants do.

**ER:** They don't have the option of running away.

**DH:** That's right. Which is a little bit of a tip-off that some of that activity in nature has an important defensive role. For example, with the gorgonians — they're a group of soft corals, and there about forty species in the Caribbean — we've assayed the antibacterial activity of these species in collaboration with Bill Fenical and Paul Jensen at Scripps, and found that a fair number of them have some kind of antibacterial activity. As an undergraduate honors thesis, Paul Kim did the same with antifungal activity on a subset of those gorgonians and we submitted the paper this week documenting that a fair number of them have some sort of antifungal chemistry.

**ER:** Are these novel agents?

**DH:** Yes, in some cases they likely are. Melissa Wagenaar working in Jon Clardy's lab at Cornell has identified some new antifungal components from our extracts. We haven't even started in on extracts from species other than seafans. So there certainly is biomedical potential for some of these chemicals.

**ER:** Do they attack the bugs in a different way than other drugs?

**DH:** That's the goal of course: to find not only new compounds as potential drugs but also new mechanisms of activity. In the case with the sea fans it would be useful if we found a novel antifungal agent against *Aspergillis sydowii* because it might have clinical relevance against the human pathgoen *Aspergillis fumigatus*.

The reality however, in terms of the clinical applications for these new compounds, is that many of them are too toxic to be used directly as drugs, but they can certainly yield important leads to biomedical researchers.

There is one compound from a Caribbean gorgonian, pseudopteresin, an anti-inflammatory compound discovered in Fenical's lab that's in clinical trials as an arthritis drug. In fact Estee Lauder working with Bill Fenical at Scripps developed an antiinflammatory skin care product that's based on the extract.

**ER:** How does environmental change influence diseases in the ocean?

**DH:** Certainly a focus on understanding how increased temperature affects disease is a priority. Alisa Alker as part of an honors thesis showed with in vitro culture experiments that the optimal temperature for the *Aspergillis* pathogen is at 30 degrees Celsius, near the peak of summertime temperatures in the Keys. Thus we would predict that warming temperatures would give the pathogen a growth advantage. This kind of experiment is only possible for pathogens that we can maintain in culture. Another challenge is simply to describe environmental change. I just spent the last hour in the Geology Department here talking with Bruce Monger and Jen Whiteis about their remote sensing project to try to work out a way of describing temperature changes that have occurred in the Caribbean over the last ten years to provide the environmental backdrop for assessing apparent increases in disease and bleaching.

What is striking is the lack of disease monitoring data to go with this environmental information. We have ways of getting at the environmental data, and now it seems to me an enormous priority to put some disease and coral bleaching monitoring programs in place. I think they are forming, but hardly fast enough to deal with this issue.

**There haven't been many cases where a new pathogen emerges that's never before been a pathogen. There are more cases where a pathogen has undergone a range shift or a host shift.**

**ER:** Isn't there a monitoring study in the Florida Keys?

**DH:** The Florida Keys is a different situation: there have been good coral monitoring programs there for the last ten years. Jim Porter, Esther Peters, Debby Santavy, Erich Mueller and others have been running a coral disease monitoring project, a collaboration between ecologists and microbiologists to assess in hundreds of sites throughout the Keys how the levels of coral disease are increasing and declining, waning and waxing.

The other thing that's innovative about work in the Florida Keys is that there's been good environmental monitoring in place for the last five to ten years also. The Southeast Regional Water Quality Program monitors temperature, chlorophyll-A, nutrients, all the relevant water chemistry parameters at hundreds of sites monthly throughout the Keys.

Our addition to this is we're working with sea fans trying to correlate changes in the prevalence of the disease - the percentage of individuals that have a disease, and the amount of damage from disease - with these various water quality parameters they are measuring. Kiho Kim has monitored seafans for three years at nine sites spanning the length of the Florida Keys, and is now attempting to correlate the disease and environmental data.

One strength of our program is that since we're only monitoring one species we are able to measure parameters in more detail: we know how sick individual sea fans are and how that varies with fan size and site, and most of the other monitoring programs measure only whether there's any disease present at a site and can't take data from individual corals, but they're monitoring diseases of fifty species.

