e.g. fish yield parameters, the plan requires a water quality *checking* pathway. If hypotheses about water quality, pond dynamics or chemical/gas cycles and how these impact fish are more important, a water quality *research* pathway is followed (Fig.1).

Water Quality Checking

In water quality checking, *basal* and *routine* (weekly) parameters are monitored at stocking and harvesting fish. The two most important basal parameters for freshwater fish culture are alkalinity and hardness. These are not critical to fish health on short time scales and change slowly over most culture periods, unless regular liming is being conducted.

Routine water quality monitors (on a weekly basis) parameters that can change rapidly and can dramatically affect fish health and experimental treatments. Surface water temperatures (at 2-5 cm depth) and pH are measured weekly at 0500-0700 and 1400-1600 hours because of their central positions as primary indicators of whether toxic concentrations of ammonia or hydrogen sulfide occur. If the pH is out of the range for good fish growth (6-8), or conductivity, Secchi disk visibility (SDV), dissolved oxygen (DO), or observations exceed the limits shown in Fig. 1, further investigations are required. It is essential that all routine water quality measurements be conducted during the critical early-morning and late-afternoon (0500-0700 and 1400-1600 hours) periods.

When adverse pH's occur during a routine water quality checking program, testing for concentrations of un-ionized hydrogen sulfide (H_2S) or ammonia (NH_3) is also conducted. Fig. 1 details testing needs if: pH's are less than 6.0 (at 0500-0700 hours) or greater than 8.0 (at 1400-1600 hours); conductivities exceed 400 µmhos/cm; SDV falls below 10 cm; 0500-0700 hours DO falls below 1 mg/l; and/or morning observations show fish gulping at the water surface and a deep green water color.

Water Quality Research

DO and pH are the most critical parameters to measure on a regular basis in aquaculture, especially in experiments using high stocking densities close to the carrying capacity of the system, or with high feeding/loading rates of organic matter and during warm seasons.

Water quality *research* should include *routine* (daily) monitoring of DO, pH and other parameters at 0500-0700 and 1400-1600 hours (Fig. 1). Water temperatures are taken at the surface (2-5 cm depth) and at the pond bottom, in order to monitor pond mixing dynamics. *Full* water quality monitoring involves weekly measurements of inorganic nutrients important for primary and total microbial production to determine interactions among carbon and nutrient pathways.

Monitoring *special* water quality parameters every two weeks allows complete determination of organic and inorganic pathways. For example, nutrient and silica cycling, sulfur cycling, biological and chemical interactions and respiratory pathways can be examined.

Suggested Further Reading

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Is ANOVA Powerful Enough for Analyzing Replicated Pond Experiments?*

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Introduction

Aquaculture pond experiments, like agricultural crop trials, are often designed according to the statistical rules of replication and randomization: several treatments are applied to a number of experimental units (in this case: ponds) after which a certain characteristic (e.g., yield) is measured in every pond. Other factors with a possible effect on the measured characteristic are held at the same constant level as much as possible so as not to disturb treatment effects. Analysis of variance (ANOVA) is used to compare the treatments.

Other things than the treatments alone can cause differences between ponds. This 'experimental error' has to be estimated by assigning the same treatment to more than one pond: replication. Treatment effects are 'between group' differences whereas 'within group' differences are 'error'. If there is much more variation between groups than within, groups are obviously very different from each other and there may be a significant treatment effect.

Randomization (random assignment of treatments to ponds) is necessary because the observations and the errors must be independently distributed in order to test hypotheses. The null hypothesis (H_0): 'all treatment means are equal' is tested against the alternative (H_1): 'the means are not equal'.

H_0 can be true or false. The value α indicates the probability of rejecting the null hypothesis when H_0 is true. This mistake (rejecting H_0 although it is true) is called a Type I error. The value of α is usually set at 0.05 or even lower to ensure that making a Type I error is very unlikely.

When H_0 is false, the value β indicates the probability of not rejecting H_0, which would also be a mistake: this is called a Type II error. Interestingly, α-levels are

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always reported in hypothesis tests but β-levels hardly ever (Peterman 1990). Not being able to reject H_0 on the basis of the experimental data does not necessarily mean that H_0 has to be accepted as the only truth on the basis of these same data.

