

I define internal rate of return as "the aggregate rate of return on all proprietor's capital invested in a project at any point in its duration", and argue that there can only be one internal rate of return for a given project.

Dr Bilek offers by way of definition "... the rate of discount that equates the present value of the entire series of cash flows associated with the project to zero". (Mao, James C.T. Quantitative Analysis of Financial Decisions 1969. p.193).

Thus far the difference is minor, in that while I put forward a definition which is intuitive, Dr Bilek's defines IRR according to its method of determination. Either approach is legitimate. The irony, however, is that Dr Bilek sees his own definition of IRR as being handicapped by the perceived possibility of "multiple solutions to internal rate of return", when in fact that is not the case. "Multiple solutions" are an artifact resulting from failure to rigorously identify those items which are "internal" to, or "associated with" the project, by comparison with those which are external and not, in any strict sense, "associated with the project".

By way of example Dr Bilek gives the following payment series (cashflow) to his students:

Period	Flow
0	-100
1	2000
2	0
3	0
4	0
5	0
6	0
7	0
8	0
9	0
10	-2100

Dr Bilek argues that this series has two IRRs, 1.1% and 1900.0%, and that, by definition, both are correct, which would appear to prove the possibility of multiple IRRs and at the same time invalidate my intuitive definition.

But does it? I would suggest that "internal" is a key word in "Internal Rate of Return". In Dr Bilek's example, \$100 is invested in Year 0. The investment is terminated in year 1 by the \$2000 payment regardless of rate of return. The balance is then invested externally to meet the call for \$2100 in year 10.

The question then is "What rate of return can be attributed to the first investment (Year 0 to 1) such that the balance, after allowing for return to capital, when invested externally

at the same rate of return, equals the demand for capital in year 10?"

Both 1.1% and 1900% are correct answers to the question, but they are not internal rates of return, because they are rates that must be generated outside of the project. (I should point out that the simple case above involves the necessity of a \$100 investment in Year 0, producing a \$2000 return in Year 1, but compelling a further \$2100 payment in Year 10. Whether or not such a series of payments would obtain in reality is beside the point. The method of analysis should allow for all possibilities.)

I suggest that all other cases of "multiple internal rates of return" will likewise be found to involve at some point(s) the return of funds to the investor which will cancel out the initial investment and leave a surplus to be invested externally to the project.

The interpretation of the multiple mathematical solutions to such so-called "IRR" problems may be complex. However it is my view that IRR, properly considered, remains a simple concept, which may be defined intuitively, and which does not present us with the logical challenges of multiple solutions.

It would be beneficial for the profession to reach a common understanding on this point, because of the weight (rightly or wrongly) attributed to discounted cash flow analysis in forest investment analysis in New Zealand.

Geoff Fischer

Critique Stretching a Point?

Sir

I suspect that Kevin Smith is missing the point, and is being a wee bit precious, in his critique (November, 1998) of the article written by David Norton.

The New Zealand Forest Accord was a superb piece of nature conservation advocacy by the Royal Forest and Bird Protection Society (RFBPS) working with the plantation industry. The intent, as Smith points out, was to identify those indigenous areas that it would be inappropriate to clear for the establishment of plantations, and to avoid costly land disputes. This was to be commended.

Norton, however, suggests that we can and should move beyond the protection of sites (eg. habitat patches) to the management of whole landscapes, in order to protect indigenous biodiversity. This premise is supported by research which has demonstrated that a)

most indigenous habitat remnants are no longer driven by internal processes, because they are too small, rather they are driven by external processes from the surrounding matrix, and b) the spatial position of these remnants in the landscape has an effect on viability. How we manage the matrix determines the survival or otherwise of the indigenous component of the landscape.

Integrated management is about, amongst other things, considering the protection of indigenous biodiversity in all aspects of land management, not just in reserves. Relying on reserves to protect biodiversity in the 60-70% of New Zealand that is not managed for nature conservation purposes is destined for failure. Hence the call by Norton and others for the integration of nature conservation, based on scientific principles, within productive landscapes.

And the difference between the 'new paradigm' and the bogey of multiple use forestry, that Kevin Smith raises, is that the latter really only required provision for tourism and aesthetics (eg. idiot strips along roadsides) while clear-felling indigenous forests. The 'new paradigm' of integrated land management requires the protection of biodiversity and ecosystem functioning while providing for human use - and if pest or weed control, or the creation of habitat corridors is required, then this becomes part of the management. To my mind the difference is dramatic.

Yours sincerely

Craig Miller

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