It's a hard thing to come up with the good data to nail down correlations between a disease and changes in the environment. It seems an obvious hypothesis that disease outbreaks may be related to declining environmental conditions, but there are very few good data to show that association. The question that I'm asked over and over again is, Are there more new diseases now? And you can't answer that question without a good historical baseline, which at this point we don't have for marine diseases.

**ER:** It seems like common sense: if you see a lot of pollution and you see dead corals, there's a connection.

**DH:** Yes, but it's important to have the right kind of replication to be able to establish cause and effect. Or in the case of trying to detect a correlation between environmental parameters and the disease, again, there needs to be enough variation. You need sites where there's high levels of the pollutants and a lot of damage from the disease, and then a range to sites with different levels of disease and stressors because there are a lot of other factors — and realistically multiple stressors — that could contribute to the mortality associated with disease. To tease apart what is the causative agent of this requires good data sets that span large spatial scales and fairly long temporal scales, and that's missing in most cases.

It's not enough to say: this seems to be a polluted site and these corals are dead. You need somethig more like five polluted sites and five control sites to demonstrate that the corals died at all the polluted sites and they did not die at any of the control sites.

**ER:** You could use Koch's rules: give the bug to them and see if they get sick; take it away and see if they get better.

**DH:** There's been very little experimental work with disease in marine organisms. To fulfill Koch's postulate requires three steps: 1) isolating a micoorganism from diseased individuals, 2) establishing it in pure culture and 3) successfully reinoculating healthy hosts and producing disease symptoms. The failure oftentimes to fulfill Koch's postulate illustrates the problem with marine diseases because many of the pathogens aren't easily cultured. It's an enormous victory when even a single pathogen is identified and Koch's postulate is fulfilled, and there are only three or four species of coral diseases for which that's the case, balanced against a list of about fifteen symptoms which distinsguish other coral diseases: white pox, porites ulcerative white pox, white band, white plague, or yellow blotch, where the causative agent is not known and we're simply relying on the symptoms.

**ER:** It seems like the Florida Keys may give you the best information.

**DH:** The problem with the Keys for making simple conclusions is that it's a very complex ecosystem. There is a substantial anthropogenic influence there, but it's overlaying an already fairly stressed, variable ecosystem. Those coral reefs are near the northern extent of reef growth. They're experiencing some of the highest of summertime temperatures and some of the lowest of the wintertime temperatures for the Caribbean, and so it's difficult to pick out what are the causative agents in some of the declines we see from that area. For example, there's a lot of fresh water input and in many cases we know the quality of that water coming out of the Everglades and Florida Bay is not always good, but its challenging to pinpoint this rather than other environmental change as a causative agent of coral decline.

**These coral reefs (in the Caribbean) are near the northern extent of reef growth. They're experiencing some of the highest summertime temperatures and some of the lowest of the wintertime temperatures for the Caribbean, so it's difficult to pick out the causative agents in some of the declines.**

**ER:** Where are new diseases coming from?

**DH:** There haven't been many cases where a new pathogen emerges that's never before been a pathogen. It seems there are more cases where the pathogens that are found have undergone a range shift or a host shift: from one organism to another. One example of a host shift is that of the morbillivirus of seals and other marine mammals, which is interesting because it's virtually identical to the canine distemper virus.
Molecular studies suggest there is likely a terrestrial link with some strains of morbillivirus and scientists think the seal morbillivirus originated in a terrestrial canid. Among the better studied bivalve diseases we also see host shifting. In oysters and clams there's a whole group of *Perkinsis* that appear to have host shifted among various bivalve taxa. This host shifting might be a common motif of a new disease appearing and is an issue that urgently needs good quantitative data.

**ER:** One thing that we know from human epidemiology is that there seems to a minimum population size for the host species to keep the disease going. Is that an issue in marine systems?

**DH:** I think those are starting to be some of the ecologically and evolutionarily exciting questions that ecologists can ask about these systems. Certainly it is a vital issue for conservation biologists to understand what's a critical host size for maintenance of an epidemic.

There's been some beautiful work done by Bryan Grenfell and colleagues with the seal morbillivirus that suggests that the population size of the seals is such that the morbillivirus will eventually always die out. That's the only case I know of where there's been that kind of an epidemiological analysis in a marine organism. There may be examples with oyster too. But for most natural populations, in the first case, we don't know what the causative agents are, and so it's hard to study the epidemiology. In the second place, we don't know enough about resistance to understand what's a viable host and what isn't. And in the third case, there just hasn't been the kind of work done in marine systems to quantitatively study the dynamics of the epidemic. I hope this will change because those are critical questions.