The value 1-β is called the 'power' of a test. Zar (1984) describes a method designed by Pearson and Hartley (1951) to calculate the power of an ANOVA and the required number of replicates.

Table 1. Experimental results.

Treatment	Replicate	Initial average weight (g)	Final average weight (g)	Average weight gain (g)
1	1	22.23	43.57	21.34
	2	20.43	30.67	10.24
	3	19.63	32.30	12.67
2	1	19.07	38.15	19.08
	2	20.93	46.17	25.24
	3	21.37	44.70	23.33
3	1	23.13	30.47	7.34
	2	21.80	32.97	11.17
	3	20.53	40.93	20.40

Table 2. Summary results of experiment.

Treatment	Mean average weight gain (g)	Percent difference between treatments in terms of overall mean weight gain
1	14.75	]] 46.5%
2	22.55	]] 57.2%] 10.6%
3	12.97	]]

overall mean weight gain = 16.76

Table 3. ANOVA results.

Source	df	SS	MS	F	F(2,6)
Mean	1	2,527.0729			
Between groups	2	155.7848	77.8924	2.624	7.26 (n.s.)
Within groups	6	178.1218	29.6870		

An Example of Power Analysis

The following are partial results from a growth experiment in earthen ponds at the Domasi Experimental Fish Farm (DEFF) in Domasi, Malawi (Chikafumbwa 1990). *Tilapia rendalli* were stocked in nine 200 m² ponds at 2 fish per m² and grown for 126 days under three treatments: (1) with 14 kg/pond/week of napier grass; (2) with 14 kg/pond/week napier grass and madeya (maize bran), fed at 3% of fish biomass per day, adjusted fortnightly; and (3) with madeya at 3% of fish biomass per day, adjusted fortnightly. There were three replicates of each treatment. Results are presented in Tables 1-3.

At first sight, treatment 2 gave the best result with about 50% increase in weight gain over treatments 1 and 3 (Table 2). This seems to be quite a large difference.

In the one-way ANOVA, the F-value for treatment effect was 2.624, less than the F(2,6)-value from the table (α=0.05). The null-hypothesis was therefore not rejected and despite their apparent size, we cannot prove that the differences between the treatments do exist. This is caused by large within-treatment variability (one pond each in treatments 1 and 3 yielded a result very close to the average of treatment 2).

To determine the power, a quantity φ (phi) is defined (Zar 1984):

$$\phi = \sqrt{\frac{(k-1)\ (\text{between groups MS})}{(k)\ (\text{within groups MS})}} \qquad (1)$$

with k = number of treatments. Using φ and a special graph set of reference curves from Zar (1984), the power (=1-β) can be determined (note that the 'within groups MS' is equal to the experimental variance s^2). With k=3,

$$\phi = \sqrt{\frac{(2)\ (77.8924)}{(3)\ (29.6870)}} = 1.32257$$

From the graph in Zar (1984), 1-β ≅ 0.30 (at α=0.05), therefore the power is 30%.

It is thus shown that, given this experimental setup and the experimental variance, there is a 70% chance of making a Type II error, or in other words, of not rejecting H_0 when it is false.

Another possibility is to specify the smallest difference δ (delta) that is to be detected. φ is then calculated as (Zar 1984):

$$\phi = \sqrt{\frac{n\delta^2}{(2k)\ (\text{within groups MS})}} \qquad (2)$$

with n = the number of replicates.

Suppose we wanted to detect a difference of 25% of the overall mean, i.e., of 4.19 g average weight gain. Then, following equation (2),

$$\phi = \sqrt{\frac{(3)\ (4.19^2)}{(2)\ (3)\ (29.687)}} = 0.5438$$

From the graph in Zar (1984), 1-β < 0.20 (at α=0.05), so the power for detecting a 25% difference is less than 20%. Reversing equation (2), the number of replications needed to detect a 25% difference with 80% power can be calculated:

$$n = \frac{2\ \phi^2\ k\ (\text{within groups MS})}{\delta^2} \qquad (3)$$

A power of 80% means that φ = 2.3, hence

$$n = \frac{(2)\ (2.3^2)\ (3)\ (29.687)}{4.19^2} \approx 54$$

So the required number of replicates would be 54 and (3) (54) = 162 ponds would be needed to do this experiment!

This method can also be applied to more complicated designs like a two-factor ANOVA (see Zar 1984, section 13.7), and also to other statistical tests like the t-test and others (Peterman 1990).

Implications for Pond Research

Replicated, randomized experiments try to eliminate disturbing factors as much as possible before the effect of treatments are measured. Unfortunately, controlling within-treatment variability in fishponds turns out to be very difficult. Pond experiments are different from crop experiments in that the foodchain in fishponds is much more complex. Indirect feeding or fertilization treatments affect the fish only after having passed through several trophic levels. There are thus lots of opportunities for disturbances and error generation, and ANOVA's can easily result in non-significant findings.

Power analysis can help in assessing the strength of our tests. It can be applied before conducting an experiment to calculate the sample size or the number of replicates needed for detecting a certain effect

size with a certain probability. It can also be applied afterwards, when the null hypothesis is not rejected, to find out how likely it was to be rejected (*a priori* and *a posteriori* power analysis, respectively, see Peterman 1990).

It seems that we are all very anxious to avoid concluding that there is a treatment effect when it is not true (avoid Type I errors) but we don't mind accepting a null hypothesis that is not true (making Type II errors). As the null hypothesis often represents the existing situation, this lack of 'power awareness' can easily lead to frustrated researchers, stagnating research and ineffective data analysis. Alternatively, we could look for techniques that utilize the variability rather than try to minimize it in a classical experimental design. The effect of treatments on all trophic levels in ponds could be measured and analyzed using multivariate statistical techniques. In this way, the variability would be reduced systematically, rather than assumed-to-be-absent-but-still-present, as it is in many experiments. Another possibility is to lump data from separate experiments together and analyze the resulting dataset as a whole. Although separate ANOVA's for individual experiments may not detect any treatment effects, multiple regression models can uncover many significant relationships between variables (van Dam 1990; Prein 1985).

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Africa Section

Farmers' Attitudes in Malawi to the Use of Excreta in Fish Farming

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Introduction

Fish farming is becoming increasingly popular among smallholder farmers in Central Malawi, particularly in Dedza and Lilongwe Districts, as a source not only of much needed high quality protein, but also of cash income. A collaborative project between the Malawi Department of Fisheries, the University of Malawi and ICLARM, financed by the German Agency for Technical Cooperation (GTZ), is seeking to encourage farmers to use locally available fishpond inputs. One possibility is to use livestock or human excreta. This depends, however, on the acceptability of the practice by farmers and the produce by consumers. These are influenced by attitudes, beliefs and risk perceptions.

To identify the main impediments to acceptability of fish raised in excreta-fed ponds by smallholder farmers and to suggest ways of encouraging excreta use in fish farming, a survey of 112 fish farmers and non-fish farmers was conducted in late 1988. The team also used published and unpublished documents and held discussions with Ministry of Agriculture (MOA) staff to supplement this information.

Fish farming is very new in Central Malawi. It was first introduced in Dedza Hills in 1986, and in Lilongwe Northeast in 1987. At the beginning of the study, there were 107 fish farmers in Dedza Hills and 16 in Lilongwe Northeast with a total of 134 ponds. However, only about 40% were stocked. Tilapias, locally called chambo (*Oreochromis shiranus chilwae* or *Tilapia rendalli*) are the main species stocked. Others are mlamba (*Clarias gariepinus*), ntchila (*Labeo mesops*) and mbamba (*Haplochromis* spp).

Students of the Bunda College of Agriculture, Malawi looking at a typical fish pond in Dedza Hills. Most of the farmers use livestock wastes in fish farming. (Photo by J.S. Likongwe)

Etudiants du Bunda College of Agriculture, Malawi, au bord d'un étang d'élevage caractéristique de la région de Dedza Hills. La plupart des éleveurs emploient des excréments d'animaux (Cliché: J.S. Likongwe)