The sea fan disease has been a fairly extended epidemic, and we've been able to study it for several years and actually develop a pretty good data base and a profile of how that epidemic has affected its population.

But many of these epidemics are like lightning strikes: they hit quickly, unexpectedly, they run through a population and then they're gone. We documented one last year in the Florida Keys at the height of the El Niño high temperatures in a gorgonian soft coral, *Briareum asbestinum:* over 60 percent of the individuals were killed within 3 months and then all symptoms of the survivors disappeared. You couldn't do an epidemiological analysis of something that was so virulent and that ephemeral.

**ER:** We may need an emergency response plan for ecosystems.

**DH:** We have talked about needing Centers for Disease Control for environmental disease and marine diseases certainly — a critical need is to identify the causative agents for new outbreaks. This requires the kind of rapid response capability and instrumentation that we associate with the CDC for human disease. Both the urchin disease that virtually eradicated the dominant urchin from the Caribbean and the frog viral and fungal diseases on land underscore the potential danger that emergent diseases can pose to biodiversity. It is important to have a rapid response capability so we can at least identify the causative micro-organisms as these outbreaks occur.

That's where I hope this interview will be useful: to help people understand that there's a real need for putting funds into understanding disease processes in natural ecosystems in the ocean. It's a whole different ball game in ocean systems than on land. We know very little about transmission biology of diseases in the ocean, and yet oceans are traditionally thought of as being open systems where water masses freely mix.

**One example of a host shift: the morbillivirus of seals is virtually identical to the canine distemper virus... scientists think the seal morbillivirus originated in a terrestrial canid (dog).**

In such an open system we would predict that disease propagules could get readily from one geographic region to another and spread very rapidly. And yet, despite the hypothesis that disease transmission might be much faster and greater in the ocean, there are no quantitative data to assess the hypothesis. Transmission of disease is very well understood in terrestrial ecosystems because of the economic importance of human and agricultural disease and concerns about endangered wildlife. Our understanding of the rates of disease transmission and the role of host resistance is far behind for marine organisms.

There are some classic cases like chestnut blight and Dutch elm disease that spread and caused enormous mortality over short periods and changed the face of terrestrial forested ecosystems within ten or twenty years. We don't have examples like that from the ocean, and I would guess that is because we've missed many of them, they've probably happened and were never recorded.

We saw the example of the *Diadema antillarum*, which is a sea urchin in the Caribbean that was virtually eradicated by a mid-eighties epidemic. That was a spectacular epidemic that was well recorded, and scientists were able to watch the urchins fall apart throughout an island within a space of several days, and then a week later it hit the next island. So there did seem to be a waterborne agent that spread Caribbean wide in that case. As far as I know, that's the only situation where there were any estimates made of transmission rates.

**ER:** These problems cross a lot a academic boundaries.

**DH:** That's why we wrote the *Science* paper, because evaluating the causes of recent increased reports of diseases in the ocean is a complex topic and it needed to be a whole paper to describe the magnitude of the problem, and that it wasn't just affecting corals, it wasn't just affecting seals, and not just oysters, but organisms in many regions were affected.

When we have processes that we don't understand causing major shifts in the composition of communities in the ocean, I think it's a cause of some concern. The impacts of disease for long-lived organisms could be far reaching. In the case of corals, some

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of these hard corals may be 700-year-old individuals and they are dying over a space of a month or two. I don't want to over inflate the urgency; as an ecologist I also know that disease has likely long been an important and natural agent structuring marine communities. Our problem today is separating natural and human facilitated impacts of diseases. Clearly scientists have agreed it's an important issue. That's why it was important to gather thirteen scientists from different disciplines together to write a consensus statement to identify which parts of this problem we agree on and that disease is having large negative impacts on biodiversity. The worrisome part is that we don't understand the ecological and evolutionary disease processes very well, and on top of that it's possible that the environment is changing through increased seasurface temperatures.

In my view part of the solution is to be sure that at least starting now we have an historical baseline for processes that are important in these marine communities and try to develop the right kind of monitoring studies so that twenty years from now we can answer the questions about whether the intensity of disease is changing.

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